CUK-H03468-17-P017831

American Sociological Review

17

A New Theory of Distributive Justice 3
Guillermina Jasso

Sexual Embourgeoisment? 33
Martin S. Weinberg and Colin J. Williams

Sudden Wealth, Gratification, and Attainment 49
Mark Abrahamson

Industry Marginality and Female Employment 58
William P. Bridges

The Flemish Movement in Belgium after World War II 76
François Nielsen

Reconciling Differences in Estimates of Delinquency 95
Delbert S. Elliott and Suzanne S. Ageton

The Geography of Soviet Criminality 111
Louise Shelley

Analyzing Collapsed Contingency Tables 123
Paul D. Allison

Comments 131
Robert W. Jackman, Glenn Firebaugh, Robert Bolin, and Susan Bolton Bolin

CALCUTTA UNIVERSITY CENTRAL LIBRARY

Volume 45 February 1980 Number 1

Vol. 45
No. 1-6
1980

301.05
Am 35

Notice to Contributors

uired for each paper submitted, except for comments; such fees will be waived for check or money order, made payable to the American Sociological Association, mission. The fee must be paid in order for the review process to begin.

r is generally ten (10) printed pages or thirty (30) typed pages. However, the *ASR* is ts up to fifty (50) typed pages in length for review. We also are interested in receiving ch and Notes Section.

the review of manuscripts, keep identifying material out of the manuscript. Attach a cover institutional affiliation and acknowledgments, and provide only the title as identification on stract.

ions Policy: Submission of a manuscript to a professional journal clearly implies commitment rnal. The competition for journal space requires a great deal of time and effort on the part of ose only compensation for this service is the opportunity to read papers prior to publication on associated with discharge of professional obligation. For these reasons, *the American ciation regards submission of a manuscript to a professional journal while that paper is under r journal as unacceptable.*

AND PREPARATION OF MANUSCRIPTS

(4) copies and retain the original for your files. Copies may be Xerox, mimeograph or multilith; but no s.

2. Enclose a stamped self-addressed postcard for acknowledgment of manuscript receipt. *Manuscripts will not be returned.* Please do not send your original copy.
3. All copy must be typed, doublespaced (including indented material, footnotes and references) on 8½ by 11 inch white opaque paper. Lines must not exceed six (6) inches. Margins must be a minimum of one inch.
4. Include four (4) copies of an abstract of no more than 150 words.
5. Type each table on a separate page. Insert a location note at the appropriate place in the text, e.g., "Table 2 about here." Send copies, retain originals.
6. Figures must be drawn in India ink on white paper. Send copies, retain originals.
7. Clarify all symbols with notes in the margins of the manuscript. Circle these and all other explanatory notes not intended for printing.
8. Footnotes should not be placed in the body of the text. Type them (doublespaced) and attach them as a separate appendix to the text. Number them consecutively throughout the text. Footnotes are to be kept to a minimum and used only for substantive observations. Source citations are made within the text rather than in footnotes.
9. Acknowledgments, credits and grant numbers are placed on the title page with an asterisk.

REFERENCE FORMAT

A. *In the text:* All source references are to be identified at the appropriate point in the text by the last name of the author, year of publication and pagination where needed. Identify subsequent citations of the same source in the same way as the first, not using *"ibid.," "op. cit.," or "loc. cit."* Examples follow:

1. If author's name is in the text, follow it with year in parentheses. [". . . Duncan (1959) . . ."]
2. If author's name is not in the text, insert, in parentheses, the last name and year, separated by a comma. [". . . (cf. Gouldner, 1963) . . ."]
3. Pagination (without "p." or "pp.") follows year of publication after a colon. [". . . Kuhn (1970:71)."]
4. Give both last names for dual authors; for more than two use "et al." in the text. When two authors have the same last name, use identifying initials in the text. For institutional authorship, supply minimum identification from the beginning of the complete citation. [". . . (U.S. Bureau of the Census, 1963:117) . . ."]
5. Separate a series of references with semicolons and enclose them within a single pair of parentheses. [". . . (Burgess, 1968; Marwell et al., 1971; Cohen, 1962) . . ."]

B. *In the appendix:* List all source citations by author, and within author by year of publication, in an appendix titled "References." The reference appendix must be complete and include all references in the text. The use of "et al." is not acceptable in the appendix; list the names of all authors. (See A. 4. for text format.)

If there is more than one reference to the same author and year, distinguish them by the letters, a, b, etc. added to the year. [". . . Levy (1965a:331) . . ."] Give the publisher's name in as brief a form as is fully intelligible. For example, John A. Wiley and Sons should be "Wiley." If the cited material is unpublished, use "forthcoming" with name of journal or publisher; otherwise use "unpublished."

Use no underlining, italics or abbreviations.

Examples follow:

1. *Books:* Jud, Gerald J., Edgar W. Mills, Jr. and Genevieve Walters Burch
   1970 Ex-Pastors. Philadelphia: Pilgrim Press.
   U.S. Bureau of the Census
   1960 Characteristics of Population. Volume 1. Washington, D.C.: U.S. Government Printing Office.
   Bernard, Claude
   [1865] 1957 An Introduction to the Study of Experimental Medicine. Tr. Henry Copley Greene, New York: Dover.
2. *Periodicals:* Conger, Rand
   Forthcoming "The effects of positive feedback on direction and amount of verbalization in a social setting." Pacific Sociological Review.
   Merton, Robert K.
   1963a "The ambivalence of scientists." Bulletin of The Johns Hopkins Hospital 112:77–97.
   1963b "Resistance to the systematic study of multiple discoveries in science." European Journal of Sociology 4:237–82.
3. *Collections:* Davie, M.
   1938 "The pattern of urban growth." Pp. 133–61 in G. Murdock (ed.), Studies in the Science of Society. New Haven: Yale University Press.

See recent issues for further examples.

Revised 1980 ASR

P 17831

AMERICAN SOCIOLOGICAL REVIEW

Official Journal of the American Sociological Association

FEBRUARY, 1980 VOLUME 45, NUMBER 1

ARTICLES

A New Theory of Distributive Justice .... Guillermina Jasso 3

Sexual Embourgeoisment? Social Class and Sexual Activity: 1938–1970 .... Martin S. Weinberg and Colin J. Williams 33

Sudden Wealth, Gratification and Attainment: Durkheim's Anomie of Affluence Reconsidered .... Mark Abrahamson 49

Industry Marginality and Female Employment: A New Appraisal .... William P. Bridges 58

The Flemish Movement in Belgium after World War II: A Dynamic Analysis .... François Nielsen 76

Reconciling Race and Class Differences in Self-Reported and Official Estimates of Delinquency .... Delbert S. Elliott and Suzanne S. Ageton 95

RESEARCH NOTES

The Geography of Soviet Criminality .... Louise Shelley 111

Analyzing Collapsed Contingency Tables without Actually Collapsing .... Paul D. Allison 123

COMMENTS AND REPLIES

Comment on Stack, ASR, December, 1978
Keynesian Government Intervention and Income Inequality .... Robert W. Jackman 131

Comment on Stack, ASR, December, 1978
The Case of the *Missing-Values* Card, and Other Mysteries:
Another Look at the Effect of Government Spending on Income Inequality .... Glenn Firebaugh 137

Reply to Jackman and Firebaugh
Direct Government Involvement in the Economy: Theoretical and Empirical Extensions .... Steven Stack 146

Comment on Quadagno, ASR, February, 1979
Sociobiology and Paradigms in Evolutionary Theory .... Robert Bolin and Susan Bolton Bolin 154

Reply to Bolin and Bolin .... Jill S. Quadagno 159

Editor: RITA J. SIMON

Deputy Editors: CLARK McPHAIL AND JOE L. SPAETH

Associate Editors: PAUL BURSTEIN, ROBERT EVAN COLE, THOMAS J. FARARO, BERNARD FARBER, DANIEL GLASER, MICHAEL J. HINDELANG, GISELA HINKLE, ROSABETH KANTER, JAMES R. KLUEGEL, JOHN LOFLAND, MURRAY MELBIN, MICHAEL MOCH, ANTHONY OBERSCHALL, VALERIE K. OPPENHEIMER, JEFFREY PFEFFER, ALEJANDRO PORTES, SAMUEL PRESTON, BARBARA RESKIN, RICHARD RUBINSON, DAVID R. SNYDER, CHARLES W. TUCKER, JOHN WILSON, MAYER N. ZALD, LYNNE G. ZUCKER.

Copy Editor: ELIZABETH ROBINSON NEUMANN

Managing Editor: LINNA McDADE

Editorial Assistant: HELEN CURLEY

Executive Officer: RUSSELL R. DYNES

SCOPE AND MISSION OF AMERICAN SOCIOLOGICAL REVIEW: The *American Sociological Review* publishes work of interest to the discipline in general, new theoretical developments, results of research that advance our understanding of the most fundamental social processes, and important methodological innovations. Like other publications of the Association, emphasis is given to exceptional quality. Unlike the more specialized journals of the Association, the *American Sociological Review's* primary focus is on whatever has the most general bearing on the knowledge of society.

Concerning manuscripts, address: Editor, *American Sociological Review,* Department of Sociology, University of Illinois, Urbana, Illinois 61801. (217) 333-6515.

The *American Sociological Review* (ISSN 0003-1224) is published at 49 Sheridan Avenue Albany, New York, bimonthly in February, April, June, August, October, and December.

Applications for permission to quote from this journal should be addressed to the Executive Office

Copyright © 1980 American Sociological Association.

Annual membership dues of the American Sociological Association, $20–$120 ($10 of this amount may cover a subscription to the *American Sociological Review*); Associate, $25; International Associate, $25; Student Associate, $12.

Application for membership and payment of dues should be made to the Executive Office. Subscription rate for members, $10; nonmembers, $15; institutions and libraries, $30; nonmember students, $10. Single issue, $4.

New subscriptions and renewals will be entered on a calendar year basis only.

Change of address: Six weeks advance notice to the Executive Office and old address as well as new are necessary for change of subscriber's address.

Concerning advertising, changes of address, and subscriptions, address: Executive Office, American Sociological Association, 1722 N Street, N.W., Washington, D.C. 20036.

Claims for undelivered copies must be made within the month following the regular month of publication. The publishers will supply the missing copies when losses have been sustained in transit and when the reserve stock will permit.

Second class postage paid at Washington, D.C. and additional mailing offices.

MEMBERS OF THE COUNCIL, 1980

Officers of the Association

PETER H. ROSSI, President
University of Massachusetts, Amherst
HELEN MacGILL HUGHES, Vice President
Cambridge, Massachusetts
WILLIAM FOOTE WHYTE, President-Elect
Cornell University
RENEE C. FOX, Vice President-Elect
University of Pennsylvania

JAMES F. SHORT, JR. Secretary
Washington State University
HERBERT L. COSTNER, Secretary-Elect
University of Washington
HUBERT M. BLALOCK, JR., Past President
University of Washington
RUSSELL R. DYNES, Executive Officer
Ohio State University

Elected-at-Large

PAULINE B. BART
University of Illinois, Chicago Circle
NORMAN BIRNBAUM
Amherst College
ERNEST Q. CAMPBELL
Vanderbilt University
ARLENE K. DANIELS
Northwestern University
IRWIN DEUTSCHER
University of Akron
WILLIAM GAMSON
University of Michigan

HELENA Z. LOPATA
Loyola University of Chicago
THOMAS F. PETTIGREW
Harvard University
MORRIS ROSENBERG
University of Maryland
IMMANUEL WALLERSTEIN
SUNY, Binghamton
CHARLES V. WILLIE
Harvard University
MAURICE ZEITLIN
University of Cal., Los Angeles

(Articles in the REVIEW are indexed in the Social Sciences Index, Psychological Abstracts, Sociological Abstracts, Ayer's Guide, University Microfilms, Abstracts for Social Workers, International Political Science Abstracts, and United States Political Science Documents.)

ITEMS

■ GUILLERMINA JASSO (A New Theory of Distributive Justice) is Research Director on the staff of the Select Commission on Immigration and Refugee Policy, Washington, D.C. Off duty she maintains her long-term interest in inequality and the sense of justice, and their consequences for social life.

■ MARTIN S. WEINBERG (Sexual Embourgeoisment?) is both Professor in the Department of Sociology and a Senior Research Sociologist, Institute for Sex Research, at Indiana University, Bloomington. He has two forthcoming books: *Sexual Preference: Its Development among Men and Women*, co-authored with Alan P. Bell and Sue Kiefer Hammersmith; and *The Solution of Social Problems: Five Perspectives* (2nd Ed.), co-authored with Earl Rubington and Sue Kiefer Hammersmith.

(Continued on page 163)

AMERICAN SOCIOLOGICAL REVIEW

A NEW THEORY OF DISTRIBUTIVE JUSTICE*

301.05
Am 35

GUILLERMINA JASSO
U.S. Immigration and Naturalization Service

American Sociological Review 1980, Vol. 45 (February):3–32

This paper proposes a new theory of distributive justice, one that is fully mathematized and fully testable. The theory describes how choices made by individuals (about which goods to value, which social aggregates to form, and what conditions to regard as just or fair for themselves) combine with two objective structural features—the actual distributions of goods and their intercorrelations—to produce the sentiment of justice in the individual. The theory views social relations as a meeting of justice sentiments. Hence, a social aggregate is characterized by the distribution of justice sentiments experienced by its members; and the social aggregate's characteristic social facts are approximated by the parameters of the distribution of justice sentiments, such parameters as the proportion who feel unjustly underrewarded, the Gini's mean difference, the lower extreme value, the overall mean, and many others. By holding constant both the goods of value to the members of a social aggregate and the condition regarded by them as just or fair, it becomes possible to generate expected distributions of justice sentiments from knowledge of the actual distributions of goods, thereby enhancing the usefulness of the theory. In this paper, using three hypothetical distributions of goods (Rectangular, Pareto, and Lognormal), we generate the corresponding expected justice distributions—one negatively skewed Exponential, a family of positively skewed Exponentials, and a family of Normals—and evaluate many parameters of interest.

This paper proposes a new general theory of distributive justice, one which is fully mathematized and fully testable and whose explicit aim is the explanation and prediction of a wide range of empirical phenomena at both the individual and the social levels. This theory builds on the specification of the justice evaluation function which I recently proposed (Jasso, 1978).

That work describes an empirically induced justice evaluation function for the case of earnings, and suggests its generalization to the sentiment of justice about all goods of distribution. The proposed function claims that the justice evaluation varies as the logarithm of the ratio of the actual share of a good to the perceived just share. That is,

$$\text{justice evaluation} = \ln \frac{\text{actual share}}{\text{just share}}, \quad (1)$$

where the sense of justice is represented by the full real-number line, with zero representing perfect justice, the positive segment representing unjust overreward, and the negative segment representing unjust underreward. This function quantifies the common opinion that deficiency is felt more keenly than comparable excess. The function was shown to accord with perceptions of justice regarding earned income, based on survey data from a probability sample of adult respondents in private households (Jasso, 1978).

The present paper attacks two major tasks: (1) to embed the justice evaluation function in an explicit theory of justice; and (2) to specify behavioral consequences of the theory which will permit an independent test of it.[1]

---

* Direct all communications to: Guillermina Jasso; 15 Penny Lane; Baltimore, MD 21209.

This paper benefited from the substantive comments and editorial advice given by an anonymous *ASR* referee, by the editors of this journal, and, most particularly, by Murray Webster, Jr. I am also grateful to Ludwig Brand for advice on preparing the figures, to Morris Zelditch, Jr., Robert Nisbet, and Herbert Passin for comments on earlier versions, and to the Barnard College Research Committee for assistance in manuscript preparation of those versions.

[1] Comprehensive summaries of the literature on distributive justice have recently appeared (e.g., Cook, 1975; Walster et al., 1976; Jasso and Rossi,

The main features of the general theory of distributive justice proposed in this paper are as follows:

(1) The theory notices only the justice evaluations made by individuals about their own shares of goods. That is, it does not conceptualize the phenomenon of "disinterested outrage" over injustice suffered by others (see Berger et al., 1972:126, for a discussion of this issue).

(2) The general theory asserts the mathematical form of the justice evaluation function's two predictor variables, the actual term and the just term, but leaves exogenous their substantive content.

(3) The general theory asserts the combinatorial function for the experience of justice about more than one good at a time.

(4) The theory suggests that an individual may be characterized by the subjectively experienced magnitude of justice or injustice and by his/her time series of justice evaluations; it also suggests that phenomena observable in individuals vary with the instantaneous justice evaluation or with the parameters of the individual's time series.

(5) The theory suggests that a social aggregate may, at any point in time, be represented by the frequency distribution of the instantaneous justice evaluations experienced by individuals in the social aggregate; it also suggests that social phenomena vary as functions of the parameters of these instantaneous distributions. Variation in social phenomena over time can be understood by reference to changes in the distribution of justice evaluations.

Thus, the theory offers a unified view of human behavior, with the sense of justice a basic factor in the etiology of phenomena at both the individual and the social levels. In this paper, however, our main focus shall be on social aggregates and social-level phenomena.

I. THE GENERAL THEORY OF DISTRIBUTIVE JUSTICE

A. Preliminary Definitions

1. Goods are what people want.

They want goods not only for their own sake, but also for the sake of happiness (Aristotle [384–322 B.C.] 1952, *Nicomachean Ethics,* Book I, chap. 7). Goods may be described along several dimensions; the theory notices three:

a. primary versus *secondary*—Primary goods are goods that "every rational [person] is presumed to want" (Rawls, 1971:62). All nonprimary goods are secondary goods.

b. social versus *natural*—Social goods are goods whose distribution is controlled socially; natural goods comprise the more intrinsic attributes of the person, goods that are, as Rawls (1971:62) puts it, "not so directly under [societal] control." Rawls (1971:62) suggests that the chief social primary goods include "rights and liberties, powers and opportunities, income and wealth" and that the natural primary goods include "health and vigor, intelligence and imagination."

c. quality versus *quantity*—Quality-goods are nonadditive, nontransferable personal attributes, such as beauty, intelligence, or social honor; quantity-goods are additive, transferable possessions, such as wealth or livestock (cf., Campbell [1921] 1952:125). The most important measurement strategy contained in the present theory, and codified in the two Principles (to be discussed here), is based on this distinction between quality and quantity.

2. Just amount or level of a good.

This is the amount or level considered to be just by the person making the justice evaluation; that is, it is the denominator of the ratio in the justice evaluation function.

3. The Comparison Aggregate (CA).

This is a physical or conceptual aggregate that is used by humans when they locate their rank on a quality-good and when they calculate their relative share of a quantity-good.[2] The Comparison Aggregate is also used when humans form the just term for quality-good and when they express the just amount of a quantity-good as a just share.

1977; Robinson and Bell, 1978; Jasso, 1978; and Webster and Smith, 1978), and will not be repeated here. Our greatest debt is to the work of George C. Homans (1967; 1974) and of Joseph Berger and his associates (1972).

[2] Our concept of Comparison Aggregate is considerably wider than its ancestors (see Hyman, 1942; 1966; Merton and Rossi, 1950; Kelley, 1952; Festinger, 1954; Merton, 1957; and Berger et al., 1972), for it encompasses any aggregate used by an individual in comparison processes, whether or not it is a "group" and whether or not the individual is a "member."

4. The Social Aggregate (SA).

This is any physical or conceptual aggregate of humans who are mutually aware of each other and of the aggregate they form (Nisbet, 1970:81). (Notice that the Comparison Aggregate and the Social Aggregate may, but need not, be the same for a given justice evaluation situation. For example, a person may evaluate his/her statesmanship relative to the Comparison Aggregate, "all Presidents of the United States," in which case the CA cannot possibly be a Social Aggregate. On the other hand, this same individual may evaluate his/her statesmanship relative to the Comparison Aggregate, "all heads of government in February of 1978," in which case the CA may well be also a Social Aggregate.)

B. The Basic Part of the General Theory of Distributive Justice

To construct the theory, we begin by stating two orienting (metatheoretical) assumptions. We take as given that:

> (1) Self-appraisal is the elementary judgment made by humans about their endowments, attributes, and possessions, whether dealt them by nature and society or obtained by personal exertion.
>
> (2) Humans form notions of justice, of the just holding of the natural[3] and social goods—that is, about the just results of the natural and social lotteries and of personal exertions.

We then formalize the method for the measurement of the self-appraisals and the notions of justice and propose the mathematical relations between them. The measurement strategy is codified in two Principles and the mathematical relations in three Laws. The formal statement of the theory follows:

First Principle:
Ordinal Appraisal of Quality-Goods and Cardinal Appraisal of Quantity-Goods

The mathematical form of the self-appraisal differs according to whether the good is a quality or a quantity.[4] *With respect to quality-goods, humans locate themselves in the rank orders of goods in selected Comparison Aggregates, calculating for themselves an Actual Rank. With respect to quantity-goods, humans measure their amount of goods in the intrinsic units of the goods, calculating for themselves an Actual Amount.*

Second Principle:
Ordinality of Just Term for Quality-Goods and Cardinality of Just Term for Quantity-Goods

The mathematical form of the notion of justice, i.e., of the just holding of a good, differs according to whether the good is a quality or a quantity. With respect to quality-goods, the just term is measured as a rank score, termed the Just Rank. With respect to quantity-goods, the just term is measured in the good's own intrinsic units, termed the Just Amount.

First Law:
Comparison of Actual to Just Holding of Goods

Humans compare their actual holdings of goods to their notions of the just holdings by taking a ratio of the Actual term to the Just term; we call this the Comparison Ratio (CR). With respect to quality-goods, the Comparison Ratio is the ratio of Actual Rank to Just Rank; with respect to quantity-goods, the Comparison Ratio is the ratio of Actual Amount to Just Amount. The Comparison

---

[3] Philosophers and theologians have almost unanimously argued that justice terms ought not to be predicated about the natural inequalities (e.g., Miller, 1976:18). For Aquinas this became an important issue, for to admit that the natural inequalities are unjust is ultimately to impute injustice to their author. Rawls (1971:102) remains in this tradition—though possibly not for the same reasons as Aquinas, noting, "The natural distribution is neither just nor unjust. . . . These are simply natural facts. What is just or unjust is the way that institutions deal with these facts." On the other hand, Freud ([1929] 1952:788) in *Civilization and Its Discontents,* as a clinician alive to empirical sentiments, observed that "nature began the injustice by the highly unequal way in which she endows individuals physically and mentally, for which there is no help." Our theory accepts Freud's manner of speaking as an accurate description of the human sentiment of justice.

[4] The two Principles embody our answer to the argument between Homans (1967:66–7) and Berger et al. (1972:127) about whether humans care about ordinal or cardinal differences in their shares of goods. We find them both right. Our work has been only to distinguish the class of goods about which ordinal differences are important from the class of goods about which cardinal differences are what matter.

Ratio is an exclusively cognitive magnitude, completely devoid of emotional content.

Second Law: Justice Evaluation

Humans evaluate the justice or injustice of their natural endowments and social attributes and possessions by means of an implicit operation of the emotions and sentiments that can be formally represented as the logarithm of the Comparison Ratio. A magnitude of zero denotes a condition of perfect justice; a negative magnitude denotes unjust underreward; and a positive magnitude denotes unjust overreward.

Third Law: Justice Evaluation of Several Goods

At times, humans simultaneously (and, of course, instantaneously) regard their holdings of more than one good. At such times, the magnitude of the justice evaluation is equal to the arithmetic mean of the single-good justice evaluations. This relation may be expressed in several equivalent forms, such as the logarithm of the geometric mean of the Comparison Ratios.

◆

Notice that the general theory regards as exogenous the two sets of factors that produce the Actual condition and the Just condition (which in turn must be known before a justice evaluation can be predicted)—regards as exogenous, that is, the actual distributions of Goods and the individual's choice of Goods, Comparison Aggregates, and Just terms.[5] How and why some goods rather than others are chosen at particular moments, some Comparison Aggregates rather than others, some notions of justice rather than others, how and why humans change their minds (going from one justice evaluation to another, to another), how and why social goods come to be distributed in particular ways—these are questions about which the general theory is silent. Later in this paper we shall make some preliminary conjectures but, for now, the history of each justice evaluation lies outside our field of interest. What matters for now is what happens once a human experiences a particular magnitude of the sentiment of justice.

C. The Theory's Strategy for the Explanation of Social Phenomena

It is neither unreasonable nor novel to suggest that the sense of justice is a crucial determinant of much in the human condition. Whether one feels justly or unjustly treated, unjustly overrewarded or unjustly underrewarded—these are primordial sentiments; and to suggest that behaviors (as well as other sentiments) depend on them is merely to echo twenty-five hundred years of reflection on the human experience.

In social matters, the principal voices of Western literature grant to justice an unusual primacy. Justice, wrote Aristotle, is "the bond of men in states," and, more than bond, it is the sine qua non of friendship. Without scrupulous attention to distributive justice, wrote Cervantes, not even a band of thieves could live together.

We now suggest a simple systematization of our inherited view. Whenever individuals come together, aware of each other and of the aggregate they form, that Social Aggregate, at that point in time, may be represented by the frequency distribution of the instantaneous justice evaluations experienced by the members; and on the parameters of that frequency distribution depend many social variables of interest. Moreover, at that moment in time, the Social Aggregate *is* the Comparison Aggregate. Since, by definition, the members of the Social Aggregate are aware of the SA, it follows that any justice evaluations experienced by an individual qua member of the Social Aggregate will have the SA as Comparison Aggregate.

This means that in the general theory, the dynamic in social life flows from factors both internal and external to the individual, both inside and outside his/her control. The internal factors include the free selection of goods of value and of what the individual considers just, for him/herself, as well as the innate constraint in the mathematical manner in which he/she appraises the Actual condition and the Just term. The major external factors are the outcomes of the natural and social lotteries, over which the individual may have little or no control. All of these factors combine to produce a distribution of justice evaluations that may assume an infinity of shapes, locations, and scales.

---

[5] By leaving exogenous the selection of the Just term, this theory can accommodate both Homans' (1974) and Berger et al.'s (1972) conceptions of the sources of justice—that is, both existential and utopian notions of justice.

This collection of justice sentiments, which we use to represent the Social Aggregate, may (as may any observed distribution) be arranged into an array, and notice taken of its many parameters, such as the arithmetic mean, the lower extreme value, the Gini's mean difference, etc. These distributional parameters constitute the independent variables suggested by the theory for use in propositions explanatory of social phenomena. The theory thus affords an enormous number of possible explanatory terms or sets of terms.

Now, social phenomena are of two types: aspects of groups that are, first, merely aggregations of individual-level phenomena and, second, specifically social phenomena. The first type includes summary measures of any phenomenon observed at the level of the individual; for example, if the theory successfully explains the individual's propensity to criminal activity, then the aggregate crime rate should be a function of the mean propensity to criminal activity. The second type includes variables that are not observed in individuals, properly speaking, but only in aggregates—such phenomena as a group's propensity to dissolution and a group's propensity to violent revolutionary conflict. Both types of social phenomena, we suggest, may be explained and predicted by parameters of the observed distribution of instantaneous justice evaluations. Thus, we are suggesting that interpersonal relations, within a Social Aggregate, may be regarded as basically a meeting of justice sentiments.

As for the temporal dynamic in social life, we propose that it arises from changes in the Social Aggregate's frequency distribution of justice evaluations. This means that whenever individuals select new Goods to value or acquire new notions of Justice or form new Social Aggregates, or whenever the actual distribution of the Good of value changes, the particular configuration of justice sentiments undergoes a corresponding change—as do all parameters of interest and whatever social phenomena vary with them. Social change, then, can be understood as changes in the justice distribution.

D. Some Hypotheses

We now suggest some hypotheses for the relations between social dependent variables and the independent variables produced by the theory.[6] We emphasize here the preliminary nature of these hypotheses. Some may not survive systematic literature review and others may not survive empirical test; and the surviving hypotheses depend totally on empirical research for specification of their functional form and for parameter estimation. The propositions that follow both illustrate the formal structure of hypotheses that one can generate from the theory and suggest some substantive relations:

1. For all Social Aggregates, the propensity to dissolution—represented by the positive segment of the real-number line, with zero the point of perfect stability (i.e., absence of a propensity to dissolution)—varies directly with the Gini's mean difference of the distribution of justice evaluation scores. When the aggregate of interest is a political unit, the propensity to dissolution may be regarded as a measure of political instability. We consider that such propensity to dissolution, or instability, depends critically and equally on all pairwise relations in the aggregate, a notion precisely captured by the Gini's mean difference.

---

[6] We follow Toulmin's (1953:77–85) view of the hierarchical structure of a theory. Thus, some of the terms used in the hypotheses (for example, "lower extreme value of the distribution of justice evaluations in a Social Aggregate") "depend for their very *meaning*" on the set of Principles and Laws in the theory. The hypotheses and the Laws cannot be logically deduced from one another; rather, they are linked by terms which appear in both. The logical connection between the hypotheses and the Laws is that such terms as "lower extreme value of the distribution of justice evaluations in a Social Aggregate" can be introduced only by reference to the Law of Justice Evaluation. Moreover, the general theory of distributive justice meets the criterion of open-endedness, for it leaves to empirical discovery the exact relations between the terms it produces and other empirical terms.

2. For all Social Aggregates, the degree of concord or discord is equal to the arithmetic mean of the distribution of justice evaluations; at the societal level, this may be regarded as a measure of social content or discontent. This measure gives equal weight to the justice sentiments of all members of the SA. It may take on either a positive or a negative value.

3. The Social Aggregate's propensity to violent revolutionary conflict varies directly with the absolute magnitude of the lower extreme value of the distribution of justice evaluations. Once cleavage occurs, splitting the Social Aggregate into two warring segments, the intensity of the revolutionary violence varies directly with the arithmetic mean of the better-off segment's justice evaluations (i.e., with the higher of the two segments' arithmetic means). Thus, we regard injustice of the underreward sort (i.e., relative deprivation) as the main determinant of the propensity to revolution and injustice of the overreward sort (i.e., relative excess) as the main determinant of the intensity of violent revolutionary conflict.

4. Crime occurs in a Social Aggregate only when the SA contains both negative and positive justice evaluation scores (i.e., both underrewarded and overrewarded persons) and when the absolute magnitude of the lower extreme value is greater than the upper extreme value of the distribution. In such case, the crime rate varies directly with the proportion found in that left-most segment whose upper end-point is of identical absolute magnitude as the overall distribution's upper extreme value. We term such a segment a Distressed segment.

5. The mean seriousness of all crimes varies directly with the absolute magnitude of the mean of the Distressed segment.

6. The mean seriousness of crimes against persons varies directly with the absolute distance between the overall mean and the mean of the Distressed segment.

7. The rate of mental illness varies directly with the proportion of negative justice evaluation scores—but excluding the Distressed segment, if any.

8. The mean severity of mental illness varies directly with the absolute magnitude of the mean of the unjustly underrewarded segment—again excluding the Distressed segment, if any.

9. For all Social Aggregates, the rate of emigration varies with the arithmetic mean of the individual members' propensity to emigrate, where propensity to emigrate is specified as the principal value of the arc secant function of the justice evaluation for justice evaluations ≤ -1 or $\geq +1$ and as zero elsewhere. This formulation permits separate measurement of emigration "from the bottom" and emigration "from the top," a feature useful in the study of not only residential migration but also other instances of leaving a Social Aggregate, such as dropping out and transferring out of school.

◆

Now, unless we directly ascertain the Actual term and the Just term for each individual in the Social Aggregate, the general theory cannot predict the distribution of justice evaluations and, hence, cannot predict the values of any of the distributional parameters which we use as independent variables in the hypotheses. To say that the selection—of Goods to value, of Social Aggregates to form, and of notions of justice—is exogenous is to assert that there is no *general* relation between the objective distributions of the natural and social goods and the individual and social behaviors which this theory seeks to explain. Nevertheless, in Part II, we propose two restrictions to the general theory which permit us to predict the distribution of justice evaluations and all its parameters solely from the distribution(s) of the good(s) of value.

II. MATHEMATIZATION AND THEORETICAL DEVELOPMENT

We have seen that the distribution of justice evaluations produced by the general theory may assume an infinity of shapes, scales, and locations. We now, first, introduce two restrictions (to the general theory) which make possible prediction of the distribution of justice evaluations from knowledge of the distribution of the good of value; second, we present the basic formulas; third, we apply the theory to Social Aggregates; and, finally, we conclude with a note on testing the theory.

A. Two Simplifying Restrictions: The Primitive Alternatives

We now introduce two restrictions which limit the individual's freedom in the selection of goods of value and of Just terms and which, together, produce a particular empirical realization of the theory which we term the Primitive model. Although these restrictions are, in fact, alternatives in the individual's selections, they are so powerful and so universal and so often chosen that we regard them as "default" alternatives and call them the Primitive alternatives. Both alternatives are present to the individual and, yet, are expressions of the human's social nature. And, as we shall see, use of the two Primitive restrictions permits a priori generation of expected distributions of justice evaluations, thus simplifying the mathematization and the task of testing the theory.

1. The just condition: "justice is equality."

The Primitive alternative in the choice of the Just term is the condition of equality. We suggest that, within Social Aggregates, when humans consider goods, when humans reflect on their attributes and their possessions, their universal longing is for equality.

The earliest expression of this identity known to us appears as words of Socrates in the *Gorgias;* but it recurs frequently in Aristotle (e.g., in the *Nicomachean Ethics,* Book 8), in the Patristic literature, in Vives, and in Cervantes. Variants of this identity may be found in Bentham [1802] 1950:104) and in Hume [1777] 1965:194); the contemporary economist E. S. Phelps (1973:27) notes that most thinkers on distributive justice agree that the only just division of a fixed pie is an equal one. Finally, we recall the observations of Homans (1967:64–7) that "justice is what men expect" and that "the sense of injustice is invariably grounded on a defeat of expectations, and expectations in the long run are always grounded on what actually happens" and of Berger et al. (1972:133) that the just amount of a goal-object is that amount which is "typically allocated."

Quantification of the Just term is straightforward: the equal level or amount of a good is the arithmetic mean of the good's distribution. Notice how exquisitely both Homans and Berger and his associates intuited this fundamental relation: the mathematical expectation of a distribution is equal to the arithmetic mean; and the typical value of a distribution may be represented by the arithmetic mean.

Now, we consider that this longing for equality is rigidly confined to the Social Aggregate. This means that psychological "membership" in a Social Aggregate structures what individuals come to regard as just (see William James, [1891] 1952:887).

2. Consensus on the Goods of value in a Social Aggregate.

The second Primitive alternative suggests that all members of a Social Aggregate value the same good or goods; that is, Social Aggregates in the Primitive model are characterized by consensus on the goods of value. This restriction limits the choice of value for members of a Social Aggregate during moments of conscious membership in the Social Aggregate.

This restriction is somewhat difficult to discuss, and perhaps expresses well the tensions and ironies of the human condition. The restriction is a feature of Social Aggregates. It arises out of a social dynamic, and yet is freely accepted by the members of the Social Aggregate. Put differently, there is a mysterious something about Social Aggregates that generates a prompting, an enticement, to consensus both generally as well as specifically on the goods of value.

That persons so often accede to this enticement to consensus is perhaps the empirical reason why Augustine chose to include consensus in his definition of the term, "people." For Augustine ([413–426]

1952, *City of God,* Book 19, chap. 24), "a people is an assemblage of reasonable beings bound together by a common agreement as to the objects of their love."

Thus, while Nisbet's definition of a Social Aggregate is wonderfully apt for the general theory, in the Primitive model we invoke Augustine's narrower conception.

Now, consensus in general refers to many things, including beliefs, attitudes, moral demands. In the present theory, however, we notice consensus about only the goods of value. That is, we restrict attention to those goods about whose distribution humans experience justice evaluations. As we shall see, while consensus in general may be a fine thing, consensus about the goods of value has mixed consequences.

B. Basic Formulas at the Level of the Individual

An important feature of the present work is that much of what we have said in words can be economically represented in the language of mathematics. In this section, we present the major formulas for the terms of the theory at the level of the individual. We assume the two Primitive restrictions. All magnitudes are instantaneous.

1. The case of quality-goods.

Consider the distribution of a given quality-good (any quality-good—such as beauty, honor, or intelligence) in a Social Aggregate. We assume that the good's distribution is continuous; this assumption ensures that the probability of tied ranks is zero. Next, we generate the set of discrete rank-order statistics (ROS), arranged in ascending order of magnitude. This set is the set of integers from 1 to the size N of the Social Aggregate. Finally, we convert the rank-order statistics into percentile ranks (see Snedecor and Cochran, 1972:123–5), forming a new, continuous Rectangular distribution. The formula for obtaining the percentile rank is

$$P_i = \frac{\text{ROS}_i\,(100)}{N + 1}\,. \qquad (2)$$

The percentile rank is, of course, the Actual Rank. Now recall that the Just Rank in the Primitive model is 50, which is the arithmetic mean of the unit Rectangular (expressed as a percentile). Thus, the Primitive justice evaluation is given by

$$\begin{matrix}\text{justice}\\ \text{evaluation}_i\end{matrix} = \log_a \frac{\text{Actual Rank}_i}{50}\,, \qquad (3)$$

where a represents the logarithmic base implicitly used by each individual, a quantity which we conjecture may be the individual's signature constant. The formula above may be restated in the following more compact form:[7]

$$\begin{matrix}\text{justice}\\ \text{evaluation}_i\end{matrix} = \log_a \frac{2\,\text{ROS}_i}{N + 1}\,. \qquad (4)$$

For k quality-goods considered simultaneously, the formula is

$$\begin{matrix}\text{multi-good}\\ \text{justice}\\ \text{evaluation}_i\end{matrix} = \log_a \left(\frac{2}{N + 1} \sqrt[k]{\prod_{j=1}^{k} \text{ROS}_{ij}} \right). \qquad (5)$$

2. The case of quantity-goods.

Consider the distribution of a given quantity-good (any quantity-good, such as wealth, earnings, income, land, or livestock) in a Social Aggregate. The quantity-good is measured in its own units. The order statistics, arranged in ascending order of magnitude, are represented by x_1, x_2, x_3, . . ., x_N, where N is the size of the Social Aggregate. The Just Amount in the Primitive model is the arithmetic mean of the goods's distribution, denoted by $\bar{x}$. Thus, the Primitive justice evaluation experienced by the i^{th} individual is given by

$$\begin{matrix}\text{justice}\\ \text{evaluation}_i\end{matrix} = \log_a (x_i/\bar{x})\,, \qquad (6)$$

where a is the individual's log base.

---

[7] A mathematically equivalent procedure for calculating the Primitive justice evaluation is as follows: Let Actual Rank be defined as the rank-order statistic; let Just Rank be defined as the arithmetic mean of the distribution of rank-order statistics, or (N+1)/2. Both procedures start out using a Discrete Uniform distribution and lead to a Continuous Uniform.

For m quantity-goods considered simultaneously, the formula for the justice evaluation experienced by the i$^{\text{th}}$ individual is given by

$$\begin{matrix}\text{multi-good}\\ \text{justice}\\ \text{evaluation}_1\end{matrix} = \log_a \sqrt[m]{\prod_{l=1}^{m} (x_{1l}/\bar{x}_l)}. \qquad (7)$$

3. Simultaneous consideration of quality-goods and quantity-goods.

The multi-good justice evaluation experienced by the i$^{\text{th}}$ individual about his/her level(s) and amount(s) of k quality-goods and m quantity-goods is given by

$$\begin{matrix}\text{multi-good}\\ \text{justice}\\ \text{evaluation}_1\end{matrix} =$$

$$\log_a\left\{ \sqrt[(k+m)]{\left(\frac{2}{N+1}\right)^k \left(\prod_{j=1}^{k} \text{ROS}_{1j}\right)} \cdot \sqrt[(k+m)]{\left(\prod_{l=1}^{m} (x_{1l}/\bar{x}_l)\right)} \right\}. \qquad (8)$$

4. Conversion of self-reported justice evaluations to the natural logarithmic scale.

While the experience of a given magnitude of the justice evaluation is the same across persons, individuals may vary in their style of description, some given to understatement, others to hyperbole. We represent this reporting style by the logarithmic base a used in the formula for the justice evaluation. Since the real-number line is infinitely dense, we conjecture that each individual may have a unique style of description, implicitly using a unique log base a. This log base a may be regarded as the individual's signature constant.

In order to render such self-reported justice evaluations comparable across persons, it is necessary to convert them into a uniform scale. We use the natural logarithmic scale for this purpose. The actual conversion process is in two steps. First, we must ascertain the log base a used by the individual; and, second, we must perform a change-of-base.

To find the individual's base a, we ascertain the Actual and the Just terms for a reported justice evaluation (denoted by^) and solve as follows:

$$a = \left(\frac{\text{Actual Term}}{\text{Just Term}}\right)^{1/\text{justice } \hat{\text{e}}\text{valuation}}. \qquad (9)$$

Next, we use the change-of-base theorem to convert the individual's self-reported scores to justice evaluations on the natural log scale:

$$\begin{matrix}\text{justice}\\ \text{evaluation}\end{matrix} = \frac{(\text{justice } \hat{\text{e}}\text{valuation})}{\log_a e}, \qquad (10)$$

where e is the base of the natural logarithms. At this point, a consequence of the change-of-base theorem becomes extremely useful, and that is:

$$\log_a \epsilon = 1/(\ln a). \qquad (11)$$

Hence, the simplest method of conversion is given by:

$$\begin{matrix}\text{justice}\\ \text{evaluation}\end{matrix} = (\text{justice } \hat{\text{e}}\text{valuation})\,(\ln a)\,. \qquad (12)$$

In this way, the self-reported justice evaluations are converted into a common scale and, hence, usable for interpersonal comparison as well as in the frequency distributions that we use to represent Social Aggregates.

C. Application of the Theory to Social Aggregates and Social Phenomena

The Primitive model, by holding constant the good(s) of value to the members of the Social Aggregate and the condition regarded by them as just, enables us to move swiftly from knowledge about the distribution(s) of the good(s) of value to the distribution of justice evaluations. Moreover, if the good of value is a quality-good, it is not even necessary to ascertain the Actual Rank of individual members of the Social Aggregate, for we already have all the information required to describe the distribution of justice evaluations and all its parameters.

There are two ways of approaching the task of representing Social Aggregates by their respective distributions of justice evaluations. The first uses the methods of descriptive statistics to answer the a posteriori questions: If the good of value has such-and-such an observed frequency distribution, then, given the Primitive restrictions, what does the distribution of justice evaluations look like? What are the observed values of the parameters of interest? This approach is particularly useful when studying Social Aggregates of small size N.

The second approach uses classical probability procedures to answer the a priori questions: If the distribution of the good of value has such-and-such density function, then (given the Primitive restrictions) what is the density function of the expected distribution of justice evaluations? What are the expected values of the parameters of interest?

1. Parameters of observed frequency distributions of justice evaluations.[8]

(a) The observed lower extreme value

Consider a Social Aggregate of size N; the members consensually value one good, either a quality-good or a quantity-good. The lower extreme value (LEV) of the distribution of justice evaluations is calculated as follows:

$$\text{lower extreme value} = \begin{cases} \ln \dfrac{2}{N+1}, & \text{quality-good} \\ \ln \dfrac{x_1}{\bar{x}}, & \text{quantity-good} \end{cases}, \quad (13)$$

where x_1 is the first order statistic of the quantity-good's observed frequency distribution and where $\bar{x}$ is its arithmetic mean.

The formulas for the lower extreme value of the distribution of justice evaluations underscore an implication of the theory: the size of the Social Aggregate always affects the experience of justice about quality-goods. The lower limit of the LEV, as N goes to infinity, is negative infinity. For quantity-goods, the lower limit of the LEV is also negative infinity, but the process is different; in this case the size of the LEV is a function of the ratio of the size of the smallest share of the good to the mean, and the lower limit goes to negative infinity as this ratio approaches zero from the right. Through its effect on the mean, N may also affect the LEV of the quantity-good's distribution.[9]

But whatever the process, whether due to the size of the Social Aggregate or to the size of the share, the formulas above mean that the sense of injustice of the underreward sort has no bounds. There is no limit to the enormity of the human experience of being unjustly underrewarded.

(b) The observed upper extreme value

Consider a Social Aggregate of size N; the members consensually value one good, either a quality-good or a quantity-good. The upper extreme value (UEV) of the distribution of justice evaluations—that is, the largest magnitude of unjust overreward—is calculated as follows:

$$\text{upper extreme value} = \begin{cases} \ln \dfrac{2N}{N+1}, & \text{quality-good} \\ \ln \dfrac{x_N}{\bar{x}}, & \text{quantity-good} \end{cases}, \quad (14)$$

where x_N is the N^{th} order statistic of the quantity-good's observed array.

The upper end of the distribution has a very striking feature. When the good of value is a quality-good, the upper limit of the upper extreme value is ln 2 (about .6931, rounded), a very rigid and modest limit to the experience of overreward. No such bound accompanies the experience of overreward about quantity-goods; in this case, the upper limit goes to positive infinity as the ratio of the size of the largest share to the mean goes to positive infinity.

These formulas have important empirical implications. For example, the most beautiful or most brilliant person in a Social Aggregate experiences a very modest magnitude of overreward. And whatever behavioral consequences are hypothesized to follow or accompany the large magnitudes of the sense of being overrewarded (such as guilt feelings or redress-ing behaviors) will be found only with respect to quantity-goods. Thus, for the case of social goods, the Primitive model predicts such behavioral consequences only when the good of interest is a material good, such as wealth or income, and not for such goods as honor or occupational prestige.

---

[8] Due to space limitations, we present formulas for only three of the parameters of observed distributions of justice evaluations.

[9] I am grateful to the anonymous referee for pointing this out.

(c) The observed arithmetic mean

Calculation of the arithmetic mean of the observed frequency distribution of justice evaluations renders visible a relation we regard as very exciting. Let us begin by deriving the arithmetic mean for the case of one good of value, either a quality-good or a quantity-good.

$$\text{arithmetic mean} = \begin{cases} \dfrac{\sum_{i=1}^{N} \ln\left(\dfrac{\left(\dfrac{100\,ROS_i}{N+1}\right)}{50}\right)}{N}, & \text{quality-good} \\[2ex] \dfrac{\sum_{i=1}^{N} \ln \dfrac{x_i}{\bar{x}}}{N}, & \text{quantity-good} \end{cases} \quad . \ (15)$$

The expressions above may be restated in their equivalent forms:

$$\text{arithmetic mean} = \begin{cases} \ln \dfrac{\sqrt[N]{\prod_{i=1}^{N}\left(\dfrac{100\,ROS_i}{N+1}\right)}}{50}, & \text{quality-good} \\[2ex] \ln \dfrac{\sqrt[N]{\prod_{i=1}^{N} x_i}}{\bar{x}}, & \text{quantity-good} \end{cases} \quad . \ (16)$$

Viewed in this way, it is immediately clear that the arithmetic mean of the distribution of observed justice evaluations in a Social Aggregate is the logarithm of the ratio of the geometric mean of the distribution of the good of value to the arithmetic mean of the good's distribution. That is,

$$\text{arithmetic mean} = \ln \frac{\text{Geometric Mean of the Good}}{\text{Arithmetic Mean of the Good}} \ . \quad (17)$$

Before turning to a discussion of this relation, we present a simple equivalent expression for the case of a quality-good, so simple that it makes an excellent computational formula:

$$\text{arithmetic mean} = \ln\left(\frac{2}{N+1}\sqrt[N]{N!}\right), \ \text{quality-good} \ , \quad (18)$$

where N! represents N-factorial. It can readily be shown that as the size N of the Social Aggregate increases, the arithmetic mean of the justice evaluations moves leftward, attaining progressively larger absolute values of negative magnitudes and approaching its limit of (ln2 − 1) or about −.3069. In the case of quantity-goods, there is no relation between N and the mean.

Returning now to the general formula for the arithmetic mean of the observed distribution of justice evaluations, let us look first at the ratio of the geometric mean to the arithmetic mean of the good's distribution. We regard this ratio as an interesting, if unorthodox, measure of relative dispersion in a distribution. It is unorthodox because measures of relative dispersion are usually formed by taking the ratio of a measure of absolute dispersion to a measure of location (e.g., the coefficient of variation) and not by taking the ratio of one to another measure of location. Nevertheless, the ratio has several interesting properties. First, since in all distributions of positive magnitudes except an Equal one the geometric mean is smaller than the arithmetic mean, the value of the ratio has fixed limits of zero and one (open at zero, closed at one). Second, the value of the ratio is independent of the units in which the good is measured. Third, the ratio is extremely easy to calculate. Finally, its most important feature may lie in its hypothesized relation to social discontent. If social discontent turns out to vary with the arithmetic mean of the justice distribution, then we may have hit upon a measure of inequality that is relevant to discontent, and we may have specified the theoretical relation:

$$\text{Social Discontent} = \ln \text{Inequality} \ . \quad (19)$$

2. *Expected distributions of justice evaluations.*

The expected distribution of justice evaluations in a Social Aggregate, given the Primitive restrictions, can be generated a priori—if we know the following three things: (1) What are the goods of value? (2) What is the probability density function of each? (3) And what are the intercorrelations among the goods? We now derive three of the simplest expected distributions and calculate the predicted values of the major parameters of interest. We have relied chiefly on three sources: Hoel (1971), Johnson and Kotz (1970a and 1970b), and Hastings and Peacock (1974).

(a) Generating expected distributions of justice evaluations

In this section, we look at three possible Social Aggregates, each having one good of value. In one Social Aggregate, the good of value is a quality-good, such as social honor or beauty or academic performance. In the other two Social Aggregates, the good of value is a quantity-good, such as any of a host of money variables (e.g., wealth, income, earnings, allowance) or land or livestock. Simply by knowing the distribution of the good of value, we can discover the distinctive representation of the Social Aggregate and predict its social facts. These three cases are offered not only as idealized representations but also as empirical possibilities.

The general method for generating expected distributions of justice evaluations is as follows: For single-good distributions, we take the actual distribution of the variable x (i.e., the good), and perform a change-of-variable in order to derive the distribution of the new variable y that is a function of the variable x, the function being the division of x by its own arithmetic mean (i.e., y is the Comparison Ratio). Next, we perform a second change-of-variable in order to derive the distribution of the new variable z that is a logarithmic function of y—that is, z is the justice evaluation.

For multi-good distributions, both change-of-variable and convolution techniques are used to obtain the distribution of multi-good justice evaluations.

All expected distributions are defined by their probability density function, denoted by f(x). Following the notation in Hastings and Peacock (1974), we use italicized small letters of the English alphabet to refer to the three basic parameters. These are:

> *a*—the location parameter. This parameter is the abscissa of a location point, usually the upper or lower or mid-point of the range.
> *b*—the scale parameter.
> *c*—the shape parameter.

The density functions for the distributions of justice evaluations are reported both in terms of their "own" parameters and as functions of the parameters of the good's distribution. In this way we can both quickly identify a given density as a member of a particular family of curves and make use of everything that is known about that family of curves and also see vividly how the characteristics of the distribution of the good of value affect the distribution of justice evaluations.

We do not in this paper formally report any of the multi-good densities we have obtained. However, notice that the many possible combinations of number of goods of value, of their intercorrelations, and of their densities make possible many distinct representations of Social Aggregates and hence help to produce the "infinite variety" of social life. We emphasize, again, our view that the multiform richness of social life may be of simple etiology and that the unifying principle may be the sense of justice.

(1) The case of one quality-good. Consider a Social Aggregate of very large size N tending to infinity. By the second Primitive restriction, all the members of the Social Aggregate value the same good. In this particular case, that one good is a quality-good. By the first Principle, the distribution of the Actual Ranks is a unit Rectangular whose loca-

tion parameter a equals zero and whose scale parameter b equals one. The density thus is given by:

$$f(x) = 1 \ , \ 0 < x < 1 \ . \qquad (20)$$

The graph of the probability density function for the unit Rectangular is shown on panel a. of Figure 1.

By the first Primitive restriction, the Just term for each member of the Social Aggregate is the good's arithmetic mean, the point of equality. The arithmetic mean of the unit Rectangular is .5. By a change-of-variable, we obtain the density function of the distribution of the members' Comparison Ratios. This new density also belongs to the Rectangular family:

$$g(y) = .5 \ , \ 0 < y < 2 \ . \qquad (21)$$

Finally, we perform a second change-of-variable in order to obtain the density of the justice evaluation variable in the Social Aggregate. That is, we obtain the density of the new variable that is a natural-logarithmic function of the Comparison Ratio variable. The new density is:

$$q(z) = e^{z}/2 \ , \ -\infty < z < \ln 2 \ . \qquad (22)$$

Upon inspection, we recognize this density as belonging to a negatively skewed family of Exponential distributions. The general formula, now using x to denote the variate, is given by:

$$f(x) = \frac{e^{(x-a)/b}}{b} \ , \ -\infty < x < a \ . \qquad (23)$$

In the present case the value of the scale parameter b equals one, and the value of the location parameter a equals ln 2. The graph of this probability density function that results from consensual valuation of one quality-good is shown on panel b. of Figure 1. (The graphs of the justice distributions shown on the b. panels of the three figures are drawn to the same scale.)

The location parameter a ($a = \ln 2$) has a very interesting interpretation. It is both the upper limit of the justice evaluation variable—that is, the greatest possible magnitude of unjust overreward, and the cologarithm of the Just term (which is .5). Thus, we can say that the Just term determines the upper limit of the sense of justice with respect to quality-goods as well as the location parameter of the distribution's density function.

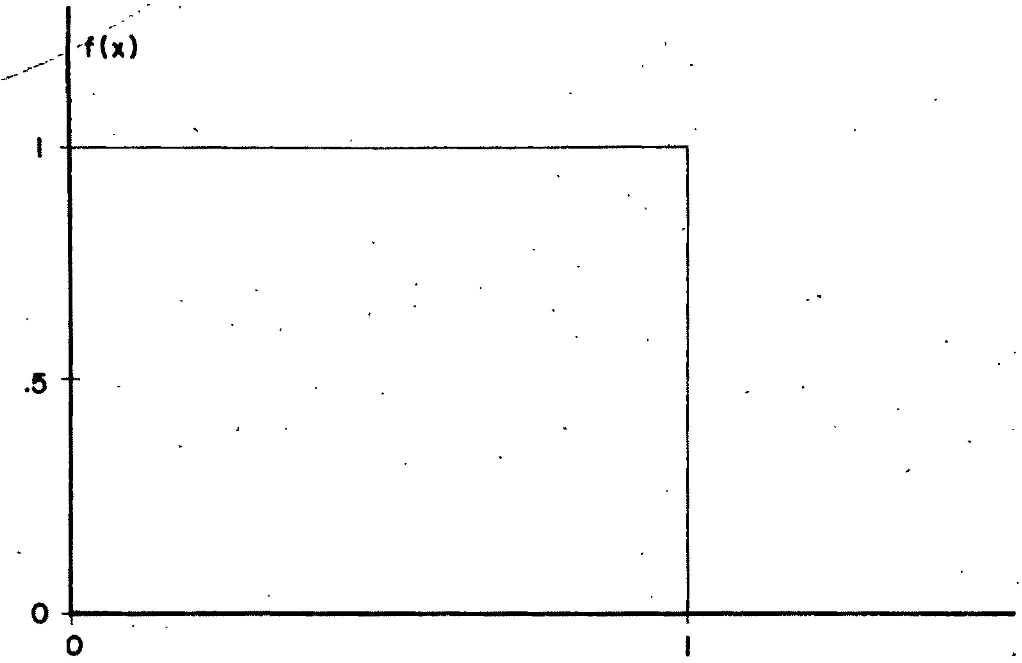
In section *2.(b)* below we shall look closely and precisely at the properties of this distribution. Here we merely note that, since in the Rectangular the arithmetic mean and the median are equal, we know that half of the members of the Social Aggregate experience unjust underreward while the other half feel unjustly overrewarded. We also know that the experience of injustice of the underreward sort ranges to negative infinity—that is, that the Social Aggregate has members who feel extremely unjustly underrewarded. Put differently, while the proportions in either condition of injustice are equal, their experience is vastly different. The intensity of the two segments' sentiments of justice differs greatly—just how greatly we shall see in section *2.(b)*.

(2) The case of a Pareto-distributed quantity-good. Consider a Social Aggregate of very large size N tending to infinity. By the second Primitive restriction all the members of the Social Aggregate value the same good. In this particular case, that one good is a quantity-good. Now the distribution of a quantity-good is regarded by this theory as exogenously given. Hence, its density function must be either empirically estimated or introduced by a priori restriction. In this section and the next, we imagine that the quantity-good of value is distributed as a Pareto and as a Lognormal variate, respectively, and inquire what may be the consequences of such distribution for the sentiment of justice experienced by the members of the Social Aggregate.

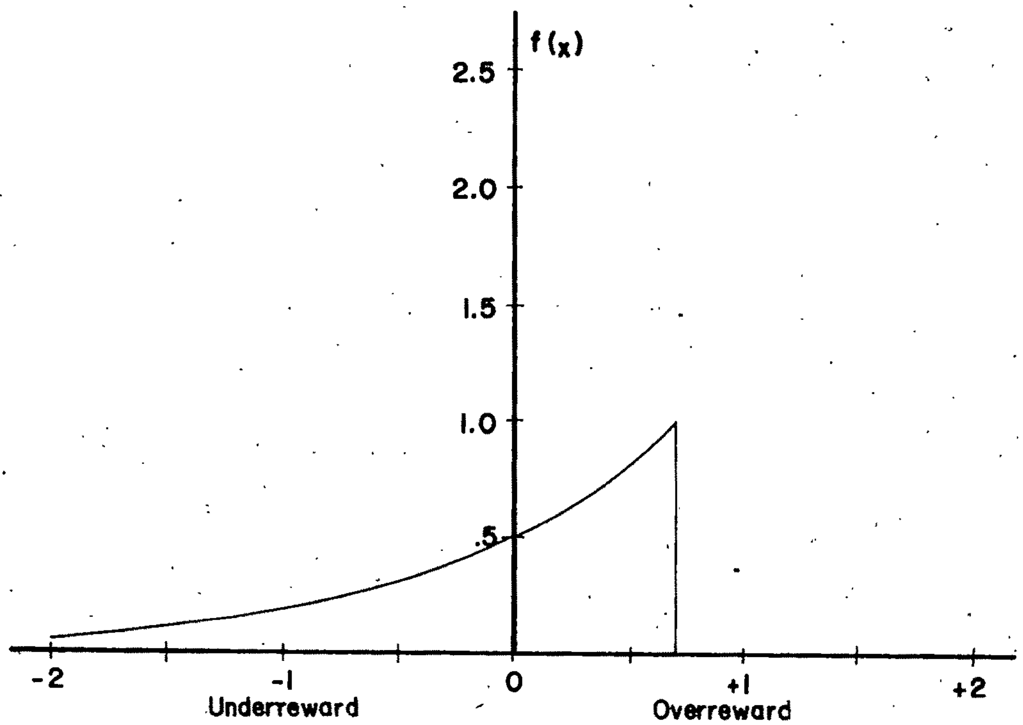
Suppose now that the quantity-good is Pareto-distributed.[10] The major features of the Pareto variate's probability density

---

[10] Many quantity-goods of value to diverse Social Aggregates may indeed be Pareto-distributed. Some examples are: family income in a welfare state; earnings within skill level, seniority, and firm; children's allowances within age cohort at a summer camp.



a. Unit Rectangular Density Function



b. Negatively Skewed Exponential Density Function

Figure 1. Distribution of Quality-Good and of Justice Evaluations

function are that it is everywhere decreasing, with a long right tail, and that the mode occurs at the lower limit. The Pareto family is often expressed in terms of a transformed variable, which is obtained by first dividing all observations by the lower extreme value. For example, if the lower extreme value of a given income distribution is $7,238.49, then all the income amounts are divided by that quantity—thereby producing a new distribution whose lower extreme value is unity. When expressed this way, the Pareto is a one-parameter family; that one parameter is a shape parameter known as Pareto's constant. Pareto's constant is considered a crude measure of inequality in the distribution (e.g., Cramer 1971:51–8), with inequality decreasing as Pareto's constant increases.

The density function of the transformed Pareto variate is given by:

$$f(x) = cx^{-c-1}\ , \quad 1 < x < +\infty\ . \tag{24}$$

Graphs of the density functions of three members of the Pareto family are shown on panel a. of Figure 2. They are drawn in terms of the untransformed variable (e.g., raw-income amounts). (The graphs of the quantity-good distributions shown on the a. panels of Figures 2 and 3 are drawn to the same scale; and each of the six curves has an arithmetic mean of 10.)

By the first Primitive restriction, the Just term for each member of the Social Aggregate is the good's arithmetic mean, the point of equality. Now Pareto's constant, or the shape parameter c, may assume any positive value. However, the arithmetic mean of the Pareto distribution is theoretically defined only when c is greater than one. Hence, we introduce the additional restriction that c must exceed unity. In such case, the arithmetic mean of the Pareto variate is equal to $(c/(c-1))$. By a change-of-variable, we obtain the density function of the distribution of the members' Comparison Ratios. This density is:

$$g(y) = cy^{-c-1}(c/(c-1))^{-c}\ , \qquad ((c-1)/c) < y < +\infty\ . \tag{25}$$

Finally, we perform a second change-of-variable in order to obtain the density of the justice evaluation variable in the Social Aggregate. The new density is:

$$q(z) = \frac{e^{-cz}(c-1)^c}{c^{c-1}}\ , \qquad \ln((c-1)/c) < z < +\infty\ . \tag{26}$$

Upon inspection, we recognize this density as belonging to a positively skewed family of Exponential distributions. The general formula, now using x to denote the variate and a and b to denote the Exponential's own location and scale parameters, is given by:

$$f(x) = \frac{e^{-(x-a)/b}}{b}\ , \quad a < x < +\infty\ . \tag{27}$$

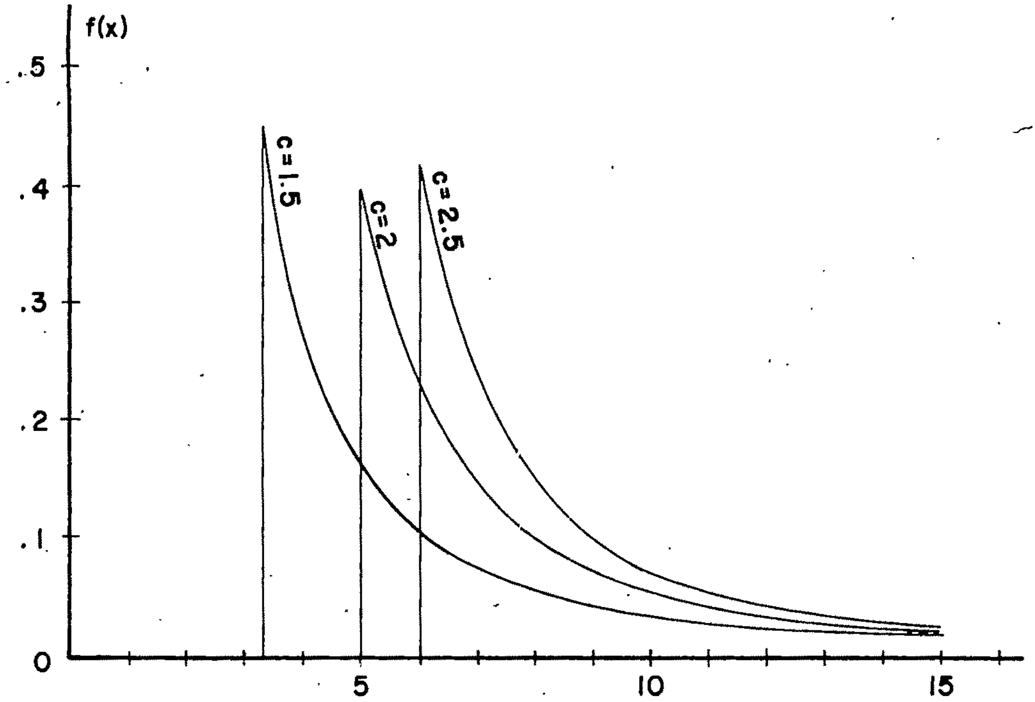
Graphs of the density functions of three members of this family are shown on panel b. of Figure 2.

Let us look now at the Exponential's two basic parameters and at their relations to the one parameter of the Pareto-distributed quantity-good. The Exponential's location parameter a is the lower limit of the Exponential; it is also the cologarithm of the Pareto variable's arithmetic mean. In other words, the Just term—the arithmetic mean of the Pareto distribution of the quantity-good—determines the location parameter of the distribution's density function (determines, that is, the lower limit of the sense of unjust underreward). And the Just term itself may be regarded as a function of Pareto's constant.

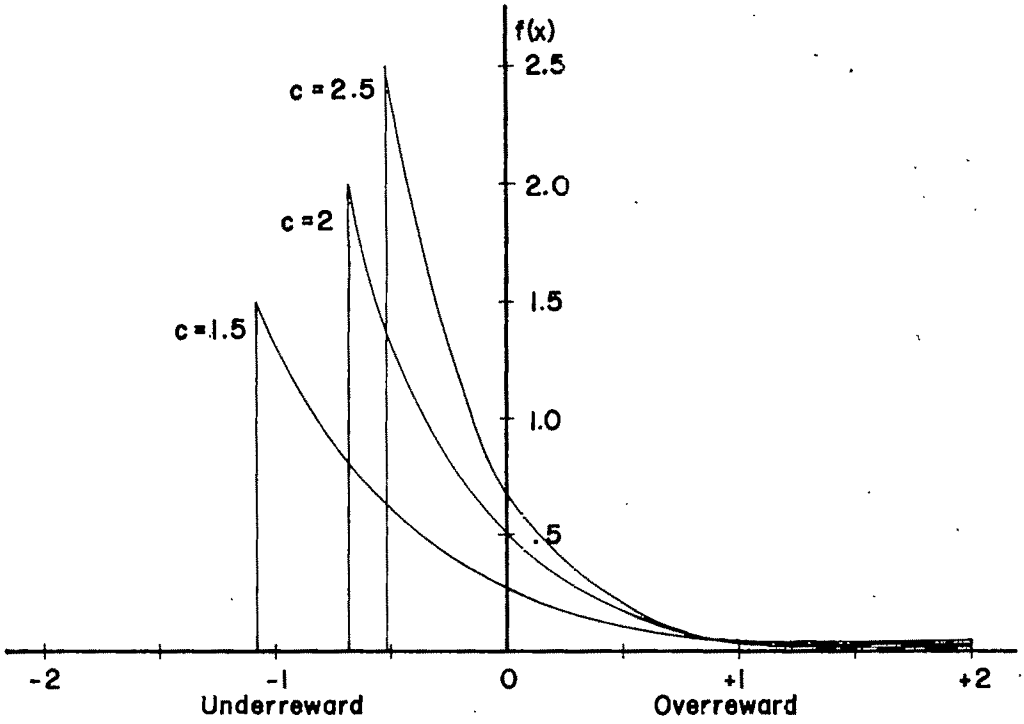
The Exponential's scale parameter b is also a function of Pareto's constant, being its reciprocal.

The Exponential's arithmetic mean we already know to be the logarithm of the ratio of the quantity-good's geometric mean to the quantity-good's arithmetic mean. Since both the geometric mean and the arithmetic mean are functions of Pareto's constant, we see clearly that the Exponential's arithmetic mean is also a function of Pareto's constant.

In section 2.*(b)* below we shall see precisely how these and other properties of



a. Pareto Density Functions



b. Positively Skewed Exponential Density Functions

Figure 2. Distributions of Pareto Quantity-Good and of Justice Evaluations

the Exponential family depend on various functions of Pareto's constant. At this point we merely say that, crudely, in one way or another, they *all* depend on the inequality in the distribution of the quantity-good.

Notice that while choice of one quality-good by the members of a Social Aggregate leads directly to one (and only one) distribution of justice evaluations—the negatively skewed Exponential described earlier—choice of one quantity-good may lead not only to different families of curves but also to different members of a family of curves. In the present case, the value of Pareto's constant may be said to determine the particular member of the Exponential family—that is, both the location and scale parameters of the positively skewed Exponential distribution of justice evaluations—that results from consensual valuation of one Pareto-distributed quantity-good.

The density function of this distribution shows that there is often a modest bound on the experience of unjust underreward when the quantity-good is Pareto-distributed. Very large absolute magnitudes, however, are to be found for the experience of overreward. But this family of curves is no panacea, for the proportion unjustly underrewarded is rather large.

This quantification renders unambiguous the meaning of the first Primitive restriction—the sense of justice responds to relative conditions. The absolute location of the quantity-good's distribution is totally irrelevant; only the inequality or dispersion in the distribution is of any import. This formulation points the way to design of strategies for reducing discontent, political instability, and the propensity to violent revolutionary conflict—and for doing so whether per capita income is increasing, decreasing, or remaining constant.

(3) The case of a Lognormally-distributed quantity-good. Consider a Social Aggregate of very large size N tending to infinity. By the second Primitive restriction, all the members of the Social Aggregate value the same good. In this case, that one good is a quantity-good and it is Lognormally distributed.

The Lognormal variate is a positively skewed, two-parameter family of curves with one important property: the distribution formed by the logarithms of the original observations belongs to the Normal family. The Lognormal has a range from zero to positive infinity. The two parameters are the scale parameter b which is the median of the distribution and the shape parameter c which is the standard deviation of the distribution of logged observations. The density function is given by:

$$f(x) = \frac{e^{-(\ln(x/b))^2/2c^2}}{xc\sqrt{2\pi}}, \; 0 < x < +\infty \,. \quad (28)$$

Graphs of the density functions of Lognormal curves are shown on panel a. of Figure 3.

By the first Primitive restriction, the Just term for each member of the Social Aggregate is the good's arithmetic mean, its point of equality. The mean is given by (b exp($c^2/2$)). By a change-of-variable, we obtain the density function of the distribution of the members' Comparison Ratios. The new density function is:

$$g(y) = \frac{e^{-((\ln y)+(c^2/2))^2/2c^2}}{yc\sqrt{2\pi}}, \quad 0 < y < +\infty \,. \quad (29)$$

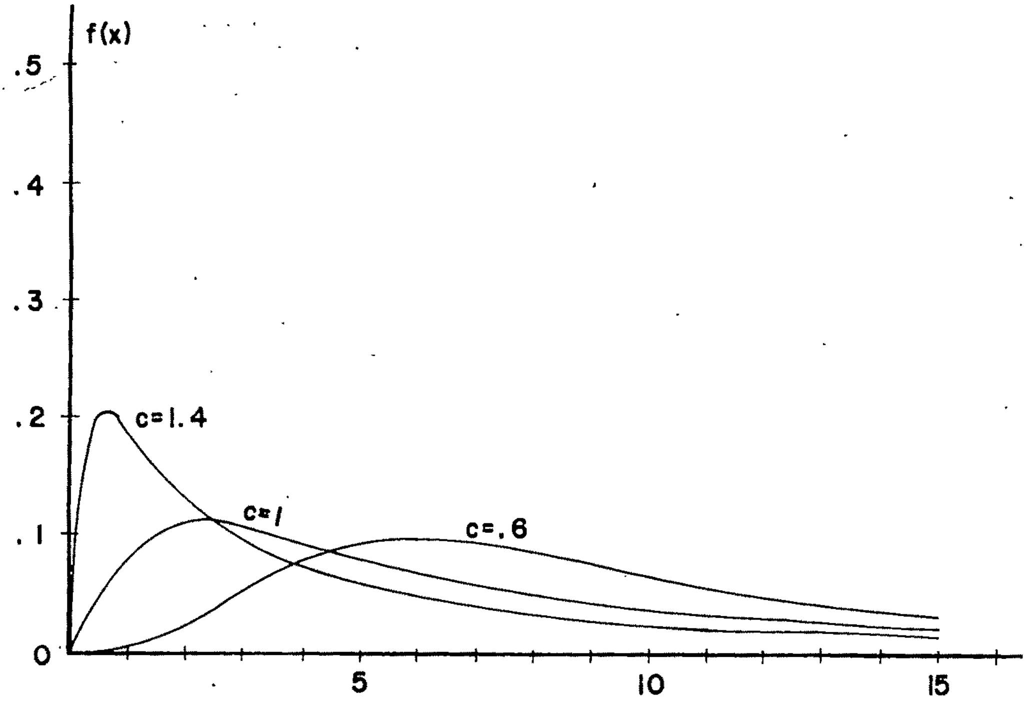
Notice that the Lognormal's scale parameter b is cancelled out at this step, so that the new density is solely a function of the Lognormal's shape parameter c.

Finally, we perform a second change-of-variable in order to obtain the density of the distribution of justice evaluations in the Social Aggregate:

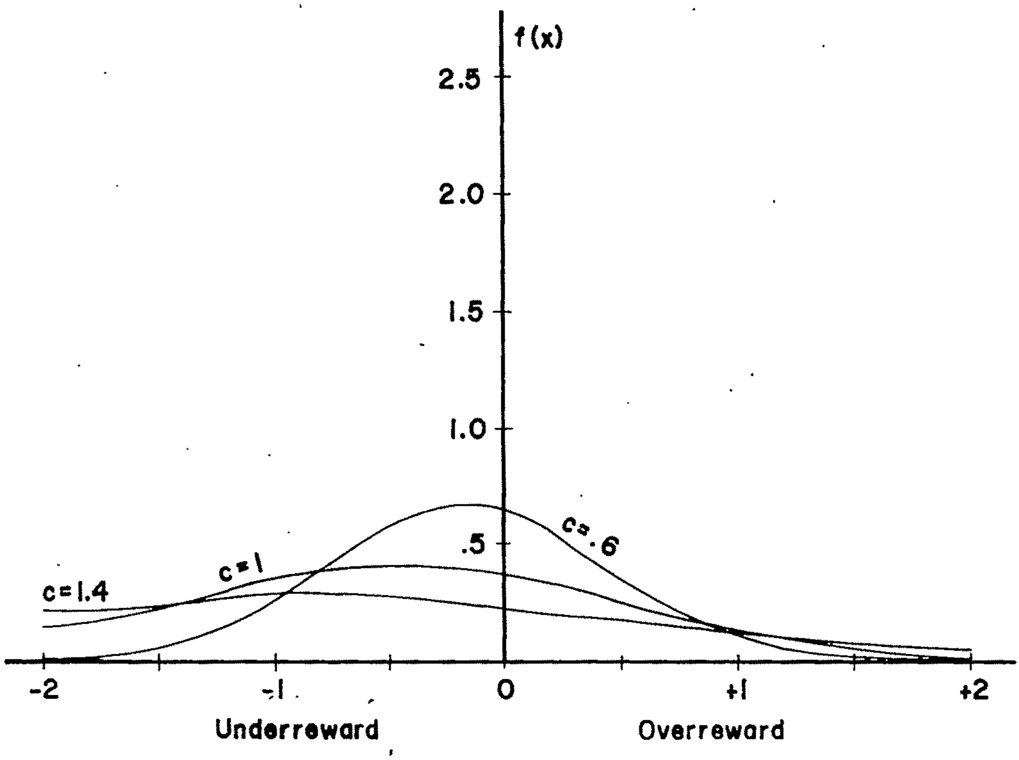
$$q(z) = \frac{e^{-(z+(c^2/2))^2/2c^2}}{c\sqrt{2\pi}}, \quad -\infty < z < +\infty \,. \quad (30)$$

This is, of course, a Normal density.

We now re-state it, using x to denote the Normal variate, a to denote the Normal's own location parameter, and b to denote the Normal's own scale parameter:



a. Lognormal Density Functions



b. Normal Density Functions

Figure 3. Distributions of Lognormal Quantity-Good and of Justice Evaluations

$$f(x) = \frac{e^{-(x-a)^2/2b^2}}{b\sqrt{2\pi}}, \quad -\infty < x < +\infty . \qquad (31)$$

In the Normal family the arithmetic mean is identical to the location parameter *a*, and the standard deviation is identical to the scale parameter *b*. Comparing the two expressions above, we see that the Normal's scale parameter *b* is identical to the Lognormal's shape parameter *c* and that the Normal's location parameter *a* is also a function of the Lognormal's shape parameter *c* (Normal's a = Lognormal's $-c^2/2$). Thus, we can re-state the density function again, this time expressing the new Normal in terms of one basic parameter:

$$f(x) = \frac{e^{-(x+(b^2/2))^2/2b^2}}{b\sqrt{2\pi}}, \quad -\infty < x < +\infty . \qquad (32)$$

Graphs of these Normal densities are shown on panel b. of Figure 3.

Thus we see that the Lognormal's shape parameter *c* determines both the mean and the standard deviation of the Normal distribution of justice evaluations that results from consideration of one Lognormally distributed quantity-good.

The Lognormal's shape parameter *c* may be regarded loosely as a measure of inequality and hence we may say that as inequality in the distribution of the quantity-good increases, the location of the distribution of justice evaluations moves leftward and inequality in justice sentiments increases as well.

These characteristics indicate that whenever the members of a Social Aggregate consensually value one Lognormally distributed quantity-good, the distribution of the sentiment of justice will be Normal with a negative mean and hence with more than half of the members experiencing unjust underreward. In the Normal, both tails go to infinity; thus, there are no limits to the experience of either extreme of injustice.

We pause to consider together the three curves we have produced. When all the members of a Social Aggregate consensually value one quality-good and accept equality as the Just term, "everything" is fully determined "inside" the theory. In such case, the distribution of Actual Ranks is Rectangular and the distribution of justice evaluations is a negatively skewed Exponential. But when the good of value is a quantity-good, then the theory—alone—cannot produce a distribution of justice sentiments; rather, "everything" depends on the actual distribution of the good.

We have considered only two families of curves as distributions of the one quantity-good. For these two, the Lognormal and the Pareto, we note that, though they behave differently in some respects, they have in common some important properties. If we hold constant the quantity-good's arithmetic mean (that is, the true arithmetic mean of the distribution of raw amounts), then we observe that: As inequality in the quantity-good's distribution decreases (with a decrease in inequality crudely indicated by an increase in Pareto's constant and by a decrease in the Lognormal's shape parameter *c*), the quantity-good distribution's median and mode move rightward toward the mean; and the proportion to the left of the mean decreases.

But of far greater importance is the effect of the inequality in the quantity-good's distribution on the distribution of justice evaluations. Regardless of the quantity-good's arithmetic mean, as inequality in the quantity-good's distribution decreases (again with a decrease in inequality crudely indicated by an increase in Pareto's constant and by a decrease in the Lognormal's shape parameter *c*), the distribution of justice evaluations moves rightward; the mean, median, and mode move toward zero; the proportion unjustly underrewarded decreases; the distribution contracts and grows taller, exhibiting a tendency toward a Dirac's Delta distribution, as defined by:[11]

$$\delta(x) = \begin{cases} 0 & , x \neq 0 \\ +\infty & , x = 0 \end{cases} . \qquad (33)$$

---

[11] Since Dirac's Delta function has the property that the integral is equal to +1, that is,

$$\int_{-\infty}^{\infty} \delta(x)\, dx = 1$$

p 17231

Thus, given consensual valuation of a quantity-good, the present theory clearly and unequivocally asserts the primacy of the degree of inequality in the quantity-good's distribution for determination of a host of social variables. The aggregate amount of the good is irrelevant; only the amount of inequality in its distribution is important. This vividly suggests that poorer societies can be socially better-off than richer ones, and specifies the condition under which this may be so.

(b) Evaluating the distributional parameters of interest

We have described the procedure for generating expected distributions and are now ready to evaluate their parameters. We shall continue working with the same distributions we generated in the preceding section.

Recall now that the parameters we are interested in include the overall arithmetic mean of the distribution of justice evaluations, the upper and lower limits, the Gini's mean difference, the proportions in various segments, and the arithmetic means of the various segments. (Although these are the only terms we evaluate in this paper, we note that other aspects of the distribution of justice evaluations may operate as independent variables in the specification of social phenomena.)

For each of these terms, we first provide the general, distribution-independent expression. Now, when the good is one quality-good, each term has one precise value; but when the good is one quantity-good, each term has a distinctive analytic solution (depending on the quantity-good's distribution), as well as a range of possible numerical values (depending on the degree of inequality in the quantity-good's distribution). Hence, for each term, we also provide the analytic solutions corresponding to the two families of curves we have obtained, as well as their theoretical limits as inequality varies. Finally, we provide numerical estimates for each term for seven curves, the negatively skewed Exponential that arises from consensual valuation of one quality-good, three positively skewed Exponentials, each arising from consensual valuation of one Pareto-distributed good of distinct Pareto's constant *c*, and three Normals, each arising from consensual valuation of one Lognormally distributed good of distinct shape parameter *c*.

All terms are obtained by evaluation of the appropriate integral. In some cases, the analytic solutions are well known (for example, the Gini's mean difference of the Normal curve). In other cases, we obtained the analytic solutions ourselves (for example, the arithmetic mean of the underrewarded segment of the positively skewed Exponential). A few of the terms have no analytic solution (notably some of the area terms of the Normal curve) so, for these, we used tabled values to approximate numerical solutions.

Table 1 reports most of the terms we have obtained for the curves discussed in this paper. Because space limitations preclude verbal description of all terms for all the curves, we will describe in words only the terms belonging to the positively skewed Exponentials that result from consensual valuation of one Pareto-distributed quantity-good. Of course, Table 1 provides all information for all the terms of all seven curves discussed in this paper.

◆

Here now is our summary description of aspects of the positively skewed Exponential family of distributions of justice evaluations:

(1) the lower limit—The lower limit of the positively skewed Exponential is equal to its location parameter *a*. We restricted Pareto's constant to values greater than unity and showed that the Exponential's location parameter *a* is an increasing function of the Pareto's shape parameter *c*. The lower limit of the distribution of justice evaluations, thus, is a function of *c*,

$$\text{lower limit} = \ln\frac{c-1}{c}. \qquad (34)$$

(see Zeldovich, 1973:423–5), and since the value of the function is always greater than or equal to zero, it satisfies the requirements for a probability density function, and we thus use it as a probability distribution. It is a perfect mathematical representation of an Equal distribution and (in this context) of a Social Aggregate all of whose members experience perfect justice.

Table 1. Principal Parameters of Some Distributions of Justice Evaluations

	Lower Limit	Arithmetic Mean	Gini's Mean Difference	Percent in Underrewarded Segment	Mean of Underrewarded Segment	Percent in Overrewarded Segment	Mean of Overrewarded Segment	Upper Limit
Distribution-Independent Expression		$_{-\infty}\int^{\infty} x\ f(x)\ dx$	$\int\int \lvert x_1 - x_2 \rvert f(x_1) f(x_2) dx_1 dx_2$	$F(0) = {}_{-\infty}\int^{0} f(x)dx$	$\frac{_{-\infty}\int^{0} x\ f(x)\ dx}{F(0)}$	$S(0) = {}_{0}\int^{\infty} f(x)\ dx$	$\frac{_{0}\int^{\infty} x\ f(x)\ dx}{S(0)}$	
A. Negatively Skewed Exponential Distribution of Justice Evaluations That Results from Consensual Valuation of One (Rectangularly Distributed) Quality-Good								
Numerical Value	$-\infty$	$(\ln 2 - 1) = .3069$	1	50.00	$-1.$	50.00	.3862	.6931
B. Family of Positively Skewed Exponential Distributions of Justice Evaluations That Results from Consensual Valuation of One Pareto-Distributed Quantity-Good								
Analytic Solution Expressed in Terms of Pareto's Constant c	$\ln \frac{c-1}{c}$	$\left(\ln \frac{c-1}{c}\right) + \frac{1}{c}$	$\frac{1}{c}$	$1 - \left(\frac{c-1}{c}\right)^c$	$\frac{\ln((c-)/c)}{1-((c-)/c)^c} + \frac{1}{c}$	$\left(\frac{c-1}{c}\right)^c$	$\frac{1}{c}$	
Limit As c Approaches 1 From the Right	$-\infty$	$-\infty$	1.0	100.0	$-\infty$	0	1.0	$+\infty$
Value When $c = 1.5$	-1.0986	$-.4319$	.6667	80.75	$-.6938$	19.25	.6667	$+\infty$
Value When $c = 2$	$-.6931$	$-.1931$	.5	75.	$-.4242$	25.	.5	$+\infty$
Value When $c = 2.5$	$-.5108$	$-.1108$	.4	72.11	$-.3084$	27.87	.4	$+\infty$
Limit As c Goes To Infinity	0	0	0	100 (1 − 1/e)	0	100 (1/e)	.0	0
C. Family of Normal Distributions of Justice Evaluations That Result from Consenual Valuation of One Lognormally Distributed Quantity-Good								
Analytic Solution Expressed in Terms of the Lognormal's Shape Parameter c		$\frac{-c^2}{2}$	$\frac{2c}{\sqrt{\pi}}$	$\frac{(1)}{\sqrt{2\pi}} {}_{-\infty}\int^{c/2} e^{-t2/2}dt$	$\frac{-c^2}{2} - \frac{ce^{-c2/8}/\sqrt{2\pi}}{F(0)}$	$\frac{(1)}{\sqrt{2\pi}} {}_{c/2}\int^{\infty} e^{-t2/2}dt$	$\frac{-c^2}{2} - \frac{ce^{-c2/8}/\sqrt{2\pi}}{S(0)}$	
Limit As c Goes To Infinity	$-\infty$	$-\infty$	∞	100.00	$-\infty$	00.00	$+\infty$	$+\infty$
Value When $c = 1.4$	$-\infty$	$-.98$	1.5979	75.80	-1.5567	24.20	.8267	$+\infty$
Value When $c = 1.0$	$-\infty$	$-.5$	1.1284	69.15	-1.0092	30.85	.6411	$+\infty$
Value When $c = 0.6$	$-\infty$	$-.18$	.6770	61.79	-0.5503	38.21	.4189	$+\infty$
Limit As c Approaches Zero From the Right	0	0	0	50.00	0	50.00	0	0

Note to Parts B. and C.:

By expressing the parameters of the distribution of justice evaluations as functions of the quantity-good's distribution, we are able to see the effects of inequality in the quantity-good's distribution. Notice that in the Pareto family of curves inequality increases as the value of Pareto's constant grows smaller, while in the Lognormal family inequality increases as the value of the shape parameter c grows larger.

The limits of *a* (that is, of the lower limit) are $-\infty$ as Pareto's constant approaches 1 from the right and zero as Pareto's constant goes to positive infinity. This means that, whenever the quantity-good is Pareto-distributed, the lower limit of the distribution of justice evaluations will be a negative quantity. (Of course, we already knew this, since, in the prior section *1.(a)*, we showed that the lower limit of the distribution of justice evaluations resulting from consensual valuation of an unequally distributed good would always be negative.) Turning now to individual members of this family, when Pareto's constant is 1.5, the lower limit is about −1.0986; when *c* is 2, the lower limit is about −.6931; and when *c* is 2.5, the lower limit is about −.5108.

(2) the upper limit—The upper limit of this family, like that of the underlying Pareto family, tends to infinity. However, its limit, as Pareto's constant goes to infinity, approaches zero from the right.

(3) the arithmetic mean—The arithmetic mean of the family of positively skewed Exponentials is given by $(a+b)$; it is thus a function of the Exponential's two basic parameters. However, recall that both the Exponential's basic parameters are functions of Pareto's constant, so that we may express the arithmetic mean as a function of the shape parameter *c* of the underlying Pareto variate. Thus, the Exponential's arithmetic mean is an increasing function of Pareto's constant, the formula given by

$$\text{arithmetic mean} = \ln\frac{c-1}{c} + \frac{1}{c}. \quad (35)$$

The limits of the arithmetic mean are negative infinity, as Pareto's constant approaches 1 from the right, and zero, as *c* goes to positive infinity. Notice that we could have arrived at this same result by using the function we obtained in section *1.(c)*—namely, the arithmetic mean of the distribution of justice evaluations is the logarithm of the ratio of the good's geometric mean to the good's arithmetic mean. In the Pareto variate, the geometric mean is the exponential function of the reciprocal function of *c* (that is, exp 1/c), and the arithmetic mean is $c/(c-1)$, so that the log of the ratio is the formula given above. Some values of the arithmetic mean are as follows: When Pareto's constant is 1.5, the arithmetic mean is about −.4319; when Pareto's constant is 2, the arithmetic mean is about −.1931; and, when Pareto's constant is 2.5, the arithmetic mean is about −.1108.

(4) the Gini's mean difference—The analytic formula for the Gini's mean difference of the distribution of justice evaluations is very simple for, in this case, the Gini's mean difference is an identity function of the Exponential's scale parameter *b*. Expressed in terms of Pareto's constant, the Gini's mean difference is the reciprocal function of Pareto's constant. Its limits are zero, as Pareto's constant goes to positive infinity, and one, as *c* approaches one from the right. Turning to some values of the Gini's mean difference—when Pareto's constant is 1.5, the Gini's mean difference is 2/3; when *c* is 2, the Gini's mean difference equals one-half; and, when *c* is 2.5, the Gini's mean difference is .4.

(5) the proportion underrewarded—The proportion underrewarded is the distribution function F(x) evaluated at x=0, that is, F(0). In terms of the underlying Pareto variate, it is the distribution function evaluated at x = the arithmetic mean, that is, $F(c/(c-1))$. The equivalent formulas are $(1 - \exp(a/b))$, in terms of the Exponential's basic parameters, and $(1 - ((c-1)/c)^c)$, in terms of Pareto's constant. The proportion underrewarded is thus a decreasing function of Pareto's constant. Its limit as Pareto's constant goes to infinity is the quantity one minus the reciprocal function of *e* (the base of the natural logarithms), that is, $(1 - 1/e)$. As Pareto's constant approaches one from the right, its limit is unity. Thus, the proportion underrewarded can never be less than about 63.21%. When Pareto's constant is 1.5, the proportion underrewarded is about 80.75%; when Pareto's constant is 2, the proportion underrewarded is 75%; and when Pareto's constant is 2.5, the proportion underrewarded is about 72.11%.

(6) the arithmetic mean of the underrewarded segment—This term is obtained by setting the area in the underrewarded segment equal to unity and then solving

for the first moment about the origin. In terms of the Exponential's basic parameters, the formula is

$$\text{underrewarded mean} = \frac{a + b(1 - e^{a/b})}{1 - e^{a/b}}. \quad (36)$$

In terms of Pareto's constant, the arithmetic mean of the underrewarded segment is an increasing function, defined by

$$\text{underrewarded mean} = \frac{\ln\frac{c-1}{c} + \frac{1}{c}\left(1 - \left(\frac{c-1}{c}\right)^{c}\right)}{1 - \left(\frac{c-1}{c}\right)^{c}}. \quad (37)$$

Its limits are negative infinity, as Pareto's constant approaches one from the right, and zero, as Pareto's constant goes to positive infinity. Turning to some specific values, when Pareto's constant is 1.5, the mean of the underrewarded segment is about −.6938; when c is 2, the mean of the underrewarded segment is about −.4242; and when c is 2.5, the mean of the underrewarded segment is about −.3084.

(7) the proportion overrewarded—The proportion overrewarded is the survival function S(x), evaluated at x=0, that is, S(0). In terms of the underlying Pareto variate, this term is the survival function evaluated at x equal the arithmetic mean, that is, S(c/(c−1)). The equivalent formulas are (exp a/b), in terms of the Exponential's basic parameters, and $((c-1)/c)^c$, in terms of Pareto's constant. Thus, the proportion overrewarded is an increasing function of Pareto's constant. Its limits are zero, as c approaches one from the right, and (1/*e*), as c goes to positive infinity. Thus, the proportion overrewarded is never greater than about 36.79%. Some values are as follows: When Pareto's constant is 1.5, the proportion overrewarded is about 19.25%; when c is 2, the proportion overrewarded is 25%; and when c is 2.5, the proportion overrewarded is about 27.87%.

(8) the arithmetic mean of the overrewarded segment—This term is obtained by setting the area in the overrewarded segment equal to unity and then solving for the first moment about the origin. In this case, this term has a simple formula; it is an identity function of the Exponential's scale parameter *b*. Alternatively, it is the reciprocal function of Pareto's constant. It is, thus, a decreasing function of c. Notice that, in this case, the arithmetic mean of the overrewarded segment is identical to the Gini's mean difference. Like the Gini's mean difference, its limits are zero, as Pareto's constant approaches positive infinity, and one, as Pareto's constant goes to one from the right. Turning to specific values, when Pareto's constant is 1.5, the arithmetic mean of the overrewarded segment is equal to two-thirds; when c is 2, the mean is .5; and when c is 2.5, the mean is .4.

There are in addition to these eight parameters many others that may be important in specifications of dependent variables. In this paper we have mentioned what we call the Distressed segment. When we first obtained the Exponentials discussed in this paper (and a Gamma family of curves not discussed in this paper), we were struck by the possibility of a Distressed segment and a Charmed segment in the left and right tails of negatively skewed and positively skewed curves, respectively. We defined a Distressed segment as the segment bounded by the lower limit and the negative fractile whose absolute value equals the upper limit; and we defined a Charmed segment as the segment bounded by the upper limit and by the positive fractile that equals the absolute value of the lower limit. And we conjectured, among other things, that crime exists only when there is a Distressed segment and that the crime rate varies directly with the proportion in the Distressed segment.

We still find much intuitive appeal in that notion and intend to test it, but we are unsure about its correct extension to Normal curves. We suspect that there exists a general rule for defining Distressed segments, of which our rule based on the bounds is a special case. However, we have not yet found it. So we provisionally apply our rule to the Normal curves, but use observed upper and lower extreme values instead of upper and lower limits to define the Distressed segment.

The negatively skewed Exponential that results from consensual valuation of one quality-good has a Distressed segment containing 25% of the population. The positively skewed Exponentials that result from consensual valuation of one Pareto-distributed quantity-good have no Distressed segment; instead they have a Charmed segment whose size varies directly with Pareto's constant, growing to an upper limit of $(1/e^2)$, or about 13.53%. Thus, we are led to the conjecture that there is very little crime, if any, in a society in which the good of value is one Pareto-distributed quantity-good and that there is a considerable amount of crime in a society in which the good of value is one quality-good.

As for the Normal curves obtained from consensual valuation of one Lognormally distributed quantity-good, if we make the simplifying assumption that all justice evaluations lie within plus or minus four standard deviations of the mean, then we find Distressed segments of such size as .03% when b equals .6, .13% when b equals one, and .47% when b equals 1.4. These are tiny Distressed segments, suggesting crime rates in these societies of much lower magnitude than in quality-good societies.

The foregoing discussion of Distressed segments underscores our view that, evaluating the parameters of the various distributions of justice evaluations—delightful exercise though it be—is not an end in itself. We do it because we are intrigued by the hypothesized relations between these parameters and social phenomena in the real world.

Thus, in terms of the theory, the information provided in Table 1 has a very precise meaning. Each of the seven rows represents a distinct Social Aggregate, and each term in each row represents one aspect of that Social Aggregate. For example, the first row of Table 1 is saying: There may be a Social Aggregate such that all the members value one and the same good, and that good is a quality-good, such as athletic skill, intelligence, social honor, or physical attractiveness; in such a Social Aggregate, half the members feel unjustly underrewarded, and the justice sentiment of the "bottom" person in the hierarchy of the good tends to negative infinity.

Likewise, each column in Table 1 is a vector of magnitudes of a variable attribute of Social Aggregates. For example, look at the column of proportion underrewarded. The expected values of this variable, according to Table 1, lie in the half-open interval, (.5,1]. The seven Social Aggregates described in Table 1 report values ranging from 50% to about 80.75%. This means that, if the good of value is a quality-good or if the good of value is one quantity-good of either Pareto or Lognormal distribution, then the proportion underrewarded in the Social Aggregate cannot be less than half.[12]

◆

Having looked in some detail at two families of quantity-good distributions—the Pareto and the Lognormal—we are now ready to state more explicitly the relation between the parameters of the distribution of justice evaluations and the magnitude of inequality in the quantity-good's distribution.

To begin with, all the major parameters of the distribution of justice evaluations are functions of the shape parameter c of the quantity-good's distribution. But they are different functions. We have already noticed that each aspect of the distribution of justice evaluations depends in its own particular way on the shape parameter c of the good's distribution, and that this particular way is specific to the distributional family. And we have noticed that the shape parameter c behaves quite differently in different distributional families. These features produce some rather interesting relations which we now summarize:

(1) Within a family of curves, the major parameters of the distribution of justice

---

[12] Of course, whenever the median of the good's distribution lies to the right of the arithmetic mean, the proportion unjustly underrewarded will be less than half. I am currently working with families of negatively skewed Beta and Gamma distributions to explore their resemblance to observed distributions of certain quantity-goods in certain Social Aggregates and to generate ensuing distributions of justice evaluations.

evaluations—though they are different functions of the shape parameter c of the good's distribution and though they differ markedly in their slopes—are, nevertheless, monotonically related to each other. Thus, if we compare any two of the Exponentials that arise from Pareto-distributed goods, whichever one is the more desirable in terms of any characteristic will be the more desirable in terms of all the other characteristics. For example: the curve with the smaller Gini's mean difference will also have the smaller absolute magnitude on the lower extreme value, will also have the smaller absolute magnitude on the arithmetic mean, will also have the smaller percentage in the unjust underrewarded segment, and so on.

(2) Between families of curves, major parameters of the distribution of justice evaluations are not necessarily monotonically related to each other. Thus, if we compare any two curves, one need not be uniformly more desirable than the other in terms of all characteristics. For example, if we compare the Exponential curve that arises from a Pareto:$c = 2$ distribution with the Normal curve that arises from a Lognormal:$c = .6$ distribution (see Table 1), we find that the Normal's arithmetic mean (−.18) is of smaller absolute magnitude than the Exponential's arithmetic mean (−.1931), but that the Normal's Gini's mean difference (.6770) is greater than the Exponential's (.5).

Now, if we say that both the arithmetic mean and the Gini's mean difference of the distribution of justice evaluations are functions of the inequality in the good's distribution, then we quickly find ourselves in a logical contradiction, with opposing readings on which curve has the greater degree of inequality. One way out of this apparent contradiction is to substitute the term, "shape parameter c of the good's distribution," for the term, "inequality." For then we pay our respects, so to speak, not only to the wildly different shapes of different families of curves but also to the wildly different behaviors of their shape parameters.

But another, equally effective, way out of the apparent contradiction is to substitute the term, "several forms of inequality," for the term, "inequality." For the perspective embodied here suggests that there are many forms of inequality (not merely many measures of inequality) and that *each* form of inequality affects a different aspect of social life. That is to say, each form of inequality has for its special province some one social phenomenon. Thus, our conjecture is that social discontent depends on that form of inequality represented by the logarithm of the ratio of the geometric mean to the arithmetic mean of the good's distribution, that the propensity to violent revolutionary conflict depends on that form of inequality represented by the logarithm of the ratio of the lower extreme value to the arithmetic mean of the good's distribution, that political instability depends on that form of inequality represented by whatever distribution-dependent function of the shape parameter c of the good's distribution produces the Gini's mean difference of the distribution of justice evaluations, and so on. We know that these forms of inequality are indeed forms of inequality because their values would all be zero if the underlying distribution of the good were a Delta (or Equal) distribution.

Put differently, whatever parameter of the distribution of justice evaluations one is interested in minimizing (that is, whatever form of inequality one is interested in minimizing)—whether it be the magnitude of unjust underreward experienced by the "bottom" person (the Rawlsian criterion) or the mean experience of justice or injustice (the Benthamite criterion), or any other—if all the curves under review belong to the same distributional family, then there is but one uniform counsel: For distributions of justice evaluations that arise from Pareto-distributed goods, the curve with the higher value of Pareto's constant is the curve with the minimum values on all parameters of interest (that is, minimum values on all forms of inequality); for distributions of justice evaluations that arise from Lognormally distributed goods, the curve with the smaller value of the shape parameter c is the curve with the minimum values on all parameters of interest (that is, minimum values on all forms of inequality).

No easy solution, however, exists when

the curves under review belong to different distributional families. The curve with the best-off "bottom" person may have the largest percentage experiencing unjust underreward, as is the case with some pairwise comparisons from Table 1. In such cases, choice of a criterion parameter—that is, choice of the particular form of inequality to be minimized—becomes the paramount policy decision.[13]

◆

D. *A Note on Testing*

We now indicate the requirements for empirical testing of the specifications we have proposed.

1. Measurement and specification of the actual and Just terms.

The general theory asserts a measurement strategy and the Primitive model offers two probabilistic propositions. We now look at them.

(a) The measurement strategy

We suggested that individuals generally measure their Actual and Just terms as ranks if the good of value is a nonadditive, nontransferable quality-good and measure them in the good's own units if the good is an additive, transferable quantity-good. Thus, the testing task is to demonstrate that, when individuals evaluate the justice or injustice of their holdings of goods, the magnitude of their justice evaluation implies measurement of the terms in the way described here. Direct observation of individuals is required.

(b) The proposition on consensus about the good of value

The Primitive restriction that the members of a Social Aggregate value the same good(s) is a probabilistic proposition that states: Given a Social Aggregate, the majority of the members ($n > .5N$) value the same good or the same combination of goods. Direct observation of individuals is required.

(c) The proposition that justice is equality

The Primitive restriction that justice is equality is a probabilistic proposition that states: Given a Social Aggregate, given any good of distribution, the frequency distribution of Just terms held by the members has for both its arithmetic mean and its mode the arithmetic mean of the distribution of the good. Direct observation of individuals is required.

2. Specification of the justice evaluation function.

We have provisionally accepted the function we proposed earlier (Jasso; 1978). However, additional research in many different settings and about many different goods is needed. Testing of this function requires direct observation of individuals.

3. Specification of the behavioral consequences of the justice evaluation.

We suggested that social summary measures of individual-level phenomena as well as specifically social phenomena are functionally dependent on parameters of the distribution of justice evaluations. Tests of hypotheses, using the general theory, require observed frequency distributions of justice evaluations and, hence, data collected from individuals. Tests of hypotheses, using the Primitive version of the theory, require knowledge of the good or goods of value, their density functions, and their intercorrelations, and hence may be carried out *entirely* by means of documentary analysis and secondary analysis of existing data sets. Hypotheses should be tested both on cross-sectional data (i.e., on a sample of Social Aggregates observed at one point in time) and on time-series data (i.e., on observations within one Social Aggregate at different points in time). The central testing task is curve-fitting—that is, finding the equation for the best approximation to the relationship between a social phenomenon and a parameter of the distribution of justice evaluations—with this task repeated for as many specifications of as many dependent variables as one may like.

III. SUMMARY AND DISCUSSION

According to the theory of distributive justice here proposed, the natural and social goods have exogenously given distributions, and the individual makes exogenous selections of Goods to value, of Social Aggregates to form, and of the holding to consider Just for him/herself. The general theory makes few claims, but these claims are universal, applying uniformly to all humans: Briefly, the mathematical representation of self-appraisal about one's holdings of goods and of judgments about the just holdings of goods differs according to whether the good is a quality or a quantity, being ranks for the former and amounts for the latter; the experience of justice or injustice varies as the logarithm of the ratio of the actual holding to the just holding; this sense of justice is the primordial independent variable which determines variation in phenomena observable in individuals and in Social Aggregates.

---

[13] Choice of the form of inequality to minimize may indeed be the major question confronting many contemporary societies. Is it better to seek a high minimum in the distribution of wealth or of income, or to seek a smaller percentage below the arithmetic mean, or to seek a larger geometric mean? I am currently systematizing these alternatives and exploring the trade-offs in alternative redistribution strategies.

According to the theory, the social life may be regarded as basically a meeting of justice sentiments, and social variables may be explained and predicted by aspects of the distribution of justice evaluations. We offered several hypotheses—among them: that the Social Aggregate's propensity to violent revolutionary conflict varies with the absolute magnitude of the lower extreme value of the justice distribution; that the intensity of revolutionary violence varies with the higher of the two warring segments' arithmetic means; that the propensity to dissolution varies with the Gini's mean difference; and that social content/discontent varies with the overall arithmetic mean.

Variation in the social phenomena, both cross-sectionally and intertemporally, is attributable to differences in the distributions of justice evaluations. Due to the exogeneity of the actual distributions of goods and of the individuals' choices, one distribution of justice evaluations cannot be predicted from a prior one.

The sentiment of distributive justice bridges the gap between individualistic and collectivistic explanation; for it is experienced by the individual and measurable in the individual but, unlike hunger or thirst or fatigue, cannot even occur except by reference to the distribution of a good among a collection of humans. And though, according to the theory, we could predict all social phenomena of interest from an observed frequency distribution of justice evaluations, the explanatory terms used in the propositions would not be limited to simple aggregations of the individuals' scores (such as the arithmetic mean), but, rather, would also include other less simple attributes of the distribution, such as the Gini's mean difference. Conversely, prediction of the social phenomena is never possible solely from the actual distribution of goods, for the individuals' exogenous choices mediate the sense of justice; even in the Primitive version, the choice of goods must be ascertained before we can move from the good's distribution to the social variables. Thus, we cannot regard objective deprivation as synonymous with the experience of unjust underreward; the individual must make certain choices before he or she can experience unjust underreward.

The general theory is innocent of human choices and of the actual distributions of goods. And so it is possible for all members to feel justly treated or even for all to feel overrewarded. This is because they may value different goods or because they hold different notions of the just condition. An example of the former sort of Social Aggregate is found in Don Quijote's description of the eternal company of knights-errant (in the conversation which opens the first chapter of Part II); he names fourteen knights-errant, stating for each the attribute in whose distribution he ranks first; thus, the most courteous is Ruggiero, the most fearless is Rodamante, the boldest is Rinaldo, and so on. Examples of the second sort of Social Aggregate include medieval societies, the so-called "traditional" marriage, Augustine's ([413–426] 1952, *City of God,* Book 22, chap. 30) description of heaven, as well as Social Aggregates which follow the Marx-Blanc-Morelly (Marx [1875] 1959, *Critique of the Gotha Program*) or the koinonial (*The Holy Bible,* "The Acts" 2:44–5 and 4:34–5) prescriptions for the distribution of material goods.

But the Primitive version of the theory—by asserting consensus on the goods of value as well as the identity of equality and justice—extinguishes all possibilities, save one, for a Social Aggregate in which no one feels unjustly underrewarded. That remaining possibility is of an equal distribution of the good. Any departure from perfect equality produces a distribution of justice evaluations which includes some sentiment of underreward and some sentiment of overreward—and produces, as well, whatever empirical consequences follow from the parameters of the justice evaluation distribution.

In the Primitive model, the Social Aggregate is given and so all begins with the consensual choice of the good of value. If the good is a quality-good, then "everything" is determined "inside" the theory, and the principal independent variable is the size N of the SA; if the good is a quantity-good, then "everything" is determined "outside" the theory by the good's actual distribution, and the principal independent variable is the good's dispersion.

In the case of a quality-good, the good's density is fixed as Rectangular (or Discrete Uniform, for not-so-large Social Aggregates). For large N tending to infinity, the effects of N are obscured—most parameters, except the lower extreme value, quickly approaching the neighborhood of their limits. But for smaller sized Social Aggregates, the size N is a crucial determinant of the parameters of the distribution of justice evaluations. As N increases, the absolute value of the overall arithmetic mean increases as does the value of the Gini's mean difference—and with them, we hypothesize, the magnitudes of social discontent and of the propensity to dissolution. In the determination of the proportion underrewarded and of the mean of the underrewarded segment, we find a special attentiveness to whether N is odd or even—the proportion underrewarded being a nondecreasing function of N, and the mean of the underrewarded segment varying as two distinct decreasing sequences, one on the set of even integers, the other on the set of odd integers (both converging to a limit of $\ln 1/e$ or -1), so that given any two adjacent integers, the odd-sized Social Aggregate has the larger absolute mean of the underrewarded segment.

In the case of a quantity-good, the crucial determinant of social variables is the inequality in the good's distribution (quantified in the Pareto and Lognormal families as the shape parameter c). General inequality is not comparable across distributional family, though particular manifestations of inequality are. Thus, we show that, within distributional family, all social variables vary with the shape parameter c; while across distributional family, one particular distribution may have a greater magnitude of, say, the Gini's mean difference (and, we hypothesize, of political instability) than a second; but the second may have a larger absolute magnitude of, say, the lower extreme value (and, we hypothesize, of the propensity to revolution) than the first.

Put differently, in the Primitive world, selection of a quality-good is the scope condition for the operation and activity of the size N of the Social Aggregate, and selection of a quantity-good is the scope condition for the operation and activity of the quantity-good's actual distribution. In both cases, the "true" explanatory variables are parameters of the justice distribution. However, in the quality-good case, the size N can be used as proxy independent variable, and in the quantity-good case, a measure of inequality can be so used, provided that all cases in the analysis belong to the same distributional family. Ignoring these conditions produces specification error and obscures the underlying relations. Of course, such specification error can be averted by using the appropriate terms from the justice distribution, rather than N or c.

One major task in developing this theory is precisely to expand the matrix depicted in Table 1 by evaluating the parameters of interest in many more distributional families. We are currently adding rows to the matrix—for example, one set for the Asymmetrical Laplace family of curves that results from consensual valuation of one quality-good and one Pareto-distributed quantity-good and another set for the negatively skewed Gamma family of curves that results from consensual valuation of two or more quality-goods. And we are adding columns to the matrix—for example, one for the mean of the special function defined as the arc secant of the justice evaluation, for justice evaluations less than or equal to −1, or greater than or equal to +1, and zero elsewhere.

The Primitive model may be viewed as a quantitative representation of some of Hobbes's [1651] 1952 *Leviathan*, chaps. 13–5,17) brilliant insights—for example: that "equal distribution is the law of nature"; that "when all is reckoned together the difference between man and man is not so considerable as that one man can thereupon claim to himself any benefit to which another may not pretend as well as he"; that "if any two men desire the same thing, which nevertheless they cannot both enjoy, they become enemies"; and that "men are continually in competition for honour and dignity . . .; and consequently amongst men there ariseth on that ground, envy, and hatred, and finally war."

In terms of the Primitive model, if the distribution of the good of value is unequal, consensus breeds scarcity. It is precisely this consensus on the goods of value that engenders the competition about which Hobbes writes.

Although Hobbes depicted powerfully the state of human society when the good of value is a quality-good, and anticipated, as well, some features of human society when the good of value is a quantity-good, it was Marx who brilliantly explored the personal and social consequences of alternative distributions of material resources (e.g., Marx [1859] 1913, *A Contribution to the Critique of Political Economy*). It was Marx whose writings compelled us to think about human sentiments and social relations when "everything" depends on the actual distribution of a quantity-good. Thus—and simplifying greatly—we may regard Hobbes as the prophet of life in a quality-good regime and Marx as the prophet of life in a quantity-good regime.

Perhaps the most striking result of this theory concerns the cross-distributional consequences of economic inequality. Across families of probability distributions, the distribution with the smaller value on one manifestation of inequality may have the larger value on another manifestation of inequality. Thus, actions to decrease one form of inequality may serve to increase another. And, since each form of inequality is viewed as exercising hegemony over a special area of social life, mitigating one consequence of inequality may serve to exacerbate another. Thus, the paramount policy decisions center on the choice of the form of inequality to minimize.

But now we must turn from the task of formulating the theory to the task of testing it. Now we must look at the many conjectures we have described and ask how faithful a likeness they are to the social reality we wish to approximate.

REFERENCES

Aristotle
[384–322 B.C.]
1952 The Works of Aristotle. Two Volumes. Trans. W. D. Ross. Chicago: Britannica.

Augustine, Saint
[413–426] 1952 The City of God. Trans. Marcus Dods. Chicago: Britannica.

Bentham, Jeremy
[1802] 1950 The Theory of Legislation. London: Routledge and Kegan Paul.

Berger, Joseph, Morris Zelditch, Bo Anderson, and Bernard P. Cohen
1972 "Structural aspects of distributive justice: a status-value formulation." Pp. 119–46 in J. Berger, M. Zelditch, Jr., and B. Anderson (eds.), Sociological Theories in Progress, Vol. 2. Boston: Houghton Mifflin.

Campbell, Norman
[1921] 1952 What Is Science? New York: Dover.

Cook, Karen
1975 "Expectations, evaluations and equity." American Sociological Review 40:372–88.

Cramer, J. S.
1971 Empirical Econometrics. Amsterdam: North-Holland.

Festinger, Leon
1954 "A theory of social comparison processes." Human Relations 7:117–40.

Freud, Sigmund
[1929] 1952 "Civilization and its discontents." Trans. Joan Riviere. Pp. 767–802 in The Major Works of Sigmund Freud. Chicago: Britannica.

Hastings, N. A. J. and J. B. Peacock
1974 Statistical Distributions: A Handbook for Students and Practitioners. London: Butterworth.

Hobbes, Thomas
[1651] 1952 Leviathan. Nelle Fuller (ed.). Chicago: Britannica.

Hoel, Paul G.
1971 Introduction to Mathematical Statistics. 4th Ed. New York: Wiley.

Homans, George Caspar
1967 "Fundamental social processes." Pp.

27–78 in N. Smelser (ed.), Sociology: An Introduction. New York: Wiley.

1974 Social Behavior: Its Elementary Forms. Rev. ed. New York: Harcourt, Brace, Jovanovich.

Hume, David

[1777] 1965 Hume's Ethical Writings. A. MacIntyre (ed.) London: Collier-Macmillan.

Hyman, Herbert H.

1942 "The psychology of status." Archives of Psychology No. 269.

1966 "Reference groups." Pp. 353–61 in David Sills (ed.), International Encyclopedia of the Social Sciences, Vol. 13.

James, William

[1891] 1952 The Principles of Psychology. Chicago: Britannica.

Jasso, Guillermina

1978 "On the justice of earnings: a new specification of the justice evaluation function." American Journal of Sociology 83:1398–1419.

Jasso, Guillermina, and Peter H. Rossi

1977 "Distributive justice and earned income." American Sociological Review 42:639–51.

Johnson, Norman L., and Samuel Kotz

1970a Distributions in Statistics: Continuous Univariate Distributions—1. Boston: Houghton Mifflin.

1970b Distributions in Statistics: Continuous Univariate Distributions—2. Boston: Houghton Mifflin.

Kelley, Harold H.

1952 "Two functions of reference groups." Pp. 410–4 in G. Swanson, T. M. Newcomb, and E. L. Hartley (eds.), Readings in Social Psychology. New York: Holt.

Marx, Karl

[1859] 1913 A Contribution to the Critique of Political Economy. Chicago: Kerr.

[1875] 1959 "Critique of the Gotha Program." Pp. 112–32 in L. S. Feuer (ed.), Marx and Engels: Basic Writings on Politics and Philosophy. Garden City: Doubleday Anchor.

Merton, Robert K.

1957 "Continuities in the theory of reference groups and social structure." Pp. 281–386 in Social Theory and Social Structure. New York: Free Press.

Merton, Robert K., and Alice S. Rossi

1950 "Contributions to the theory of reference group behavior." Pp. 40–105 in R. K. Merton and P. Lazarsfeld (eds.), Continuities in Social Research: Studies in the Scope and Method of "The American Soldier." Glencoe: Free Press.

Miller, David

1976 Social Justice. Oxford: Clarendon Press.

Nisbet, Robert

1970 The Social Bond. New York: Knopf.

Phelps, Edmund S.

1973 "Introduction." Pp. 9–31 in E. S. Phelps (ed.), Economic Justice. Baltimore: Penguin.

Plato

[429–347 B.C.]

1952 The Dialogues of Plato. Trans. Benjamin Jowett. Chicago: Britannica.

Rawls, John

1971 A Theory of Justice. Cambridge: Harvard University Press.

Robinson, Robert V., and Wendell Bell

1978 "Equality, success, and social justice in England and the United States." American Sociological Review 43:125–43.

Snedecor, George W., and William G. Cochran.

1967 Statistical Methods. 6th ed. Ames: Iowa State University Press.

Toulmin, Stephen

1953 The Philosophy of Science: An Introduction. London: Hutchinson.

Walster, Elaine, Ellen Berscheid, and G. William Walster

1976 "New directions in equity research." Pp. 1–42 in L. Berkowitz and E. Walster (eds.), Advances in Experimental Social Psychology, Vol. 9. New York: Academic Press.

Webster, Murray, Jr., and Le Roy F. Smith

1978 "Justice and revolutionary coalitions: a test of two theories." American Journal of Sociology 84:267–92.

Zeldovich, Ya. B.

1973 Higher Mathematics for Beginners: And Its Application to Physics. Trans. G. Yankovsky. Moscow: Mir.

ERRATUM

■ In the article, "White Movement to the Suburbs" (*ASR*, December, 1979), by Harvey Marshall, his Table 3, footnote 7, on page 989, should have read:

[7] The spurious component is that portion of the correlation between two endogenous variables due to their dependence on causally prior variables.

SEXUAL EMBOURGEOISMENT? SOCIAL CLASS AND SEXUAL ACTIVITY: 1938–1970*

MARTIN S. WEINBERG
Institute for Sex Research
Indiana University

COLIN J. WILLIAMS
Institute for Sex Research
Indiana University-Purdue University, Indianapolis

American Sociological Review 1980, Vol. 45 (February):33–48

Using the idea of "embourgeoisment," as it appears in stratification literature, that working-class people are increasingly adopting middle-class values and behaviors, this paper examines changes in the relationship between social class and sexual behavior over the last thirty years. Comparing data collected in 1938–63 with data collected in 1969–70, we find the following: For males, there are now similarities between the working and middle classes with regard to the incidence of various sexual activities, but differences exist for age at first experience, number of opposite sex partners, and the incidences and frequencies of adolescent sexual activities. For females, there were similarities between classes in the past, but now we find differences between the two classes regarding age at first experience, reactions to first experiences, and the incidences and frequencies of adolescent sexual activities. We have concluded that these findings offer little support for the idea that the sexual patterns of the classes have become indistinct. Social class is still seen as an important determinant of sexual behavior.

There can be no doubt that our society has experienced substantial change in its macrostructure. Over the last two generations the United States has undergone momentous events such as the social dislocation produced by a world war, as well as less explosive but no less important trends such as increasing affluence, urbanization, and so on. Sociologists have long been interested in such changes—at the societal level and in their effect on individual lives. Often there is disagreement, however, on the extent and character of the presumed consequences. This is also true in the area of sexual attitudes and behavior. Thus, for example, we find Reiss (1974:311) commenting on recent social change:

> When most people have no real problem in obtaining food, clothing and shelter, and even many of the luxuries, then the stage is set for them to devote their energies to issues concerning the quality of life. . . . Our sexual customs are changing in accord with the emphasis . . . on the quality of human relationships. . . .

In contrast, Gagnon and Simon (1970:110–3) state:

> The litany of technological, scientific, economic and social changes in (our) society is nearly endless. . . . Part of all this change has been events that everyone thought must surely lead to vast changes in sexual behavior. . . . (But) the reason why there was *not* any (such) change was that many of the things that were thought to have something to do with sex didn't have anything to do with it.

Unfortunately, such issues are difficult to resolve because data are not available. Not only do we lack data that would adequately describe earlier sexual patterns, but sex research has not progressed to the extent that we have adequate data for the contemporary situation. Moreover, where such a conjunction of past and present data exists, we find that the data are not directly comparable. Nonetheless, it is often possible to chart, in a general way, the effects of major social changes. Indeed, our choice is between making use of imperfect data and hoping for additional research or providing even more speculative descriptions of the shape of social change.

This paper examines changes in the relationship between social class and sexual behavior in an attempt to elucidate the

---

* Direct all communications to: Martin S. Weinberg or Colin J. Williams; Institute for Sex Research; Indiana University; Bloomington, IN 47401

We are grateful to Paul Gebhard, Ain Haas, Linda Haas, Sue Kiefer Hammersmith, and David Knoke for their helpful suggestions.

effects of our changing class structure on sexual life over the last thirty years. Specifically, we shall be examining the "embourgeoisment" thesis with regard to sex. In its general form this states that, as a result of a rising standard of living since World War II, "the traditional dividing line between manual workers and white collar employees no longer holds because large segments of the working class now share a 'white collar' style of life and many also accept middle class values and beliefs" (Mayer, 1963:467).[1] If this is true, one might expect a convergence between middle-class and working-class patterns of sexual behavior. This is directly stated by Delora and Warren (1977:542–3):

> . . . the culture of the lower classes and of ethnic minorities is undoubtedly influenced somewhat by the mass media and by contact with middle class society through education and other social institutions. To the extent that lower class sexual behavior and attitudes differ from those of the upper classes . . ., we can expect lower class sexual interaction to continue to become increasingly like the white middle class standard.

Such a position is also taken by Rainwater and Handel (1963) who argue that the traditional working-class family has radically changed and become more like the middle-class family in the face of increasing affluence. This change, they maintain, has taken the form of the nuclear family becoming more important than the extended family with the result that the modern working-class family is characterized less and less by marital role segregation. Consequently, this greater cooperation and solidarity among spouses, as in the middle class, allegedly increases their level of intimacy and hence their sexual satisfaction. Again, then, we confront the assertion that the working class is adopting middle-class patterns of sexual interaction.[2]

Finally, the work of Schmidt (1977) divides the working class into stable and unstable levels (defined mainly on grounds of economic insecurity), and notes that there is a basic similarity between the sexual patterns of adolescents from the stable working class and the middle class.[3]

Thus, many authors suggest that not only has there been an embourgeoisment of large sections of the working class that has influenced many social attitudes and values, but that this process has also intruded into the most intimate aspects of personal life, in this case, sexual behavior. Furthermore, embourgeoisment, they assume, will proceed unabated if present trends continue so that contemporary societies will eventually develop a common sexual ethic with little or no variation in sexual patterns by social class. Wilensky (1964:180) has summarized the overall outcome succinctly:

> Thus, we may assume that the influence of mass education, the media, and the centralized state will in the long run overcome the influence of variations in work, religion, age, and locality as sources of cultural values and leisure styles and we can expect mass culture in both Europe and America to penetrate structures now more or less insulated from it.

This paper will present data on the relationship between social class and sexual patterns during two time periods in an attempt to examine to what extent embourgeoisment has taken place, and, thus, in this realm, how social classes have or have not insulated their members from intrusive social forces. That is, to what extent will our data delineate two distinct, as opposed to two similar, sexual life styles between the working and middle class?

DATA AND METHODS

For the earlier time period, we use data collected by Alfred Kinsey and his associates. The data collection began in 1938 and continued through 1963. (Approximately one-fifth of the 5,460 white male interviews were carried out after publication of the male volume in 1948 and 2% of the 5,386 white female interviews after publication of the female volume in 1953.)[4] The bulk of the data, however, is from the 1940s (94% of the male interviews and 98% of the female interviews).[5]

---

[1] Other sociologists whose names have been associated with the "embourgeoisment" thesis are Dahrendorf (1964), Mills (1959), Lenski (1963), Ginsberg and Berman (1963), Zweig (1961), and Wilensky (1964).

[2] To quote Rainwater and Handel (1963:73), " . . . the working class couples in our family planning study with less segregated conjugal-role relationships are indistinguished from the middle class couples in their patterns of sexual relations, but are quite different from working class couples who have highly segregated role relationships."

[3] However, his data do show important differences between these groups (Schmidt, 1977). Note, too, that other writers strongly assert the existence of a differentiated working class (cf., Miller, 1964; Hamilton, 1963; MacKenzie, 1973).

Although these data have been criticized for their unrepresentativeness, they do constitute the most comprehensive set of data on sexual behavior for that period.[6] Moreover, we present data from what is referred to as the Kinsey Basic Sample (Gebhard and Johnson, 1979), a sample "cleaned" of persons derived from sources with a known sexual bias, for example, prostitutes. Also absent from this sample are prisoners who, it has been criticized, bias the working-class respondents (Sagarin, 1973). We present for the first time, therefore, analysis of the Kinsey et al. data from a sample that has been greatly improved.

For the more recent time period, we rely on data collected in 1969–70 by the Institute for Sex Research in connection with a large scale study of homosexuals from the San Francisco area (Bell and Weinberg, 1978). In an effort to provide a control group for comparison with the homosexuals, a sample of 284 white males and 101 white females from San Francisco's general population was obtained through the use of probability sampling with quotas. Random probability sampling was used to select census tracts and city blocks within the chosen tracts. Quota sampling was then used within each block to provide representation for various race, age, sex, and education categories.[7]

We are, however, unable to determine the representativeness and comparability of the San Francisco and Kinsey Basic Samples. For example, we do not know how the respondents in San Francisco may differ from respondents in other locales. Confirmations of prior research, however, with regard to a variety of locales (e.g., Weinberg and Williams, 1975, which includes San Francisco and other locales in three Western countries), have made us optimistic with regard to the generalizability of sexual patterns. For example, in the above study, not only did we find the same distribution of different degrees of homosexuality, but also we found that data on sexual practices and sexual frequencies showed no appreciable fluctuations among any of the European and American locales. Similarly, sociological relationships were replicated over locales. In the case of heterosexuality, we see no prima facie reason why replications regarding relationships would also not appear. This should especially be so since our independent variable, social class, is a major influence on social behavior. Such replication is even more likely when such locales are situated in the same society and share common economic and educational institutions. As with most research, however, we look forward to replications with other samples.

The form of our analysis is as follows. First, we computed zero-order Pearsonian correlation coefficients between social class and sexual behavior separately for males and females from the Kinsey Basic Sample. An F probability of .05 was the criterion for statistical significance and an r greater than .10 was the criterion for substantive significance.

Next, we did the same thing for the San Francisco Sample. Then, we compared the correlations between the two different time periods to see which, if any, changes emerged. The z_r transformation for r was used to test for the statistical significance of the difference between the two values of r (Edwards, 1976:89–90). (We have not been able in every case to make comparisons between the two studies; for example, there sometimes are data provided by one study that were not collected in the other. These additional data, however, are often used because they are relevant to our research question and valuable in illustrating global points in our argument.) All these procedures involved whites only. To check for spurious relationships, partial correlation coefficients were run controlling for age, marital status, age at first marriage, and religiosity. Tables of means and percentages are also provided for the reader to compare more closely the nature of the relationships represented by the correlations.

---

[4] One hundred and eighty-five of the male interviews and 58 of the female interviews were carried out by Kinsey's associates after his death in 1956.

[5] In the Kinsey Basic Sample the number of white males interviewed each year from 1938–63 is as follows: 1938, 30; 1939, 429; 1940, 516; 1941, 217; 1942, 220; 1943, 132; 1944, 727; 1945, 721; 1946, 413; 1947, 471; 1948, 855; 1949, 391; 1950, 25; 1951, 23; 1952, 20; 1953, 32; 1954, 31; 1955, 16; 1956, 6; 1957, 4; 1958, 31; 1959, 18; 1960, 121; 1961, 7; 1962, 3; 1963, 1. For females the corresponding numbers are: 1938, 1; 1939, 93; 1940, 149; 1941, 82; 1942, 83; 1943, 380; 1944, 747; 1945, 1,017; 1946, 462; 1947, 336; 1948, 999; 1949, 914; 1950, 15; 1951, 11; 1952, 3; 1953, 9; 1954, 16; 1955, 8; 1956, 3; 1957, 6; 1958, 3; 1959, 7; 1960, 35; 1961, 4; 1962, 2; 1963, 1.

[6] For critiques of Kinsey et al.'s methodology, especially the sample, see Wallin (1949); Hobbs and Lambert (1948); Cochran, Mosteller and Tukey (1953).

[7] For additional details about the sample and sampling procedure, see Bell and Weinberg, 1978:37–9.

Kinsey et al. (1948) used two measures of social class in their work: occupational level and educational achievement. They found that, where both were used systematically, there was little difference in the results produced. At the same time, Kinsey et al. (1948:330–1) noted that "educational level . . . [is] the simplest and best-defined means for recognizing social levels. . . . Occupational classes are more poorly defined than educational-levels." The San Francisco study also has both measures, and we, as well, chose to make our comparisons using educational attainment as the measure of social class, principally because it produced (as for Kinsey et al.) the least ambiguous results.[8] (One source of ambiguity remains, however—the lack of a clear dividing line between the social classes. This problem exists, though, with most indicators of social class and for lay people as well as for social scientists; cf., Kinsey et al., 1948:332.) Finally, we believe, as both Delora and Warren and Wilensky have suggested, that education is a key variable through which the culture of the working class has been modified. While Kinsey et al. (1948) split their educational groups into 0–8 years, 9–12 years, and 13+ years, we divided both their and our sample into high school or less, some college, and four or more years of college. This modification was made because of changes in levels of educational attainment which produced the small N for the 0–8 group in the San Francisco Sample.

RESULTS

Males

The Kinsey Basic Sample. A generation ago, Kinsey et al. (1948) found a relationship between social level and the sexual patterns of males. One finding was that lower level males were more restricted in their sexual practices. According to Kinsey et al. (1948:369):

> The record is . . . one of more extended pre-coital play at the upper levels, and of a minimum of play at the lower levels. Many persons at the lower level consider that intromission is the essential activity and the only justifiable activity in a "normal" sexual relation.

Thus, in terms of petting behaviors (for example, manual stimulation and oral-genital contact) there was a higher incidence of these behaviors among the more educated males (Table 1A). For instance, 61% of the high school group and 69% of the some college group, but 87% of the college group had masturbated a female (r = .24***), with 56%, 59%, and 80% of these respective groups having been masturbated by a female (r = .23***); approximately a seventh of the high school group and some college group, as compared with a third of the college group had performed oral-genital contact (r = .20***); and approximately a fifth of the high school group and some college group, as compared with 37% of the college group, had received oral-genital contact (r = .18**) (Table 1B). (Unfortunately, Kinsey et al. did not systematically obtain *frequency* data for these various behaviors.) Finally, Table 1A shows that in the Kinsey et al. data social class was a marked determinant of male sexual experience but not of female.

---

[8] Both samples are biased in the direction of higher class respondents. For example, for both males and females, among the San Francisco respondents, 25% had had high school or less, 33% some college and 42% a college degree or more.

Table 1A. Zero-Order Correlations between Social Level and Incidence of Masturbation and Petting Activities

	Males				
Sexual Activity	Kinsey et al. Sample		San Francisco Sample		Z Level
Masturbation	.02	(5453)	.04	(281)	n.s.
Orgasm in Sleep	.19***	(5413)	.15**	(278)	n.s.
Masturbated Opposite Sex	.24***	(5383)	.06	(280)	**
Masturbated by Opposite Sex	.23***	(5340)	.03	(280)	*
Oral-Genital on Opposite Sex	.20***	(5338)	.08	(280)	*
Oral-Genital by Opposite Sex	.18**	(5328)	.03	(280)	**

	Females						
Sexual Activity	Kinsey et al. Sample		San Francisco Sample†		Z Level	Kinsey et al. Male-Female Z Level	San Francisco Male-Female Z Level
Masturbation	.11***	(5381)	.18*	(97)	n.s.	***	n.s.
Orgasm in Sleep	.02*	(5338)	−.06	(97)	n.s.	***	n.s.
Masturbated Opposite Sex	.06**	(5351)	.07	(99)	n.s.	***	n.s.
Masturbated by Opposite Sex	.04***	(5365)	.05	(99)	n.s.	***	n.s.
Oral-Genital on Opposite Sex	.07***	(5284)	.08	(99)	n.s.	***	n.s.
Oral-Genital by Opposite Sex	.08***	(5293)	.03	(99)	n.s.	***	n.s.

† Generally comparable to Kinsey et al. coefficients which are significant because of the extremely large N.

Note: * will indicate statistical significance at the .05 level, ** at the .01 level, and *** at the .001 level; for the Z-test column, n.s. will indicate nonsignificance.

Table 1B. Social Level and Percentage Ever Experiencing Masturbation and Petting Activities

	Males					
	Kinsey et al. Sample			San Francisco Sample		
	Educational Level					
Sexual Activity	HS	SC	C	HS	SC	C
Orgasm in Sleep†	76%(761)	93%(1615)	95%(3039)	75%(69)	81%(94)	91%(117)†
Masturbated Opposite Sex†	61 (755).	69 (1590)	87 (3040)	[no significant difference]		
Masturbated by Opposite Sex†	56 (752)	59 (1566)	80 (3024)	[no significant difference]		
Oral-Genital on Opposite Sex†	15 (744)	13 (1574)	33 (3022)	[no significant difference]		
Oral-Genital by Opposite Sex†	19 (742)	19 (1568)	37 (3020)	[no significant difference]		

	Females					
	Kinsey et al. Sample			San Francisco Sample		
	Educational Level					
Sexual Activity	HS	SC	C	HS	SC	C
Masturbation†	48% (1028)	39% (2020)	58% (2335)	40% (25)	58% (31)	63% (41)

† Only significant relationships (cf., Table 1A).

Note: HS will indicate high school education, SC, some college, and C, college.

Table 2A. Zero-Order Correlations between Social Level and Age at Initial Sexual Experiences

	Males				
Sexual Activity	Kinsey et al. Sample†		San Francisco Sample		Z Level
Masturbation	.06***	(5154)	.13*	(278)	n.s.
Orgasm in Sleep	−.09***	(4956)	−.19***	(237)	n.s.
Masturbated Opposite Sex	—		.17**	(247)	—
Masturbated by Opposite Sex	—		.22**	(245)	—
Oral-Genital on Opposite Sex	—		.14*	(221)	—
Oral-Genital by Opposite Sex	—		.21***	(232)	—
Sexual Intercourse	.32***	(3864)	.27***	(265)	n.s.

	Females						
Sexual Activity	Kinsey et al. Sample†		San Francisco Sample		Z Level	Kinsey et al. Male-Female Z Level	San Francisco Male-Female Z Level
Masturbation	.07***	(2604)	.09	(59)	n.s.	n.s.	n.s.
Orgasm in Sleep	.01	(1138)	.23	(20)	n.s.	*	n.s.
Masturbated Opposite Sex	—		.05	(82)	—	—	n.s.
Masturbated by Opposite Sex	—		.08	(85)	—	—	n.s.
Oral-Genital on Opposite Sex	—		.19*	(72)	—	—	n.s.
Oral-Genital by Opposite Sex	—		.24*	(77)	—	—	n.s.
Sexual Intercourse	.18***	(3067)	.38***	(95)	*	***	n.s.

† Dash will indicate no such data from Kinsey et al.

A second finding was that less educated males had experienced sexual intercourse at an earlier age (Table 2A). As Kinsey et al. (1948:349) note:

> College males are very slow in arriving at their first pre-marital intercourse, and a comparison . . . indicates that, on an average, they do not have their first experience until . . . [several] years after the lower level males start.

The average age at first coitus was 17.3 for the high school group, 17.8 for the some college group and 20.5 for the college group (r = .32***) (Table 2B).[9] Table 2A also shows that in the Kinsey et al. data, for males, social class is a stronger correlate of the age of first coitus and first orgasm in sleep than it is for females.

A third finding was that among those males who had premarital intercourse the less educated had more premarital coital partners than did the better educated (Table 3A). Kinsey et al. (1948:349, 383) say:

> A good many college males never have pre-marital intercourse with more than the one girl whom they subsequently marry. . . . Lower level males [on the other hand] . . . are often highly promiscuous in their choice of pre-marital partners, and there are many who have no interest in having intercourse with the same girl more than once.

Table 3B shows that the average number of premarital sexual partners was 18.9 for the high school group, 10.5 for the some college group and 9.6 for the college group (r = −.11***).

With regard to all the above results, Kinsey et al.'s (1948) explanation was that working-class males were more likely to see coitus as natural, whereas other sexual techniques were more likely to be seen as a perversion. Thus, their earlier age at first coitus, greater number of partners,

---

[9] In addition, in all classes, sexual behaviors are begun at an earlier age now than in the past. Also, the San Francisco sample shows that at all social levels, oral-genital relations are experienced initially at a later age than is coitus. (No similar data are available for Kinsey et al.)

Table 2B. Social Level and Mean Ages for Initial Sexual Experiences

	Males					
	Kinsey et al. Sample			San Francisco Sample		
	Educational Level					
Sexual Activity	HS	SC	C	HS	SC	C
Masturbation	[no significant difference]			12.3 (70)	12.7 (92)	13.3 (116)
Orgasm in Sleep	[no significant difference]			14.6 (54)	14.0 (76)	13.5 (107)
Masturbated Opposite Sex	—	—	—	17.4 (60)	17.0 (80)	19.1 (107)
Masturbated by Opposite Sex	—	—	—	17.3 (58)	17.9 (84)	19.5 (103)
Oral-Genital on Opposite Sex	—	—	—	20.4 (50)	20.1 (75)	22.1 (96)
Oral-Genital by Opposite Sex	—	—	—	18.9 (55)	18.9 (79)	21.1 (98)
Sexual Intercourse†	17.3 (461)	17.8 (960)	20.5 (2445)	16.7 (67)	17.3 (95)	18.9 (115)

	Females					
	Kinsey et al. Sample			San Francisco Sample		
	Educational Level					
Sexual Activity	HS	SC	C	HS	SC	C
Oral-Genital on Opposite Sex†	—	—	—	21.6 (16)	22.8 (25)	24.7 (31)
Oral-Genital by Opposite Sex†	—	—	—	20.0 (19)	23.0 (26)	24.2 (33)
Sexual Intercourse†	21.0 (823)	20.3 (717)	22.8 (1529)	18.1 (24)	19.4 (31)	21.5 (40)

† Only significant relationships (cf., Table 2A).

and more restricted sexual practices were characteristic of the working-class males.

The San Francisco Sample. The San Francisco results for 1969–70 clearly differ from the Kinsey et al. findings for the 1940s in that, for males, social level is no longer significantly correlated with having ever experienced particular sexual acts (Tables 1A and 1B). (One exception is nocturnal emissions, r = .15**.) Furthermore, they differ from the Kinsey et al. data in showing that social class now no longer has a differential effect on the sexual incidences of males vs. females (Table 1A).

In general, such class convergence among males, however, appears to be limited to incidence. Social level is still related to other dimensions of sexual experience. Our current data, for example,

Table 3A. Zero-Order Correlations between Social Level and Number of Opposite Sex Partners†

	Males		
Sexual Activity	Kinsey et al. Sample	San Francisco Sample	Z Level
Number of Opposite Sex Partners	−.11*** (3297)	−.19*** (284)	n.s.

	Females				
Sexual Activity	Kinsey et al. Sample	San Francisco Sample	Z Level	Kinsey et al. Male-Female Z Level	San Francisco Male-Female Level
Number of Opposite Sex Partners	.02 (1876)	.04 (101)	n.s.	***	*

† Kinsey et al. dependent variable is number of premarital coital partners. San Francisco dependent variable includes marital, extramarital, and premarital partners with regard to oral-genital activity, coitus, and masturbation.

Table 3B. Social Level and Mean Number of Opposite Sex Partners[†]

	Males					
	Kinsey et al. Sample			San Francisco Sample		
	Educational Level					
Sexual Activity	HS	SC	C	HS	SC	C
Number of Opposite Sex Partners	18.9 (389)	10.5 (830)	9.6 (1895)	12.8 (71)	13.6 (95)	7.7 (118)

[†] Kinsey et al. dependent variable is number of premarital coital partners. San Francisco dependent variable includes marital, extramarital, and premarital partners with regard to oral-genital activity, coitus, and masturbation.

show that the less educated males (viz., those who were not college graduates) are more likely to have initially experienced *all* the studied sexual behaviors (except nocturnal emissions) earlier than did better educated males (Tables 2A and 2B; comparable data are not available from Kinsey et al. for petting and oral-genital activities). Kinsey et al. (1948:349, 551) found this to be the case only for coitus (our criteria for substantive significance exclude the masturbation and nocturnal emissions' relationships—r = .06*** and −.09***, respectively). Lower level males, therefore, no longer seem to have an aversion to petting techniques and are engaging in them, as they do coitus, at a relatively early age. (Age at first marriage does not explain these findings.) Thus, in the San Francisco Sample, the average age at first coitus is 16.7 for the high school group, 17.3 for the some college group, and 18.9 for the college group (r = .27***); for the first experience of masturbating a female or being masturbated by a female, the mean ages are 17.4 and 17.3 for the high school group, 17.0 and 17.9 for the some college group, and 19.1 and 19.5 for the college group (r = .17**, r = .22**); for cunnilingus, 20.4 and 20.1 years of age for the high school group and some college group and 22.1 years of age for the college group (r = .14*); for being fellated, 18.9 for the high school group and some college group and 21.1 for the college group (r = .21***).

In addition, the San Francisco data show that less educated males still have more sexual partners—12.8 for the high school group, 13.6 for the some college group, and 7.7 for the college group (r = −.19***) (Tables 3A and 3B). (It should be noted here that compared with the Kinsey et al. data this is only an indirect comparison. Their "number of sexual partners" refers only to premarital coital partners whereas the San Francisco variable includes marital and extramarital partners in addition to premarital partners, and masturbation and oral-genital activity in addition to coitus. It is interesting, however, that even with a broader definition of "partners" and sexual behaviors a similar relationship is found.)

Finally, our current data on the incidence and frequency of male sexual activities between the ages of 14 and 17 (comparable data are not available from Kinsey et al.) show that during these years less educated males had a higher incidence and frequency of both petting and sexual intercourse than did better educated males (Tables 4A-4D).[10] For example, the high school group and some college group had four times the frequency of the college group in coital activities during these years (r = −.23***) (Table 4D). Tables 4A and 4C also show that social class had no differential effect on adolescent sexual patterns (incidence and frequency) of males vs. females.

In sum, with regard to social level, male sexual patterns show two types of convergence over the last generation or so. On the one hand, there is a *between-class convergence;* namely, working-class males are more like middle-class males in *incidence* of various sexual experiences. On the other hand, there is a *within-class*

---

[10] We did not arbitrarily choose the age range 14–17. This was part of the wording of the questions asked in the San Francisco study.

Table 4A. Zero-Order Correlations between Social Level and Incidences of Sexual Activities between the Ages of 14 and 17 (San Francisco)†

	San Francisco Sample		
Sexual Activity	Males	Females	Z Level
Necked	−.12* (284)	−.14 (100)	n.s.
Clothed Breast (Caressing/Caressed)	−.14** (293)	−.22* (100)	n.s.
Bare Breast (Caressing/Caressed)	−.17** (284)	−.14 (100)	n.s.
Touched Opposite Sex Genitals	−.13** (284)	−.00 (100)	n.s.
Genitals Touched by Opposite Sex	−.13** (283)	−.17* (100)	n.s.
Sexual Intercourse	−.23*** (284)	−.39*** (100)	n.s.

† No such data from Kinsey et al.

convergence; namely, the effects of social level assert themselves to make *age at first sexual experiences and incidence and frequency of adolescent petting behaviors* consistent with the earlier Kinsey et al. findings for coitus. In addition, we still find, as did Kinsey et al., an inverse relationship between social class and number of sexual partners.

In general, other data from San Francisco show no differences between males from different social levels in adult frequencies for specific sexual activities; in positive or negative reactions to the first or last time the respondents had various sexual experiences, or in the last age at which they engaged in the particular sexual activity.

Females

The Kinsey Basic Sample. The findings of Kinsey et al. (1953) on the incidence of various sexual behaviors of women from different social levels stand in strong contrast to what was found for men. Given our criterion for substantive significance (r > .10), there was only one significant difference—higher level females were somewhat more likely to have ever masturbated (Tables 1A and 1B).[11] However, as expressed in correlational terms, this is far from a strong relationship and not much stronger than those found for other sexual outlets. In essence, then, in the 1940s, social level did not distinguish the incidence of various sexual experiences of women. Moreover, according to Kinsey et

---

[11] The strongest relationship they found between social class and sexual activity for females was in the case of homosexuality (Kinsey et al., 1953:459)—viz., a greater incidence of homosexuality was found among the highest educational groups.

Table 4B. Social Level and Percentage Ever Experiencing Sexual Activities between the Ages of 14 and 17 (San Francisco)†

	San Francisco Sample Males		
	Educational Level		
Sexual Activity	HS	SC	C
Necked	94% (71)	89% (95)	85% (118)
Caressed Clothed Breast	90 (71)	80 (94)	75 (118)
Caressed Bare Breast	79 (71)	74 (95)	60 (118)
Touched Opposite Sex Genitals	68 (71)	67 (95)	53 (118)
Genitals Touched by Opposite Sex	56 (71)	65 (95)	43 (117)
Sexual Intercourse	58 (71)	58 (95)	31 (118)
	San Francisco Sample Females		
	Educational Level		
Sexual Activity	HS	SC	C
Clothed Breast Caressed‡	76% (25)	71% (34)	51% (41)
Genitals Touched by Opposite Sex‡	48 (25)	29 (34)	27 (41)
Sexual Intercourse‡	40 (25)	18 (34)	2 (41)

† No such data from Kinsey et al.
‡ Only significant relationships (cf., Table 4A).

Table 4C. Zero-Order Correlations between Social Level and Frequency of Sexual Activities between the Ages of 14 and 17 (San Francisco)†

	San Francisco Sample		
Sexual Activity	Males	Females	Z Level
Necked	−.11* (284)	−.15 (100)	n.s.
Clothed Breast (Caressing/Caressed)	−.18*** (283)	−.25** (100)	n.s.
Bare Breast (Caressing/Caressed)	−.18*** (284)	−.26** (100)	n.s.
Touched Opposite Sex Genitals	−.17** (284)	−.16* (100)	n.s.
Genitals Touched by Opposite Sex	−.13** (283)	−.25** (100)	n.s.
Sexual Intercourse	−.23*** (284)	−.37*** (100)	n.s.

† No such data from Kinsey et al.

al. (1953), not only is there no relationship found between social level and sexual incidence for women, but also there is none for other dimensions of sexual experience as well. (One exception which Kinsey et al. did not discuss was the earlier age for first coitus among lower level females; $r =$ .18***)[12] (Tables 2A and 2B; 3A and 3B). Kinsey et al. (1953) attributed this lack of relationship to female sexuality being less conditionable than male sexuality (cf., Robinson, 1976:102, 110) in that not only social class, but also most of the other social factors affecting male sexuality were inoperative in the case of women.[13] As Kinsey et al. (1953:649–50) say:

> In general, males are more often conditioned . . . by a greater variety of associated factors than females. . . . It is highly significant to find that there are evidences of such differences between the females and males of infra-human mammalian species, as well as between human females and males.

The San Francisco Sample. Our first set of findings for San Francisco women are similar to what Kinsey et al. (1953) found in the 1940s: viz., no significant relationship between social level and the incidence of various sexual behaviors (Table 1A). (Masturbation does, however, relate

---

[12] Reiss (1965) has also pointed this out. An additional difference that Kinsey et al. (1953:378–9) did point out, but which did not affect their conclusions, was the small positive relationship between educational level and rate of marital orgasm ($r =$ .04*).

[13] This was not quite the case, however, as religiosity and generation showed marked effects on female sexuality (cf., Robinson, 1976:102).

Table 4D. Social Level and Mean Frequency of Sexual Activities between the Ages of 14 and 17 (San Francisco)†

	San Francisco Sample Males		
	Educational Level		
Sexual Activity	HS	SC	C
Necked	23.81 (71)	24.92 (95)	17.05 (118)
Caressed Clothed Breast	14.84 (71)	12.63 (94)	8.06 (118)
Caressed Bare Breast	8.66 (71)	8.72 (95)	3.70 (118)
Touched Opposite Sex Genitals	5.90 (71)	6.62 (95)	1.95 (118)
Genitals Touched by Opposite Sex	2.95 (71)	5.60 (95)	1.27 (117)
Sexual Intercourse	4.20 (71)	4.05 (95)	.98 (118)

	San Francisco Sample Females		
	Educational Level		
Sexual Activity	HS	SC	C
Clothed Breast Caressed‡	5.60 (25)	6.05 (34)	3.15 (41)
Bare Breast Caressed‡	7.10 (25)	1.21 (34)	1.07 (41)
Touched Opposite Sex Genitals‡	1.80 (25)	.97 (34)	.78 (41)
Genitals Touched by Opposite Sex‡	4.80 (25)	1.09 (34)	.78 (41)
Sexual Intercourse‡	3.60 (25)	.71 (34)	.15 (41)

† No such data from Kinsey et al.
‡ Only significant relationships (cf., Table 4C).

Table 5A. Zero-Order Correlations between Social Level and Positive Reaction to Initial Sexual Experiences (San Francisco)†

	San Francisco Sample		
Sexual Activity	Males	Females	Z Level
Masturbation	−.07 (277)	.24* (61)	*
Orgasm in Sleep	−.06 (238)	.21 (21)	n.s.
Masturbated Opposite Sex	.13* (247)	.32*** (82)	n.s.
Masturbated by Opposite Sex	.06 (245)	−.02 (86)	n.s.
Oral-Genital on Opposite Sex	−.02 (221)	.21* (72)	n.s.
Oral-Genital by Opposite Sex	−.07 (269)	.05 (78)	n.s.
Sexual Intercourse	−.03 (269)	.18* (95)	n.s.

† No such data from Kinsey et al.

to social level as in the Kinsey et al. data.)[14]

Unlike Kinsey et al. (1953), though, our data show that social class differences in female sexuality do exist with regard to other dimensions. For example, we now find that lower level females experience oral-genital sex (fellatio, r = .19*; cunnilingus, r = .24*) and coitus (r = .38***) at an earlier age than do higher level females (Tables 2A and 2B). Kinsey et al. (1953) found this only for coitus which, as previously mentioned, they failed to discuss. We also find that there is an inverse relationship between social level and sexual incidences and frequencies between the ages of 14 and 17 (Tables 4A–4D). That is, during these teenage years, less educated females were more likely to experience petting and expecially coitus (r = −.39***), and to experience them more frequently, than did better educated females. We propose that these findings signify a broadening within-class convergence: namely, lower level females are "catching up" to lower level males—diverging from higher level females—in terms of age at first experiences and incidences and frequencies of early sexual activity.

Another finding from our current data concerns the way females reacted to their first sexual experiences. As shown in Tables 5A and 5B, where such differences are found, it is upper level women who are more likely to report positive feelings toward a variety of initial sexual experiences. Thus, for example, the strongest differences show 40% of the high school

Table 5B. Social Level and Percentages Responding Positively to Initial Sexual Experiences (San Francisco)†

	San Francisco Sample Males		
	Educational Level		
Sexual Activity	HS	SC	C
Masturbated Opposite Sex‡	82% (60)	92% (79)	93% (107)

	San Francisco Sample Females		
	Educational Level		
Sexual Activity	HS	SC	C
Masturbation‡	40% (10)	44% (18)	73% (26)
Masturbated Opposite Sex‡	19 (21)	46 (24)	51 (37)
Oral-Genital on Opposite Sex‡	68 (19)	69 (26)	76 (33)
Sexual Intercourse‡	33 (24)	58 (31)	63 (40)

† No such data from Kinsey et al.
‡ Only significant relationships (cf. Table 5A).

---

[14] As an aside, there is an overall increase in oral-genital activity for both men and women: for Kinsey et al. 19% (HS), 19% (SC), and 37% (C) of the males had been fellated at least once, while in the San Francisco Sample the respective percentages are 80, 83, and 83; for females the respective percentages of those who had ever performed fellatio were 31, 16, 34, in the Kinsey et al. data and in the San Francisco Sample 64, 74, 74.

group and 44% of the some college group as compared with 73% of the college group reacting positively to their first masturbation experience (r = .24*) and 19% of the high school group as compared with 46% of the some college group and 51% of the college group reacting positively to the first experience of masturbating a male (r = .32***). Again this might be interpreted as a broadening within-class convergence. In the Kinsey et al. (1948:367–74) sample, middle-class males, for example, responded more favorably to masturbation, petting techniques, a variety of coital positions, etc., than did working-class males. Thus, our contemporary middle-class women show similarities to the middle-class men in the 1940s in their acceptance of a larger range of sexual experiences.[15] Rainwater and Handel's (1963) hypothesis would suggest that more positive responses among middle-class females might be a result of greater role integration among men and women, married or unmarried. We should also note LeMasters's (1975) vivid evidence that working-class males prefer to be segregated from females.

In general, no differences were found among the San Francisco women from different social levels in age at last experience of various sexual activities, adult frequencies for specific sexual activities, number of sexual partners, or positive or negative reactions to their most recent sexual practices. Finally, we should note that despite some large differences between the correlations for San Francisco males and females, few are statistically significant.

Male and Female Differences over Time

Kinsey et al. (1953) made much of the fact that social level distinguished the sexual patterns of males but not females. Thus, for the Kinsey et al. data, there are significant differences between the social class relationships for men vs. women with respect to various dimensions of sexual experience. In contrast, our more current data show that social level affects the sexual patterns of both males and females and in similar ways. Thus, in the San Francisco Sample there are no significant differences between the social class relationships for men vs. women with respect to incidence, age at initial sexual experiences, incidence and frequencies of adolescent sexual activities, and positive reactions to initial sexual experiences. Since in all cases Kinsey et al. (1953) did not obtain comparable data, we do not know if some of the same patterns we found were also characteristic of the 1940s. If, as they suggest, these patterns were not evident then, our findings represent a true change. Contrary to Kinsey et al. (1953), though, we believe that any lack of social class effects with regard to these variables would have resulted from the common socialization experiences of females, not, as they maintained, because women are less sexually conditionable. Regardless of their social level, this socialization discouraged women's sexual expression even more in Kinsey's era than in the late sixties (cf., Klassen et al., forthcoming). Thus, it is our belief that in the 1940s *gender* outweighed *social level* in the determination of female sexual patterns.

Today, female sexual expression is more accepted. As our results indicate, the role of gender seems to have been attenuated somewhat and other factors allowed to come into play that have effects on sexual patterns. With these changes, the sexuality of females has taken on some of the features that characterize males of the same social class. Thus, social class now competes with gender in the determination of female sexual patterns.

CONCLUSIONS

The preponderance of evidence we have been able to assemble leads us to conclude that there has been no strong embourgeoisment of sexual patterns since the 1940s. The limited embourgeoisment we found has been among males for whom social class no longer distinguishes what type of sexual behaviors they will experience. Aside from this, social class is important among males with regard to the age at onset of various sexual behaviors as well as incidences and frequencies of these acts during adolescence. For females, social class is related to early sexual activity and to positive feelings toward various first sexual experiences.

---

[15] We thought that this might be related to the later age at which they begin their sexual activity. Controlling for initial age, however, did not affect the relationship.

The class differences we find seem to have a consistent logic: a movement we have described as within-class convergence. The uniqueness of class-related styles of sexuality, discovered by Kinsey et al. (1948; 1953), appears to have widened and become more consistent rather than disappearing into some general sexual pattern shared by most Americans.

These findings are in line with other empirical studies of embourgeoisment, most of which show that strong class differences still remain in modern affluent societies (cf., Goldthorpe et al., 1968a; 1968b; 1969; Glenn and Alston, 1968; MacKenzie, 1973). Some of the reasons that have been suggested for the continuing influence of social class can be applied to our findings.

One, favored more by left-wing European sociologists, is that structural differences still exist in the larger society that make the economic situation of the classes different. Thus, for example, despite increases in the standard of living, the working class still lags behind in earnings, job security, occupational mobility, fringe benefits and hours worked (Rinehart, 1971). This is especially the case for those at the bottom of the working class. Schmidt (1977:289) has written that a " . . . certain socio-economic level is a prerequisite for emotionally satisfying and mutual sexual relations enjoyable for both partners" so that the effects of insecurity, poverty and joblessness ruin sexuality.[16] This is no doubt true for that section of the working class he refers to as the "unstable" working class. It is unlikely, however, that he is referring to the level of working-class people in our current sample.

Furthermore, one need not take a deprivation approach with respect to structural factors, especially when dealing with the upper levels of the working class.[17] MacKenzie (1973) suggests that we must stress not income but a person's position in the division of labor. His study of affluent construction workers shows them to be far from economically deprived yet to reflect distinct working-class traits because of their location in the division of labor. This is seen to have important consequences in how they socialize their children. In line with Kohn (1969), he sees working-class parents as emphasizing conformity, respect for others, and good behavior consistent with jobs that require few decisions. This is in contrast to middle-class socialization, which inculates values of self-confidence, innovation, and perseverance, all of which are important to middle-class occupations. One would imagine that such varying values could also affect sexual patterns. Deferred gratification, for example, has been frequently cited for its role in delaying middle-class sexual activities (cf., Kinsey et al., 1953; Linderfeld, 1960).

At the same time, it is difficult to see how structural factors per se can account for our findings. (For example, they seem at odds with the lower class deprivation hypothesis unless one were to explain the precociousness of working-class adolescent sexual behavior in terms of ineffectual homes caused by economic pressures on the parents. In addition, controlling for age at marriage, a class-linked structural characteristic, does not change our findings.) Structural factors do, however, seem important in isolating or insulating the working class from a variety of influences.

---

[16] He also writes (Schmidt, 1977:289) that " . . . the socioeconomic situation of an individual is one of the most powerful and basic variables correlating with the sexuality of an individual, though the exact relationship of cause and effect is unknown."

[17] Schmidt (1977:293) does, however. He writes that the working conditions of the stable working class " . . . are manual, often physically exhausting activities that must be performed to earn a living and which do not provide much satisfaction or self actualization, and no station or prestige. These conditions make it very difficult for people to approach and conceive of sexuality as something playful that can be enhanced with fantasy. For them sex, much the same as eating, is a serious and direct fulfillment of a physical need. . . . Within the middle class, on the contrary, the process of fulfillment is artfully delayed, refined beyond the physical body-to-body aspect and maximized." (This contrasts with the basic similarities he claims exist between adolescents from the stable working class and middle class.)

This relates to a second major reason given for the persistence of class differences and an answer favored more by American sociologists. This position sees classes as cultural groups, more or less separate from each other, each enjoying a distinct style of life.[18] What these studies of social class strikingly demonstrate is the persistence of social attitudes and values over time (Shostak, 1969; Miller and Riessman, 1961; LeMasters, 1975; Miller and Simon, 1974). There certainly is a structural base to these class cultures (as, for example, MacKenzie [1973] and Rainwater and Handel [1963] have demonstrated), but investigators are more likely to emphasize cultural determinism in their explanations. We would argue that the class differences found in our study are best explained in this way, although the structural basis of these class cultures should not be ignored (for example, when young adults find themselves in college rather than in the labor force). Moreover, such structural factors operate to segregate the classes from each other and allow them to retain their unique cultural character. (At the same time, however, most of this segregation is probably self-imposed; the classes simply do not want to mix or adopt the life styles of one another.) Thus old meanings persist. For the working class, sex is still seen as something natural, to be engaged in as early as possible. What social change has occurred suggests that working-class females are now coming to share some of the working-class males' sexual patterns.[19] In addition, the working-class male has extended his sexual repertoire since Kinsey et al.'s time, but in the same class-prescribed manner in which he previously pursued coitus. For the middle class, sex is still deferred more. Middle-class men and women both start sexual activities at a later age and have less sex during adolescence than do working-class men and women. In addition, more middle-class than working-class women enjoy their first sexual experiences.

The link between class and sexual patterns still remains, we believe, because social classes as cultural groups have not really changed. We propose that the following has occurred:

1. Greater affluence (e.g., more TVs, cars, suburban houses, etc.) produces only surface changes among the working class. It creates a working class with all the accoutrements of an improved standard of living, but it does not produce a working class that has changed its values simply because it has become less distinguishable from the middle class in terms of its consumption patterns. Old values still remain and are used to interpret those surface changes so that the new is absorbed into the old.

2. A similar process probably occurs also in the case of the mass media and its supposed influence. The two-step flow of communication still operates so that middle-class messages are filtered through other working-class people and not transmitted directly to atomistic individuals (cf., LeMasters, 1975:142–3).

3. So-called embourgeoisment influences do not seem to have affected working-class behavior in other areas (e.g., political behaviors, cf., Goldthorpe et al., 1968b), so why should we expect them to affect sexuality? Social classes still remain segregated from one another, which limits middle-class influences. Moreover, if knowledge of others' sexual patterns is characterized by "pluralistic ignorance" (Gagnon, 1965), there is little that has altered this. That the sexual content of the media has increased does not necessarily change things. In addition to

---

[18] Reiss (1965:756), for example, states, "My interpretation of the relation between social class and permissiveness suggests that in studies of this sort social class data should be refined to produce groups that actually share a common style of life. People who share a generally liberal or conservative attitude are more likely to have similar attitudes toward sex than are people who merely earn the same salary or have gone to school the same number of years, though of course style of life is in part a consequence of income and education."

[19] Whether the change in working-class women's sexual patterns means they enjoy sex more or are coerced into it more by males, we do not know. Other studies suggest the latter interpretation (Rubin, 1977).

the filtering effect of the two-step flow of communication, class and gender socialization are far more powerful than the influence of the media (Rubin, 1977).

4. Most of the emphasis in studies of embourgeoisment has been on changes in the working class. An understanding of the issue cannot be achieved, however, unless we also consider changes in the middle class. Many middle-class occupations are rapidly approaching the situation of working-class jobs in terms of routinization, lack of control, and remuneration (cf., Rinehart, 1971; Roberts et al., 1978). To avoid the decline into the working class, many members of the middle class distinguish themselves by emphasizing middle-class symbols (Gusfield, 1967). This can be expressed through sexual values and practices, which again results in segregation between the classes (Zurcher et al., 1971).

5. Finally, we should not overstate the notion of a few separate and distinct classes, each having its own unique sexual patterns. (Kinsey et al. (1948) seemed to do this in their descriptions.) Today, it is arguable that stratification has become more complex and that there are a number of distinguishable strata within both the working and the middle class. Thus it might be that any talk of embourgeoisment or convergence between the classes will depend on exactly how many classes are thought to exist and how their distinguishing characteristics are conceptualized.

REFERENCES

Bell, Alan and Martin S. Weinberg
1978 Homosexualities: A Study of Diversity Among Men and Women. New York: Simon and Schuster.

Cochran, William G., Frederick Mosteller, and John W. Tukey
1953 "Statistical problems of the Kinsey report." Journal of the Americal Statistical Association 48:673–716.

Dahrendorf, Ralf
1964 "Recent changes in the class structure of European societies." Daedalus 93:225–270.

Delora, Joann S. and Carol A. B. Warren
1977 Understanding Sexual Interaction. Boston: Houghton Mifflin.

Edwards, Allen L.
1976 An Introduction to Linear Regression and Correlation. San Francisco: W. H. Freeman.

Gagnon, John
1965 "Sexuality and sexual learning in the child." Psychiatry 28:212–28.

Gagnon, John H. and William Simon
1970 "Prospects for change in American sexual patterns." Medical Aspects of Human Sexuality 4:100–17.

Gebhard, Paul H. and Alan B. Johnson
1979 The Kinsey Data: Marginal Tabulations of the 1938–1963 Interviews Conducted by the Institute for Sex Research. Philadelphia: W. B. Saunders.

Ginsberg, Eli and Hyman Berman
1963 The American Worker in the Twentieth Century. Glencoe: Free Press.

Glenn, Norval D. and Jon P. Alston
1968 "Cultural distances among occupational categories." American Sociological Review 33:365–82.

Goldthorpe, J. H., D. Lockwood, F. Bechhofer, and J. Platt
1968a The Affluent Worker: Industrial Attitudes and Behavior. Cambridge, Eng.: University of Cambridge Press.
1968b The Affluent Worker: Political Attitudes and Behavior. Cambridge, Eng.: University of Cambridge Press.
1969 The Affluent Worker in the Class Structure. Cambridge, Eng.: University of Cambridge Press.

Gusfield, Joseph R.
1967 "Moral passage: the symbolic process in public designations of deviance." Social Problems 15:175–88.

Hamilton, Richard F.
1963 "The behavior and values of skilled workers." Pp. 42–57 in Arthur B. Shostak and William Gomberg (eds.), Blue-Collar World: Studies of the American Worker. New Jersey: Prentice-Hall.

Hobbs, A. H. and R. D. Lambert
1948 "An evaluation of sexual behavior in the human male." American Journal of Psychiatry 104:758–64.

Kinsey, Alfred C., Wardell B. Pomeroy and Clyde E. Martin
1948 Sexual Behavior in the Human Male. Philadelphia: W. B. Saunders.

Kinsey, Alfred C., Wardell B. Pomeroy, Clyde E. Martin, and Paul H. Gebhard
1953 Sexual Behavior in the Human Female. Philadelphia: W. B. Saunders.

Kohn, Melvin L.
1969 Class and Conformity. Homewood: Dorsey.

Klassen, Albert D., Eugene E. Levitt and Colin J.
Forthcoming American Sexual Standards (tentative title).

LeMasters, E. E.
1975 Blue-Collar Aristocrats: Life Styles at a Working-Class Tavern. Madison: University of Wisconsin Press.

Lenski, Gerhard
1963 The Religious Factor (rev. ed.). Garden City: Doubleday.
Linderfeld, Frank A.
1960 "A note on social mobility, religiosity, and students' attitudes towards premarital sexual relations." American Sociological Review 25:81–4.
MacKenzie, Gavin
1973 The Aristocracy of Labor. New York: Cambridge University Press.
Mayer, Kurt B.
1963 "The changing shape of the American class structure." Social Research 30:458–68.
Miller, Patricia Y. and William Simon
1974 "Adolescent sexual behavior: context and change." Social Problems 22:58–75.
Miller, S. M. and Frank Riessman
1961 "The working-class subculture: a new view." Social Problems, Vol. 9, No. 1:86–97.
Miller, S. M.
1964 "The American lower classes: a typological approach." Social Research 31, No. 1:1–22.
Mills, C. Wright
1959 The Power Elite. New York: Oxford University Press.
Rainwater, Lee and Gerald Handel
1963 "Changing family roles in the working class." Pp. 70–6 in Arthur B. Shostak and William Gomberg (eds.), Blue-Collar World: Studies of the American Worker. New Jersey: Prentice-Hall.
Reiss, Ira L.
1965 "Social class and premarital sexual permissiveness: a re-examination." American Sociological Review 30:747–56.
1974 "Changing trends, attitudes and values on premarital sexual behavior in the United States." Pp. 286–316 in Felix F. de la Cruz and Gerald D. LaVeck (eds.), Human Sexuality and the Mentally Retarded. Maryland: Penguin Books.
Rinehart, James W.
1971 "Affluence and the embourgeoisment of the working class: a critical look." Social Problems 19:149–59.
Roberts, K., F. G. Cook, S. C. Clark, and Elizabeth Semeonoff
1978 The Fragmentary Class Structure. Great Britain: Humanities Press.
Robinson, Paul
1976 The Modernization of Sex. New York: Harper and Row.
Rubin, Lillian Breslow
1977 Worlds of Pain: Life in the Working-Class Family. New York: Basic Books.
Sagarin, Edward
1973 "The good guys, the bad guys, and the gay guys." Contemporary Sociology 2:3–13.
Schmidt, Gunter
1977 "Working class and middle-class adolescents." Pp. 5–20 in John Money and Herman Musaph (eds.), Handbook of Sexology. Amsterdam: Excerpta Medica.
Shostak, Arthur B.
1969 Blue-Collar Life. New York: Random House.
Wallin, Paul
1949 "An appraisal of some methodological aspects of the Kinsey report." American Sociological Review 14:197–210.
Weinberg, Martin S., and Colin J. Williams
1975 Male Homosexuals: Their Problems and Adaptations. New York: Penguin Books.
Wilensky, Harold L.
1964 "Mass society and mass culture: interdependence or independence?" American Sociological Review 29:173–97.
Zurcher, Louis A., R. George Kirkpatrick, Robert G. Cushing and Charles K. Bowman
1971 "The anti-pornography campaign: a symbolic crusade." Social Problems 19:217–38.
Zweig, Ferdynand
1961 The Worker in an Affluent Society. London: Heinemann.

SUDDEN WEALTH, GRATIFICATION AND ATTAINMENT: DURKHEIM'S ANOMIE OF AFFLUENCE RECONSIDERED*

MARK ABRAHAMSON
University of Connecticut

American Sociological Review 1980, Vol. 45 (February):49–57

This paper discusses the effects of sudden increases in wealth, a relatively neglected aspect of Durkheim's theory of anomie. The effects are examined in a representative sample of persons who won between $500 and $100,000 in state lotteries. Following an incubation period of over one year, it is found that the magnitude of winnings are positively related to gratification permissiveness and inversely related to future aspirations. However, both of these immediate effects tend to be anomia-reducing, a finding which seems contrary to Durkheim's affluence thesis. The findings are interpreted in relation to changes in normative structures, and it is proposed that such changes alter the meaning of anomia as a personal-level indicator of a state of anomie in society.

Durkheim's theory of how individuals are integrated into society stressed the importance of temporal continuity. A "rapid alteration" of the social organization, Durkheim stated (1951: 368), "must be morbid" in its consequences. If change is sudden or dramatic, it tends to sever both the ties and the commitments which bind people to the social order (cf., Wilensky, 1961: 521–39). In other words, people are unable to establish and maintain a coherent life plan (Mannheim, 1940:56). When alterations in the social structure produce widespread disruptions, Durkheim described the resulting malintegration and normative breakdown as a state of anomie. From a social psychological position, such disruptions presumably reduce the individuals' sense of belongingness, resulting in anomia at a personal level (Srole, 1956: 709–16).

At a microlevel many of the early symbolic interactionists shared Durkheim's theoretical emphasis upon continuity. Noteworthy in this regard are Cooley's (1909) view that change-induced disruptions can become like a disease to people, and Mead's (1934) view that capacities for reflective thought and realization of the self are dependent upon gradual changes in the relationship patterns characteristic of a community or society.

Most of the contemporary research that examines temporal continuity has focused upon variables such as attempted suicides (Paykel, 1974: 134–63) and grade changes (Wildman, 1978: 34–46) as consequences of such life changes as job loss and death of loved ones. Thus, adverse life changes (as conventionally defined) have been emphasized. Durkheim appreciated the significance of adverse changes—such as the sudden loss of business fortunes—but he also stressed how anomie could result from improvements in material conditions. He warned, for example, "of the moral danger involved in every growth of prosperity" (Durkheim, 1951:254). This study, following Durkheim's improvement concerns, examines the adaptations of winners of state lotteries in order to clarify further people's reactions to sudden affluence.

Durkheim's Two Types of Anomie

In both the *Division of Labor* (1933) and in *Suicide* (1951), Durkheim regarded anomie as a normative breakdown. In the former, he traced anomie to a malintegration among the components of a society and it was primarily this view which Merton helped to popularize in two essays (Merton, 1934:537–45; 1938:672–82), the second of which presented Merton's well-known means-ends typology. In this typology he largely equated anomie with

---

* Direct all communication to: Mark Abrahamson; Department of Sociology; University of Connecticut; Storrs, CT 06268.

I am indebted to Jerold Heiss, Charles Logan, and Kimberly Meyers for their assistance in the data analysis; to Albert K. Cohen, Ephraim H. Mizruchi, and Whitney Pope for their help in formulating the issues; and to Robert K. Merton for his extensive and helpful comments.

tendencies to overemphasize either institutionalized means or culturally prescribed goals. Merton dwelled specifically upon situations in which the institutionalized means were denigrated, as epitomized by the person who cheats at solitaire.

Through the deviance research stimulated by Merton's original paradigm, further attention was directed at the problem of differential access to the legitimate means (cf., Cloward and Ohlin, 1960; Clinard, 1964). Finally, anomic conditions (i.e., blockage) were regarded as most pronounced among the poorest segments of American society. In *Suicide* (1951), Durkheim recognized the potential anomie-producing qualities of both poverty and affluence, and Merton also noted the "anomie of affluence" that can result "when Fortune smiles" (Merton, 1957:188). However, Durkheim's analysis of affluence was less emphasized by Merton, especially in his earlier essays and by the Merton-inspired applications (Mizruchi, 1967:439–46). Most overlooked was Durkheim's (1951:254) contention that poverty was "a restraint in itself" which by "compelling moderation, accustoms men to it . . . . Wealth, on the other hand . . . suggests the possibility of unlimited success." Thus, wealth deludes people into thinking that there are no limits and in the absence of normative restraints they become rebellious, immoral, and eventually, despondent.

A de-emphasis upon the anomie of affluence may be partially attributable to the time at which Merton's first essays appeared, namely, during the great depression of the 1930s (Simon and Gagnon, 1976:356–78). People's capacity for handling plentiful riches hardly presented a pressing social problem. Similarly, the general economic conditions then prevalent may also have led Merton to be sensitive to restricted opportunities. However, Simon and Gagnon (1976:356–78) argue, access to institutionalized means is not now problematical for large segments of the American population. It is the relative capacity of people to cope with potentially abundant gratifications that has correspondingly become more problematical because when access to institutionalized means of attainment is overly easy the rewards can become trivialized. By juxtaposing gratification against attainment orientations, Simon and Gagnon present a typology of adaptations that may be highly relevant in a period where relative affluence characterizes a large segment of the society.

For Durkheim, however, the anomie of affluence is a consequence not only of the distribution of wealth, but also of changes in wealth. He argued, at a societal level, that states of anomie were especially likely if an improvement in material conditions occurred rapidly. "In the case of . . . an abrupt growth of . . . wealth . . . the scale is upset; but a new scale cannot be immediately improvised" (Durkheim, 1951:253). Following Durkheim, but focusing upon social psychological correlates, this study will examine the anomia-producing effects of sudden increases in personal wealth, and the role of gratification and attainment orientations as intervening processes.

It is, of course, generally difficult to equate Durkheim's conception of anomie with any personal (i.e., individual-level) qualities. This is particularly true when the focus is upon his anomie of affluence thesis, because it contains social psychological implications that seem to be incongruent with each other.

On the one hand, Durkheim describes affluence as entailing an insufficient discipline of passions which ultimately leaves people empty-handed and weakens their desire to live. This effect may be consonant with the low sense of belongingness (or high estrangement) tapped by the Srole anomia scale (and other similar scales that purport to measure alienation, internal-external control, or the like).

On the other hand, Durkheim also views affluence as overstimulating desires by bestowing upon people a sense of power that deceives them into believing that they are answerable only to themselves. While Durkheim regards it as a false sense, such feelings of power nevertheless seem quite inconsistent with high scores on an anomia, or related, scale.

There is, in sum, ample reason to be hesitant about translating Durkheim's anomie of affluence theory directly into individual-level hypotheses. The conditions under which anomia can be viewed as a personal-level proxy for a state of anomie will be examined in the final section of this paper.

THE SAMPLE

Each year the lottery division of the Connecticut State Commission on Special Revenues provides the U.S. Internal Revenue Service with printouts of the names, addresses and social security numbers of all persons who won any amount of money during the preceding year. Also included is the amount they won, the date and the type of lottery involved. These printouts for the years 1977 and 1978, containing a total of almost 5,000 entries, were taken as the initial population.

Most persons win relatively small amounts, especially when compared with what is promised by advertisements for the lottery. However, when both 1977 and 1978 were combined, there were: 58 persons who won between $50,000 and $100,000; 26 persons who won between $21,000 and $49,000; 94 persons who won exactly $20,000; and 12 persons who won between $11,000 and $19,000. In total, there were 190 persons who won between $11,000 and $100,000, and all of them were included in the sample list. An additional 110 persons who won $10,000 or less (down to a few hundred dollars) were also included by a systematic sample of every 41st name not included among the over $10,000 winners. Thus, 300 persons were initially selected to receive mailed questionnaires. The questionnaires were anonymous, but from coding on the return envelopes each respondent was identified on the Internal Revenue Service list from which the amount and date of their winnings was ascertained and coded with the questionnaire data. (Each respondent was subsequently coded by number only, and the IRS lists were destroyed.)

As many as three separate mailings were eventually sent to each of the 300 persons initially selected. In the first and second round 19 persons were excluded either because they had died (in the interim) or moved and no forwarding address existed according to the post office records. From this revised list of 281 "eligible" persons, 164 subsequently returned (at least partially completed) questionnaires—a return rate of approximately 60%. This sample of 164 persons was then compared with the revised list of 281 eligibles. There were no significant, or substantial, differences with respect to date of winning, type of lottery, or amount of winning, though refusals exhibited a slight tendency to have won earlier (i.e., longer ago) than respondents.

Twenty-one months before this survey was conducted, a multistage probability sample of the entire state of Connecticut (n = 568) indicated that just over one-half of all adults in the state had participated in one or both of the lotteries during the preceding year (Abrahamson and Wright, 1977). In terms of occupation, sex and income they were highly representative of all state residents, but participants did tend to be slightly older. The 164 respondents included in this sample are, in all of the preceding respects, almost perfectly representative of the larger sample of lottery participants who, in turn, are generally representative of all adults in the state.

Open-ended interviews with 10 of the lottery winners provide additional background information. Half of them were in-person interviews, conducted independently of this study (Monagan, 1979:33–6). The other half were telephone interviews conducted as part of this study.

THE MEASURES

The winnings of persons in the sample, as previously noted, vary from as little as a few hundred dollars to as much as $100,000. The greater their winnings, it is hypothesized, the greater should be the anomia effects. However, the value, or marginal utility, of people's winnings may vary as a function of their prior income levels. A $75,000 prize, for example, will probably have a greater impact upon an $8,000 a year factory worker than upon a $50,000 a year executive. Therefore, winnings are operationalized in two ways: by the (log of the) amount won (cf., Hamblin, 1971:423–52), and by a ratio of the absolute amount won to preceding year's total income.

Anomia is measured by the five-item Srole (1956:709–16) scale and one item from the Neal and Seeman (1964:216–26) powerlessness scale. (Other Neal-Seeman items were also included, but they helped to identify other factors that were theoretically uninteresting.) These six items comprise a unidimensional measure according to a (varimax orthogonal) factor analysis. Items and loadings are as indicated in Table 1. Each respondent was given a composite factor score on the anomia factor, as defined above.

In order to construct composite measures of the gratification and attainment orientations discussed by Simon and Gagnon (1976:356–78), a large (and equal) number of items relating presumably to each dimension were subjected to a variety of factor analyses. Those items which greatly overlapped both dimensions were systematically removed until items relating to the two constructs were eventually distinguishable from each other. The theoretically clearest solution entailed two somewhat oblique factors (r = −.22).

The first factor, which includes the gratification items, focuses upon people's perceptions of, and their approval or disapproval of, contemporary patterns of indulgence: alcohol consumption, drug use, and sexual standards. More specifically, this factor is built on the degree to which people generally condemn what they perceive as the styles that are in vogue. This factor may be further described as involving proscriptive gratification norms or an ascetic orientation.

Table 1. The Anomia Factor

Items	Anomia Factor Loadings
(16) In spite of what some people say, the situation of the average person is getting worse, not better.	.64
(7) More and more I feel helpless in the face of what's happening in the world today.	.64
(12) These days people don't really know who they can count on.	.62
(23) It's hardly fair to bring a child into the world with the way things look for the future.	.58
(20) Nowadays, a person has to live pretty much for today and let tomorrow take care of itself.	.48
(5) Most public officials are not really interested in the problems of the average person.	.48
(Eigenvalue = 2.59; % of Var. = 43)	

The second factor focuses upon people's general commitment to strive for future attainments: to build a marriage, become wealthy, or simply excel at whatever they do. The third item, dealing with the enjoyment of wealth, appears, at face value, to be discrepant with the other items. Its inclusion requires additional interpretation. Perhaps its variation in common with the other attainment items results from respondent's subjective reaction to the question as meaning that enjoyment is natural *after* people strive to acquire wealth. This is admittedly conjecture, but such an interpretation would account for the item's inclusion in this factor. All of the items and their loadings are presented in Table 2.

By contrast to the first factor (i.e., gratifications), the second factor (i.e., attainment) stresses prescriptive rather than proscriptive norms, and a more futuristic orientation. These are, of course, two of the major characteristics attributed by Weber to the early Puritans (Weber, 1958). If Weber was correct in noting a tendency for these orientations to fuse, that would account for the preliminary difficulty encountered here in constructing separate indices of each. However, the importance of their analytic distinction from each other prompts their operational separation and it should be recalled that, as measured here, these two dimensions are only mildly related.

Single items were also included in the questionnaire to measure: total yearly income, marital and occupational status (both before winning and now), and sex and age.

RESULTS

Neither measure of the winnings variable exhibits a significant, or substantial, direct correlation with anomia (i.e., factor

Table 2. Gratification and Attainment

	Loadings, Orientation to:	
Items	Gratification	Attainment
(9) There is too much drinking of alcoholic beverages today. (Disagree)	.67	.04
(6) People should never smoke marihuana because it leads to a life of drugs. (Disagree)	.55	−.03
(19) Almost everyone finds leisure time more satisfying than work.	.50	−.12
(22) Today's sexual morality seems to be, "anything goes." (Disagree)	.46	−.12
(14) It is important for people to make sacrifices in order to build a happy marriage.	−.20	.59
(10) It is natural for people to try hard to become wealthy.	−.01	.46
(4) It is natural for people to enjoy being wealthy.	.05	.42
(8) I admire people who try to be best at whatever they do.	−.24	.38
	Eigenvalue = 2.16 % Var. = 20	Eigenvalue = 1.61 % Var. = 15

scores on anomia). However, anomia is significantly related to both the denial of gratifications and future attainments factors, though neither zero-order correlation is large. A complete correlation matrix, including all of the variables, is presented in Table 3. For the most part, it contains few substantial correlations. The entire pattern becomes clearer, however, when people are dichotomized according to the time they won; that is, the time that elapsed between this survey and the time they won. After winning a large amount, most of those interviewed report experiencing a long hiatus. "It took a good year to realize what happened," one winner explained. It is after such an incubation period that the effects of winning become visible. The approximate mid-point of the time variable in the sample is one and one-half years, and this interim corresponds reasonably well with the self-reported incubation periods. (One hundred and fifty people totally completed questionnaires; 73 of them won between one and one-half and three years before the survey.) Also presented in Table 3 is the correlation matrix including only those persons who won one and one-half years, or more, before the survey.

The relationships between anomia and both gratification and attainment are more pronounced in this subsample; and both of the latter variables are also more strongly related to the winnings variables, (log) amount and ratio of winnings to prior income.

The positive relationships between both measures of winning and people's acceptance of gratification are congruent with Durkheim. He proposed that a dramatic increase in wealth would lead people to become more permissive, but an endless pursuit of pleasure, coupled with a rejection of normative restraints, was regarded as generating anomie. However, these findings indicate an inverse relationship between anomia and acceptance of gratification (r = −.30); the more indulgent the orientation the lower the anomia.

For some persons, orientations toward gratification are relatively fixed by internalized moral norms; as such, they are not responsive to changes in opportunity (cf., Campbell, 1964). In addition, the proscriptive quality of gratification norms (i.e., thou shalt not) is associated with inflexibility and pervasiveness (cf., Mizruchi and Perrucci, 1962). Little indulgence is therefore possible for a person who is striving to maintain a moral identity in a social network that supports proscriptive gratification norms; the boundaries are too inelastic and too few situations are defined as allowing for exceptions.

The roles of both internalized standards and social pressures are illustrated by the experiences of a 32-year-old married woman whose lottery ticket produced a very large winning. Just before the lottery drawing she dreamt of owning a Lincoln Continental, "the big one with the wheel on back and everything." After she won, she also began to think about going to fancy restaurants, staying in nice hotels, going on a big "shopping spree" at Tiffany's in New York.

Two years later, however, her life style

Table 3. Correlation Matrix (Winners, One and One-half Years Ago or More, n = 73 / Total Sample n = 150)

	(2) Win Ratio	(3) Sex (M)	(4) Age	(5) Prev. Marital	(6) Marital Change	(7) Prev. Employ. Status	(8) Occup. Change	(9) Attainm.	(10) Gratific.	(11) Anomia
(1) (log) win	.77/.84	.02/ − .01	.01/.11	.05/.08	.13/.08	− .06/.04	.22/.08	− .20/ − .09	.16/ − .01	− .12/ − .04
(2)		− .04/ − .04	− .08/.10	− .10/ − .01	.10/.04	− .07/ − .02	.24/.11	− .18/ − .13	.16/ − .05	.06/.07
(3)			.13/.00	.11/.13	.25/.20	.21/.30	.09/.14	− .07/ − .02	.15/.22	− .06/.00
(4)				.25/.07	− .10/ − .08	− .37/ − .34	.09/.02	.06/.05	− .50/ − .50	.03/.06
(5)					− .15/ − .11	.02/.10	.07/ − .05	.13/.14	− .22/ − .11	.23/.15
(6)						− .03/.00	.03/.01	− .25/ − .18	.25/.17	− .21/ − .11
(7)							.17/.14	.02/.01	.26/.26	− .11/ − .02
(8)								− .01/ − .02	− .03/ − .01	.03/.04
(9)									− .34/ − .22	.25/.12
(10)										− .30/ − .24

and her possesions are the same as they were before, except for a much larger savings account. "At times . . . a part of me says, 'Just do it! Break loose!' But we don't." She claims to not fully understand it, but something inside of her seems to restrain her impulses. She is aware, though, of her own and her husband's concern for appearances; that is, how they think they would look to their friends if their life style became more indulgent. "People we know wouldn't have accepted it . . . they would have thought, 'the lottery made her do it.' "

For other big winners, there is the previously described hiatus, and then an increased pursuit of gratification that is anomia-reducing. These persons are either more relaxed about indulging themselves or/and associate with others who are more permissive in this regard. In either case, they seem able to establish more subjectively satisfying modes of adaptation. The experiences of another large winner, also a woman and in circumstances similar to the first prior to winning, are instructive. She quit her job at the drug store and then took a trip, bought horses, cars, a mink coat, etc. "I can't think of anyone who is a friend who was upset or envious." She claimed that it did take her a while to get used to indulging, but "now if I get bored I go out and buy something and then I feel better."

Aspirations for future attainment, in contrast to gratification acceptance, are inversely related to the size of a person's winnings and positively related to anomia. Thus, large winnings depress strivings for the future, and the more they are not depressed, the greater the experience of anomia. It may be that a lottery windfall produces dissonance for persons with a strong attainment orientation; perhaps, for them, it is not the way people are supposed to acquire wealth. Thus, they may be estranged from meaningful commitments about which they have organized their lives. In addition, some large lottery winners became local celebrities as the result of media coverage. Becoming a public figure is often associated with new, and baffling, expectations. One winner was asked to finance a new public pool in town; another was troubled by a "messenger from God" who kept appearing at the door asking for donations; the minor automobile accident of a winner's son resulted in front page headlines in a local newspaper. All of these situations can be experienced by winners as entailing a diminished capacity to either predict or control future events, which results in stress (cf., Coleman, 1973:1–17). The subjective stress that can ensue was clearly expressed by a 55-year-old man, who, speaking for his wife as well, looked back at the last two years and said, "We've just been bothered and worried ever since we won."

From the preceding discussion it is apparent that the indirect effects of the size of winnings, notably through attainment and gratification orientations, are more important than winning's direct effect upon anomia. These effects are examined in the path model presented in Figure 1.

Winning, it will be recalled, occurred at least several months prior to the survey for all respondents. It can be viewed unambiguously as the start of the causal chain. (The ratio of winnings to preceding year's total income and the log of amount won behave similarly as independent variables, though the effects of winning ratio are somewhat stronger and this variable will be employed in the following model.) On theoretical grounds, anomia must be placed at the end of the chain. The placement of gratification prior to attainment is the most problematic decision, but it is based upon the previously discussed view of gratification acceptance as entailing internalized proscriptive norms which, as such, are more enduring personal qualities, less susceptible to situational change than prescriptive attainment norms.

The adequacy of this path model is summarized in Table 4. Based upon the ratio of Betas to their standard errors, the various paths from gratification are the most "secure." The entire model explains a modest 15% of the variance in anomia, but it is the role of gratification orientations that warrants theoretical emphasis.

The amount that people won is positively related to subsequent changes both in marital and in occupational status, but neither status change is included in the path model because neither contributes any additional source of explained variance in anomia. However, marital change is the only variable that focuses upon behavioral changes in "bondedness" and it therefore warrants attention. In addition, it helps to clarify further the impact of changes in gratification permissiveness. First, operationally, this variable is a dichotomy between any change in marital status and no change at all. The two most prevalent changes that occurred after

Table 4. Multiple Regression Results

Variable	Multiple R	R^2	Beta	S.E. Beta
D.V. = Anomia				
Attainment	.27	.07	.20	.12
Gratification	.36	.13	−.27	.11
Win Ratio	.38	.15	.12	.12
D.V. = Attainment				
Gratification	.35	.12	−.33	.10
Win Ratio	.38	.14	−.13	.11

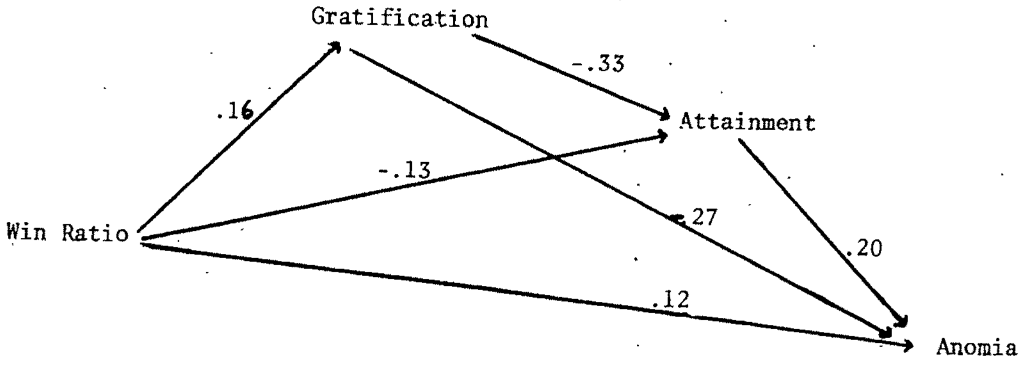


Figure 1. The Path Model

winning were: from married to separated or divorced, and from single to married.

Marital changes are positively related to acceptance of gratification (r = .25) and, like gratification acceptance itself, marital changes are inversely related both to future attainment (r = − .25) and to anomia (r = − .21). This observed pattern of relationship suggests that marital changes are best viewed as behavioral adjustments to more permissive gratification-inspired life styles. (Marital change was initially included in the path model after gratification and attainment on the assumption that changes in behavior occur subsequent to changes in attitude; but all of the variation it shared in common with other variables was already accounted for by gratification.)

CONCLUSIONS

The findings indicate that the major effects of the size of winnings are an increase in gratification acceptance and a decrease in future attainment orientations. These results are substantially congruent with the first part of Durkheim's anomie of affluence thesis. Also congruent is the positive, albeit small, direct path between size of winnings and anomia (if anomia is regarded as a personal-level proxy for anomie). I think this direct path reflects a weak tendency for a rapid increase in affluence, per se, to result in higher anomia; but some nonmeasured variable may intervene.

However, the larger, indirect effects of the size of winnings tend to be anomia-reducing. By spurring a more permissive orientation toward gratifications and by depressing future attainment aspirations, *larger winnings lead to lower anomia.* Thus, increased desires do not seem so brutalizing; the appetities, when whetted, do not seem to result in detachment. High anomia is, rather, the price that is paid by those who resist a readjustment in their consumatory styles. It must also be recalled that this pattern becomes increasingly characteristic as more time elapses since the person won. There is, therefore, little basis for inferring that the observed pattern represents only a temporary mode of adaptation. Thus, when anomia is interpreted as indicating anomie at an individual level, the strongest findings of this study seem to contradict the main thrust of Durkheim's anomie of affluence thesis.

In the late Victorian period which Durkheim confronted, the failure to restrain indulgent impulses may eventually have produced a sense of bondlessness and detachment; that is, anomia. However, the prudish respectability of the Victorian era is normatively very different from the contemporary ("if it feels good do it") scene. Anomia, Mizruchi suggests, may be attributable to the strain of being out of step with the larger social structure (Mizruchi, 1969:243–51); and who it is that is marching to the tune of a different drummer has changed dramatically since the Victorian period. Thus, anomia is today experienced by those persons who, in the face of sudden wealth, continue to reject permissive gratification norms.

There is, of course, one final interpretation, rendered plausible by the lack of perfect isomorphism between Durkheim's macroconception of anomie and the microlevel measurement of anomia. Specifically, from Durkheim's position, the fact that denial of permissive gratification norms is found to produce a sense of detachment might itself be indicative of a societal state of anomie. In other words, it might indicate that there is insufficient internalization of shared regulatory norms; the problem of "modern individualism" (cf., Parsons, 1964:118–53).

This interpretation suggests that, in general, anomia can be regarded as a personal-level proxy for anomie only when a society is characterized by widely shared restrictive gratification norms. In this type of (i.e., Victorian) context it is the people with indulgent orientations who are out of step, and, hence, experience anomia. The greater the number of such persons, we may propose, the more intense the societal level of anomie. However when prescriptive affirmations ("if it feels good do it") are in vogue, the number of persons who experience anomia may actually provide an inverse indicator of anomie because it is the people who maintain proscriptive regulatory norms who are now out of step.

REFERENCES

Abrahamson, Mark and Jack Wright
1977 Gambling in Connecticut. Department of Sociology, University of Connecticut, Storrs. Mimeo.

Campbell, Ernest Q.
1964 "The internalization of moral norms." Sociometry 27:233–54.

Clinard, Marshall B.
1964 Anomie and Deviant Behavior. New York: Free Press.

Cloward, Richard A. and Lloyd E. Ohlin
1960 Delinquency and Opportunity. New York: Free Press.

Coleman, James S.
1973 "Loss of power." American Sociological Review 38:1–17.

Cooley, Charles H.
1909 Social Organization. New York: Scribner's.

Durkheim, Emile
1933 Division of Labor in Society. New York: Macmillan.
1951 Suicide. New York: Free Press.

Hamblin, Robert L.
1971 "Mathematical experimentation and sociological theory." Sociometry 34:423–52.

Mannheim, Karl
1940 Man and Society in an Age of Reconstruction. New York: Harcourt, Brace.

Mead, George H.
1934 Mind, Self and Society. Chicago: University of Chicago.

Merton, Robert K.
1934 "Durkheim's division of labor in society." American Journal of Sociology 40:319–28.
1938 "Social structure and anomie." American Sociological Review 3:672–82.
1957 Social Theory and Social Structure. New York: Free Press.

Mizruchi, Ephraim H.
1967 "Aspiration and poverty." Sociological Quarterly 8:439–46.
1969 "Romanticism, urbanism, and small town in mass society." Pp. 243–51 in P. Meadows and E. H. Mizruchi (eds.), Urbanism, Urbanization and Change. Reading, Mass.: Addison-Wesley.

Mizruchi, Ephraim H. and Robert Perrucci
1962 "Norm qualities and deviant behavior." American Sociological Review 27:391–9.

Monagan, Charles
1979 "Thanks a million." Connecticut, Jan.:33–36.

Neal, Arthur G. and Melvin Seeman
1964 "Organizations and powerlessness." American Sociological Review 29:216–26.

Parsons, Talcott
1964 "Durkheim's contribution to the theory of integration of social systems." Pp. 118–53 in K. Wolff (ed.), Essays on Sociology and Philosophy. New York: Harper and Row.

Paykel, E. S.
1974 "Life stress and psychiatric disorder." Pp. 135–49 in B. S. and B. P. Dohrenwend (eds.), Stressful Life Events. New York: Wiley.

Simon, William and John H. Gagnon
1976 "The anomie of affluence." American Journal of Sociology 82:356–78.

Srole, Leo
1956 "Social integration and certain corollaries." American Sociological Review 21:709–16.

Weber, Max
1958 The Protestant Ethic and the Spirit of Capitalism. New York: Scribner's.

Wildman, Richard C.
1978 "Life change with college grades as a role-performance variable." Social Psychology 41:34–46.

Wilensky, Harold L.
1961 "Orderly careers and social participation." American Sociological Review 26:521–39.

MANUSCRIPTS FOR THE
ASA ROSE SOCIOLOGY SERIES

Manuscripts (100 to 300 typed pages) are solicited for publication in the *ASA Arnold and Caroline Rose Monograph Series*. The Series welcomes a variety of types of sociological work—qualitative or quantitative empirical studies, and theoretical or methodological treatises. An author should submit three copies of a manuscript for consideration to the Series Editor, Professor Suzanne Keller, Department of Sociology, Princeton University, Princeton, NJ 08540.

INDUSTRY MARGINALITY AND FEMALE EMPLOYMENT: A NEW APPRAISAL*

WILLIAM P. BRIDGES
University of Illinois—Chicago Circle

American Sociological Review 1980, Vol. 45 (February):58–75

The postulate that women are overrepresented in marginal industries is examined by formulating six hypotheses which relate aspects of industry marginality to female employment. Data from Population and Business Censuses and other published sources are used to evaluate these hypotheses empirically for detailed industries in both the goods-producing and the service sections of the economy. The relationship between industry differences in male/female employment and occupational segregation by sex is taken into account with direct standardization techniques. The results show that women are underrepresented in capital-intensive industries but that their industrial distribution is not strongly associated with a wide range of marginality characteristics. Furthermore, some types of industries (such as durable-goods manufacturing) employ fewer than expected numbers of women, even when the average marginality of industries in those groups is taken into account.

The belief that discrimination against women results from structural factors is a central tenet of many recent labor market studies, particularly those in the tradition of institutional economics.[1] A clear statement of this principle is given by Beck et al. (1978:707), "In providing a nonindividualistic alternative for the analysis of earnings, theories of the dual economy provide structural explanations for economic differences between racial and sexual groups. Rather than interpreting group differences in earnings or poverty as due to different rates of individual 'failure' in a competitive market, the theory suggests that such group differences may be the outcome of *differential assignments* of group members within the sectoral structure of the economic order" (emphasis added).[2] In this paper, I evaluate one of the central tenets of segmented labor market theory—that women are overrepresented in marginal industries—by assessing several explicit hypotheses with detailed census data. Furthermore, the analysis takes into account the fact that occupational segregation by sex is rampant and may influence the industrial distribution of women workers. The distribution of nonwhites, a topic worthy of attention in its own right, is not analyzed here, however. This analytic separation of women from other "economic" minorities is justified by recent empirical evidence which shows that the two groups play somewhat different roles in the economy (see Villemez, 1977; Snyder and Hudis, 1976). Therefore, the goal of this paper is to explore the applicability of the theory to but one of the groups it supposedly covers.

Hypotheses

The version of the institutional or sectoral approach that I will apply here was

---

* Direct all communications to: William P. Bridges; Department of Sociology; University of Illinois-Chicago Circle; Box 4348; Chicago, IL 60680.

Support for this project was provided by the Graduate College Research Board, UICC, and the University Computer Center. Helpful comments have been received from Dick Berk, Baila Miller, Mildred Schwartz, Ross Stolzenberg, an anonymous reviewer, and especially from Wayne Villemez. As usually happens, I could not put into practice all the worthwhile suggestions which were made. An earlier version of this paper was presented to the 73rd Annual Meeting of the American Sociological Association, held at San Francisco in 1978.

[1] This collection of studies is known by a variety of different names. The more common ones are identified as studies of dual labor markets, segmented labor markets, internal labor markets, split labor markets, core and peripheral economies, etc. An early summary is provided by Gordon (1972). A discussion of their applicability to women is presented by Blau and Jusenius (1976). For a critique, see Cain (1976).

[2] A related argument is made by Bibb and Form (1977:978).

first articulated by Bluestone (1968; 1973) and has been expanded by Fusfeld (1973). They begin with the premise that industries are grouped into a high-wage, primary sector and a low-wage, secondary sector. (I will subsequently refer to this as the marginal industry theory.) Its fundamental assertion is that workers in low-wage industries are not exploited by the enterprises for which they work. Instead, the theory portrays employers, hiring low-wage labor, as middlemen who mediate the relationship of the larger system to the disadvantaged groups of workers. Thus, it is the economy as a whole which exploits certain industrial sectors by depriving them of the institutional protection which is needed to garner high profits, make costly capital investments, and regularize their labor relations by negotiating with powerful unions. The role of individual employers is simply to pass on this disadvantage to their employees so that all levels of the industry, together, must brave the chill of open competition. Bluestone (1968) is explicit about this principle when he writes, "(we) believe that individual low-wage industries, by and large, have had little success in attempting wage exploitation. The working poor are being exploited by the economy as a whole rather than by individual firms employing them."

The distinction between marginal and nonmarginal industries which appears in the dual economy literature should not be confused with the widespread classification of industries as either goods-producing or service-producing (see Fuchs, 1968:14–17). In fact, as many dual economy schemes recognize, the two types of classifications cross-cut one another and it is logically possible for "core" industries to appear in both the service (e.g., banking) and goods-producing (e.g., auto manufacturing) sections of the economy. For reasons discussed here, it is desirable to analyze goods and services sectors separately. The reader will note that, following the terminology of Clark (1951), goods-producing industries are sometimes referred to as "primary/secondary" and services as "tertiary" industries. Neither of these terms is intended to connote anything about the marginality of these groups.

Returning to the main focus, the marginal industry theory, while being only one rendition of the general institutional economic theme, is crucial for two reasons: (1) it is the foundation of further theoretical developments—primarily those which add a neo-Marxist or radical twist (see especially, O'Connor, 1973; Edwards, 1975); (2) in conjunction with the work of Averitt (1968), it has provided the underpinnings for much of the recent empirical research (e.g., see Bibb and Form, 1977; Beck et al., 1978; and Lyle and Ross, 1973). It is essential, therefore, to know how accurately it describes the division of labor by sex.

Several important decisions were made in translating this theory into hypotheses about the patterns of female employment. First, industry marginality was regarded not as a dichotomous attribute but, rather, as a continuous property which was indexed by measurements on several different variables. Bluestone et al. (1973) suggest several relevant dimensions:

> Beyond the fringes of the core economy lies a set of industries that lack almost all of the advantages found in center firms. Concentrated in agriculture, nondurable manufacturing, retail trade, and subprofessional services the peripheral industries are noted for their small firm size, labor intensity, low profits, low productivity, intensive product market competition, lack of unionization, and low wages.

To this list, Fusfeld (1973:28–33) adds the variable of geographic dispersion, arguing that an important aspect of industry dualism is for marginal industries to function as providers of essential supportive services for the core industries which (in a regional sense) are "export"-oriented. Thus, marginal industries are necessarily less geographically concentrated, because they must be present in all producing areas or regions.

In this paper, I have deliberately avoided reducing the several different dimensions of marginality to a unitary vector or, worse, dichotomy. The major benefit of this strategy is that it avoids the wholesale loss of information which occurs with these simplifications. Moreover, findings which are presented here are from the 3-digit level of industrial detail, in contrast to the cruder 2-digit classification systems used in recent studies of comparative income attainment. (See Beck et al., 1978:718–9, for an informative summary of these schemas.) Thus, applying ready-made classification schemas would entail the further sacrifice of useful data and has been rejected.

The following is a list of hypotheses which are amenable to empirical test. Each carries the implicit qualification that "other relevant factors have been controlled":

H_1: The larger the average firm size in an industry, the smaller the proportion of workers who are female.

H_2: The higher the relative level of capital investment, the smaller the proportion of workers who are female.

H_3: The higher the relative level of productivity, the smaller the proportion of workers who are female.

H_4: The higher the level of market concentration in the industry, the smaller the proportion of workers who are female.

H_5: The higher the level of geographic concentration, the smaller the proportion of workers who are female.

H_6: The higher the level of male wages in an industry, the smaller the proportion of workers who are female.

Although the participation of other economic minorities (e.g., nonwhites) is an alleged consequence of marginality and not a defining characteristic of it, the subsequent analysis adds controls for some of these variables. The rationale for these controls is that such factors may act as suppressor variables if they are negatively correlated with female employment and positively correlated with marginality. Omitting them raises the possibility that a "true" differential allocation of women to marginal industries would be underestimated. Consequently, variables measuring nonwhite participation and employment in the South are included in the subsequent regression models.[3] In addition—because of the potential for misspecification introduced by considering nonwhite participation as an exogenous variable—an alternative specification of its role is reported in the findings.

Even though wage levels are included by Bluestone et al. (1973), as a defining characteristic of industry marginality, perhaps the level of wages is better seen as an outcome of industry structure and is therefore out of place in the block of structural variables. Or—pursuing the argument in a less-extreme form—if wages must be included in this analysis, perhaps they should be included with the control variables mentioned in the previous paragraph. Although plausible, these cautionary arguments must go unheeded. The reason is that some of the variables which are defining characteristics of marginality could not be included in this analysis and the wage data are used as a proxy measure for them.

The two variables which have been omitted are level of unionization and profitability. The first is missing because reliable data at this level of industry detail are not available; while the Internal Revenue Service does report profit data by industry, these are unsatisfactory for two reasons. First, the reporting context is suspect, not only because corporations spend millions to minimize their apparent earnings to the tax collector but also because firms in different industries can spend different amounts playing this game. Second, all other statistics in this analysis have been collected on an establishment basis by the Commerce Department, but the IRS collects data on a company basis and often aggregates data from parts of firms that operate in different industries.[4]

Because direct measurement of these

---

[3] The employment of other groups of "disadvantaged" workers might also be taken into account by adding additional control variables. In results not reported here, controls for groups such as teenage males, teenage females, nonwhite females, etc., were used. The findings are barely affected by the presence of these variables and they have the disadvantage of introducing higher collinearity into the matrix of independent variables.

[4] When IRS data are used in this paper they are never used as the sole source of data for a particular variable and they have been subjected to an adjustment procedure to heighten their comparability to Business Census data.

variables is important the inclusion of wage data is a compromise strategy. However, the conclusion that the partial effect of wages is a proxy for these variables is verified by several interindustry studies which show an association between wage levels and unionization, particularly at low levels of concentration (see Fuchs, 1968; Lewis, 1963; Weiss, 1966; and Scherer, 1970) and a similar set of findings implying a link between high wages and monopoly profits (Dalton and Ford, 1977; and Scherer, 1970).

Occupation Effects

A relevant question, although one not typically addressed by those in the dual economy tradition, is how industry variability in female employment is affected by the differing occupational mixes of industries. Because of widespread occupational segregation by sex (see Gross, 1968; Williams, 1976; Oppenheimer, 1970; and Blaxall and Reagan, 1976), two industries which are identical in terms of structural variables (e.g., market concentration) might have differing proportions of female workers if one has a higher proportion of female-stereotyped occupations (e.g., lower-level white collar workers) than does the other. Furthermore, without additional knowledge, it is impossible to say whether the effect of these occupational distributions will be merely to increase the variance in proportion female by adding random error or will be to change the relationship between industry marginality and female employment through the addition of some type of correlated error.

In this paper, the influence of occupational composition is treated in the following way: Using one of the elementary identities of direct demographic standardization, I separate the aggregate percent female in an industry into two components: (1) a standardized rate or adjusted percent female, and (2) an occupational compositional effect (details provided below). This procedure enables one to assess how much the effects of industry characteristics on total female employment (the "crude rate") are due to the relative overrepresentation of women within occupational categories and how much they are due to differences in occupational composition itself.[5] To cite a simple example, industries with a large average firm size are caught in the middle of conflicting forces. On one side, larger size is indicative of less marginality and a hypothetically greater ability to acquire male labor. On the other side, larger average firm size is probably also associated with greater bureaucratization, a larger office work force, more clerical employees, and ceteris paribus, more women workers. The type of analysis proposed does allow one to judge, separately, the size and direction of each of these types of effects.

For the sake of parsimony, the industry effects which are mediated by the occupational compositional component are subsequently referred to in this paper as the effects through *femininity of occupations,* FOCC. The effects through the standardized rate of female employment will be labeled as the effects through *femininity of personnel,* FPER.

While there is a clear need to control for occupational differences when measuring industry effects, using this type of decomposition raises the question of how to interpret the effects of the industry structure variables on the separate components. Would the six hypotheses stated earlier imply relationships between the structural variables and only one of the components or would they imply relationships to both components? The position taken in this paper is that the hypothesized relationships should apply to both component variables.

Let us consider each in turn. In the case of FOCC (the femaleness of the occupational distribution), a two-part argument can be made. First, the industry structure variables do affect the relative sizes of different occupational categories in an industry (i.e., they are not just the result of tradition or the inherent nature of the product). For example, writing about the effects of one structural variable, capital intensity, Blauner (1964:168) says,

---

[5] This procedure does not permit one to examine how the effects of industry marginality vary across different occupations, a question which is beyond the scope of this analysis. Results indicating varying effects of marginality at different occupational levels would mirror Stolzenberg's finding (1975) of occupation specific differences in industry wage effects.

> [A]utomated manufacturing paradoxically increases traditional craft skill, which is applied, however, to maintenance problems rather than production work. Since automation involves a considerable increase in intricate plant machinery and technical processes, considerably more repairmen in various trades are needed to maintain this equipment.

The second half of the argument concerning FOCC is that the existence of a link between industry structure and occupational structure necessarily implies a link between industry structure and female employment levels given rampant sex-stereotyping of occupations. Nevertheless, it is important to recognize that this path of influence of industry structure on female employment depends on sex-stereotyping which reflects, at least in part, the conduct of actors outside the industry in question. That is, part of the reason that craft-work occupations are overwhelmingly male and, by implication, therefore, the industries that use them disproportionately male, has to do with the restrictive admissions policies of craft unions, differences in sex role socialization, and so on. Thus, the path of influence passing through FOCC reflects the articulation of industry marginality with societal sex-stereotyping.

It is easier to establish that structural variables should also influence the second component, FPER. As a "standardized rate," this component measures the industry's "preference" for females, net of its occupational structure. Thus, given the arguments about the need for unprotected marginal industries to employ unprotected vulnerable workers, we would expect the substitution of females within occupational categories for males, wherever possible. Thus, differences in FPER reflect industry differences in the use of females within occupation categories.

Data

The numbers of female and total employees across detailed occupation (n=441) and industry (n=235) categories were taken from the 1970 Population Census Occupation by Industry Table (Subject Report Tape PC(2)-7c). Manufacturing, mining, and construction industries were classified as primary/secondary industries and transportation, communication, utilities, wholesale trade, retail trade, finance, and services were classified as tertiary. Agricultural, household, and government employment were not included in the present analysis, nor were industries labelled "allocated" or "not specified" as these were at a different level of aggregation than the others. Of these 193 within-scope industries, 23 others (mostly in professional services) had to be excluded because of a complete absence of comparable data. The remaining 170 industries represent the basic study population, although there is some additional attrition resulting from the problem of selective missing data on certain variables. This is particularly true in the tertiary sector.

The primary source of data for independent variables used in this study was the economic censuses of 1967 and 1972 (U.S. Bureau of the Census, 1970; 1971a; 1971b; 1971c; 1972b; 1976a; 1976b; 1976c; 1976d). Because assertions of causality rest on substantive and not on temporal grounds in this paper, 1967 and 1972 are taken as representing a single time period. For industries which were outside the scope of the economic censuses, it was necessary to use alternative published data sources. However, noncensus figures were subjected to an adjustment procedure to heighten their comparability to census data. Details are available on request from the author.

The following are used as measures of the key independent variables. Coded from the 1967 economic censuses were the number of firms (with or without employees), the number of employees for the census week, value of fixed assets, new capital investment, and value added. From the 1972 economic censuses, concentration ratios were taken as the proportion of total industry sales held by the top four firms in each industry and, as an alternative measure of employer size, the presence of medium and large establish-

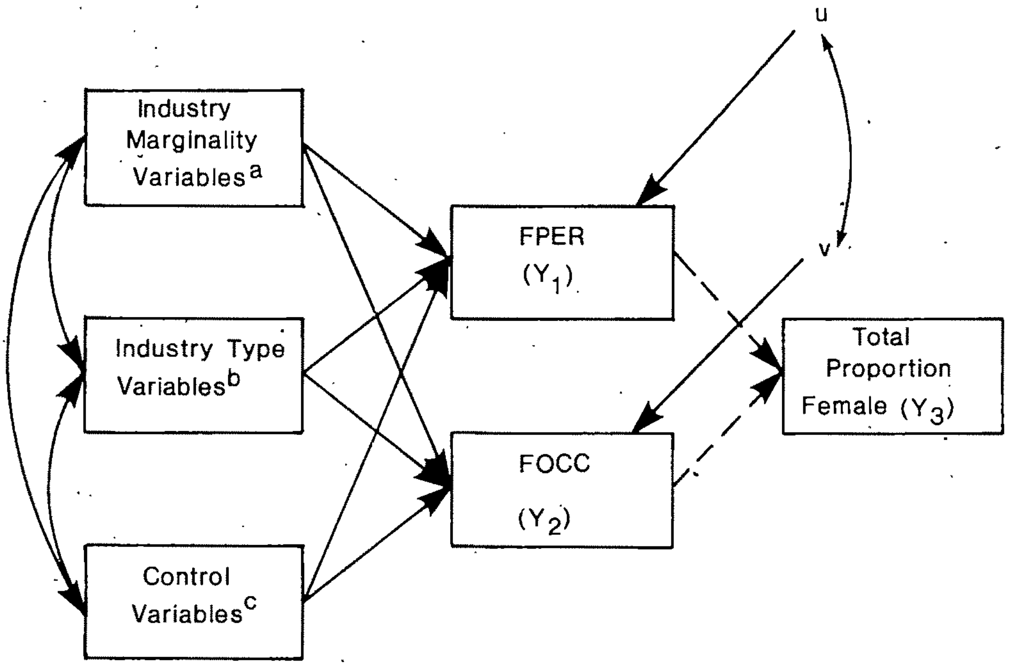

Industry Marginality Variables^a
Industry Type Variables^b
Control Variables^c
FPER (Y_1)
FOCC (Y_2)
Total Proportion Female (Y_3)
u
v

Figure 1. Model of Components of Industry Femininity

[a] Includes following variables: Fixed 'assets' per worker, value added per worker, average firm size, concentration ratio, regional concentration, median earnings.

[b] Includes following variables: Mining or construction dummy, durable-goods dummy in primary/secondary industries; transportation, communication, or utility dummy, wholesale trade dummy, retail trade dummy in tertiary industries.

[c] Includes following variables: Proportion males who are nonwhite; proportion employees who are in South.

ments was evaluated by coding the total number of establishments and the number of establishments with over 20 employees. From the 1970 Population Census, geographic concentration was measured by computing, across census regions, an Index of Dissimilarity between the labor force in each industry and the total civilian labor force. (Values range from 0 to 63.7.) From the same source, nonwhite males employed in each industry were taken as a proportion of total males employed; and employment in the South was measured with the ratio of employees in the South to total industry employment. To indicate relative wage levels, the median dollar earnings of male workers in the experienced civilian labor force of each industry was coded from the 1970 Population Census. Although these are earnings—and not wage—data, in the analysis included here, these data are adjusted for the percent of male workers employed 50–52 weeks per year.[6]

Methodology

As already indicated, there are three dependent variables in this analysis: the adjusted rate of femaleness, FPER; the occupational compositional effect; and the "crude rate" or total proportion female. These are represented on the right-hand side of Figure 1. An important consideration is that the third component, crude proportion female, is algebraically (and not statistically) related to the first two. That is, allowing for error components in the constituent variables, FOCC and FPER, means that there is no additional error in their sum, the crude rate. This can be represented in the following model:

---

[6] Another limitation of the wage data is that they are aggregated across all occupational levels in a particular industry. If, as Stolzenberg (1975) argues, the relative wage position of an industry varies at different occupational levels, different results might have been obtained if occupation-specific wage levels were analyzed. This procedure, however, seems inconsistent with the marginality thesis which argues that whole industries are stricken with low wages.

(1) $Y_3 = b_{Y3Y1}$ FPER + b_{Y3Y2} FOCC;
where: Y_3 = Crude proportion female, $b_{Y3Y1} = b_{Y3Y2} = 1$ = "unstandardized" coefficient of Y_3 on FOCC and FPER.

Being nonstatistical in character, they are indicated with broken lines in the diagram. A further consequence of this representation of the total proportion female as a synthetic third variable is that no direct effects of the exogenous variables on it are permitted. (It has no errors independent of FPER and FOCC.)

The payoff to this analytic strategem is that it does permit one to assess the differential contributions of marginality variables through FPER and FOCC. Thus, from the basic theorems of path analysis, the total effect (in Alwin and Hauser's [1975] sense of the term) of an exogenous variable—say, X_3 on the total proportion female—can be represented thusly:

(2) $$\text{Total effect} = p_{Y_1X_3}\,p_{Y_3Y_1} + p_{Y_2X_3}\,p_{Y_3Y_2}$$

Moreover, the size of this total effect is equal to the standardized regression coefficient of Y_3 on X_3 in the reduced-form equation.

Scores for FOCC and FPER were derived from the following computational formulas:

(3) $$\text{FPER}_k = \overset{N}{\underset{i=1}{\Sigma^k}} f_{ki}O_{.i}, \text{ and}$$

(4) $\text{FOCC} = Y_3 - \text{FPER}$;
where:

i = detailed occupational class,
k = detailed industry class,
f_{ki} = proportion female in occupational class i in industry class k,
$O_{.i}$ = proportion of workers in occupation i in standard population,
Y_3 = crude proportion female, and
N_k = number of detailed occupational classifications represented in industry k.

Of prime importance is the fact that standardization was carried out using the most detailed 3-digit census occupational classification. As an example of the sensitivity of this scheme, within the clerical category, keypunch operators are distinguished from tabulating-machine operators. Using data categorized this finely is consistent with the argument that higher levels of occupational segregation by sex appear with more disaggregated data (see Gross, 1968). As a standard population, the aggregate occupational distribution of the 170 within-scope industries was used.[7]

In the subsequent analysis, separate equations are estimated for primary/secondary (goods-producing) and tertiary (service-producing) industries. The reasons for separating the two are both substantive and technical. First, productivity levels and the role of labor in achieving high productivity may be different in services than in goods-producing sections of the economy. This may be for the reasons cited by Fuchs (1968:195–8), or it may simply be that the relatively low level of mechanization in service industries means that firm size (and the attendant specialization and scale economies accompanying it) is a more important relative input to productivity in services than in goods-production. In other words, firm size may be a better indicator of productivity in the tertiary case where it is difficult to achieve the same effects with substitution of capital for labor.

---

[7] A technical difficulty concerned adjustments to be made when all detailed occupations were not represented in a specific industry. (This was generally the case.) The occupational proportions in the standard population were recalibrated for each specific industry, with the base consisting of the sum of all those members of the standard population in occupations which actually were represented in the industry in question. This produces results which are consistent with the assumption that "missing occupations" in a specific industry have a proportion female which is equal to the obtained adjusted proportion female.

If that were not sufficient reason, a separate analysis is also required by the facts that: (1) there are differences in the completeness of coverage mentioned earlier and (2) some measures which are readily available for primary/secondary industries are almost totally unobtainable for many tertiary industries. Thus, had the two sectors not been analyzed separately, the measures of new capital investment and value added could not have been included in the analysis of the goods-producing sector. Because marginal industries may be slightly underrepresented in the goods-production sector, analyzing the two subpopulations separately does raise the issue that the effects of marginality are underestimated by sundering two groups which might have been analyzed together. Results presented here, however, indicate that this underestimation is trivial.

Another methodological concern is whether the representation of industry structure as six distinct variables in a regression equation captures the "spirit" of the notion of industry marginality. Thus, a different conceptual and methodological tack might be taken in which a single unobserved variable representing marginality is included in the model with a maximum-likelihood technique being used to estimate, simultaneously, both measurement and causal models. This strategy has been rejected because of the difficulty of specifying a priori a measurement model appropriate to these aggregate data and because of the danger of affirming the consequent by assuming the presence of a *single* unmeasured variable. That is—a central theme of this paper is: that industry marginality is likely to be multidimensional; that little is known about what the different dimensions are; and that at this stage of theoretical development more is to be learned by proceeding with a simpler mode of analysis. In any event, a preliminary principal-components analysis of the independent variables used here resulted in the emergence of three separate underlying factors.[8]

The analysis reported in the next section is complicated by the additional factor that nearly all of the ratio-independent variables were affected by considerable positive skew. This was particularly true for measures of average company size, value added per worker, fixed assets per worker, and new investment per worker. The subsequent regressions were thus carried out with the ratio-independent variables (but not the control variables: e.g., percentage males who are nonwhite) entered in log form. The consequences of this respecification of the functional form are discussed here.

A final difficulty concerns the correlation to be made in median male earnings for weeks worked per year. This was accomplished by purging the median-wage variable of the effect of percentage working 50–52 weeks per year with a preliminary regression and entering the residual in the main equation. Again, an alternative formulation was also tried and is reported on in the "Results" section.

Thus, the two equations estimated for primary/secondary industries are as follows:

$$(5)\ Y_1 = a + b_1X_1 + b_2X_2 + b_3\ln X_3 + b_4\ln X_4 + b_5\ln X_5 + b_6\ln X_6 + b_7\ln X_7 + b_8\ln X_8 + b_9X_9 + b_{10}X_{10} + e_{Y1}, \text{ and}$$

$$(6)\ Y_2 = a + b_1X_1 + b_2X_2 + b_3\ln X_3 + b_4\ln X_4 + b_5\ln X_5 + b_6\ln X_6 + b_7\ln X_7 + b_8\ln X_8 + b_9X_9 + b_{10}X_{10} + e_{Y2};$$

where:

Y_1 = FPER or adjusted proportion female,

Y_2 = FOCC or occupational compositional effect,

X_1 = 1 if mining or construction industry, 0 otherwise,

X_2 = 1 if durable-goods industry, 0 otherwise,

---

[8] When the principal-components analysis was done for the entire industry sample—i.e., ignoring the distinction between goods and service producing—only five variables could be included in the analysis. While the three significant factors account for 81% of the variance, the first factor accounts for only 38%. Restricting the analysis to goods-producing industries, where a more complete set of variables is available, confirms this result: Three factors are extracted; the first accounts for 34% of the variance and the three taken together account for 72%.

X_3 = Number of employees/number of companies (H_1),
X_4 = Fixed assets in \$1,000/number of employees (H_2),
X_5 = Value added in \$1,000/number of employees (H_3),
X_6 = Sales of top 4 firms/total industry sales (H_4),
X_7 = Dissimilarity index of industry labor force and civilian labor force across regions (H_5),
X_8 = Median male earnings purged of effects of percent working full year (H_6),
X_9 = Number of nonwhite males in industry/number of males, and
X_{10} = Number of employees in South in industry/number of employees.

For service industries, identical procedures were followed, but the variable representing value added per worker was dropped due to missing data. In addition, for both sectors, the reduced-form equation was also reestimated—using the independent variables listed earlier, but replacing the crude proportion female with a new dependent variable which consisted of the residuals of the number of females in the industry predicted from the total number of workers.[9]

Results

In Table 1, means, standard deviations, and correlations of variables used in each of the two regression models are shown. Although the need for separate analyses of goods-producing and service-producing industries has been argued on technical and substantive grounds, it can also be advocated on empirical ones. First, the slight difference in their adjusted rates of female employment bears witness to the fact that the separate analyses are not hiding a large component of difference in female employment. Second, a preliminary analysis confirms that a significant increment in explained variance results from allowing for separate slopes within subpopulations.[10]

This in mind, we now examine the results in Table 2, where regression coefficients are presented for the models estimating FPER and FOCC in primary/secondary industries. Of the six original hypotheses, only H_2—that female employment is depressed in capital intensive industries—is strongly supported. Furthermore, the regression coefficient for median male earnings is in the predicted direction for both components, although it is considerably smaller than that for fixed assets. The signs of the regression coefficients for the variables used to operationalize the other hypotheses are generally inconsistent across the two

---

[9] This was done because the marginal industry thesis is used here to make predictions about the *relative* utilization of female labor, and the dependent variables are expressed as ratios of the number of women to the total number of employees in a particular industry. Most independent variables also entered the equation in ratio form—e.g., new investment was measured as new capital spending per worker; company size was specified as number of workers per firm; percentage of workers in the South was measured as number of workers in the South divided by total industry total employment in all regions; etc. Consequently, these procedures fly in the face of some recent statistical warnings about regression of ratio variables with common terms (Schuessler, 1974; Fuguitt and Lieberson, 1974). Thus, to examine what irregularities had been introduced by the inclusion of variables with common terms, an additional specification was examined for each sector. Rather than express the dependent variable in the reduced form as a ratio, the number of women workers (the term that had formerly been in the numerator) was regressed on the number of workers (the term that had formerly been in the denominator), and the residuals of this equation were used as the new dependent variable. This procedure has the advantage of creating a dependent variable which is purged of size effects and which, therefore, is unlikely to have a spurious correlation with an independent variable in which size is the denominator. It has the disadvantage of building equations which are difficult to interpret, as the new zero-order correlations are, in reality, part correlations of ratio variables with number of females controlled for size of industry. However, the purpose of this procedure is merely to provide a point of comparison for the relative importance of variables in the initial specification and interpretations of significance levels and explained variance for this secondary model can safely be held in abeyance.

[10] The test performed here is described by Johnston (1972:198–9) and is similar to the well-known Chow test except that it isolates the sum of squares due to differential slopes from that due to differential intercepts. A significant reduction in error was obtained by allowing for differential slopes when the dependent variable was the total percent female and when it was either of the two components of that total.

Table 1. Means, Standard Deviations, and Correlations of Variables in Regression Analysis

Variable	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	Mean	S.D.
1. Adj. Prop. of Workers who are Female (FPER)	1.000	.748	—	—	—	—	−.229	−.191	−.073	−.299	.126	−.039	−.166	−.264	−.291	.142	.328	.085
2. Occupational Composition Effect (FOCC)	.859	1.000	—	—	—	—	−.001	−.034	.262	−.112	.178	.102	−.189	−.081	−.277	.105	−.010	.113
3. Mining or Construction Industry	−.429	−.272	1.000	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
4. Durable-Goods Industry	−.129	−.266	−.271	1.000	—	—	—	—	—	—	—	—	—	—	—	—	—	—
5. Percentage Med. and Large Estabs. (log)	.194	.318	−.540	.018	1.000	—	—	—	—	—	—	—	—	—	—	—	—	—
6. Value Added Per Worker (log)	−.410	−.288	.126	−.281	.085	1.000	—	—	—	—	—	—	—	—	—	—	—	—
7. 4-Firm Sales Concent. Ratio (log)	−.057	.088	−.218	.043	.440	.201	1.000	−.044	.648	.338	.242	−.043	.096	.638	.195	−.462	2.59	1.20
8. Regional Concentration (log)	.168	.209	−.171	.175	.280	−.102	.310	1.000	−.035	.298	.080	−.053	−.297	.171	−.149	.012	1.87	.687
9. Average Firm Size (log)	.035	.156	−.401	.149	.792	.146	.436	.364	1.000	.278	.323	−.041	−.090	.546	.120	−.190	2.25	1.31
10. Fixed Assets Per Worker (log)	−.640	−.463	.160	−.172	.202	.711	.247	−.087	.221	1.000	−.108	−.078	.044	.742	−.270	−.239	2.10	1.07
11. Adjusted Median Male Wage	−.279	−.362	.284	.229	−.374	.332	.305	−.094	−.008	.060	1.000	−.245	−.468	.137	.118	−.297	0.00	968.7
12. Proportion Males who are Nonwhite	−.153	−.069	−.138	−.116	−.098	−.128	−.024	.040	−.153	.023	−.356	1.000	−.047	−.004	−.015	−.179	.077	.040
13. Proportion of Workers in South	−.058	.109	.109	−.382	.032	−.080	−.007	−.102	.007	.141	−.515	.436	1.000	.058	−.047	.074	.292	.051
14. Trans., Communication, or Utility	—	—	—	—	—	—	—	—	—	—	—	—	—	1.000	−.247	−.292	.143	.353
15. Wholesale Trade	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1.000	−.433	.268	.447
16. Retail Trade	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1.000	.339	.478
Mean	.289	−.036	.075	.475	3.68	2.58	−1.19	−1.67	4.56	2.28	0.00	.093	.292	—	—	—	—	
S.D.	.074	.092	.265	.502	.566	.335	.808	.619	1.25	.809	986.0	.047	.179	—	—	—	—	—

Note: Tertiary Industries in upper triangle (N = 56); Primary-Secondary Industries in lower triangle (N = 80).

components, and their sizes are relatively small.[11]

The results of this model also reveal large underrepresentations of women in mining and construction as well as in durable-goods manufacturing. An appropriate question is whether these differences occur because of the lesser "marginality" of these sectors or for some other reason. By looking at the differences between them and the comparison group, nondurable-goods manufacturing, before and after industry marginality variables are added to the equation, informative comparisons are made possible. That is, if the scarcity of women is attributable to the fact that these sectors are less marginal, there ought to be large zero-order differences (between them and the comparison group) which disappear when controls for marginality factors are added.

By adding equations (5) and (6), it is readily seen that the net differences in *total* female employment between one group and another comparison group can be obtained by adding the two unstandardized coefficients. Thus, for durable-goods manufacturing, the regression models in Table 2 indicate that the proportion of women in these industries is 13 percentage points less than that in the comparison group. In contrast, the difference *before* measures of marginality are added is actually less, about 10.6%. Thus, the numerical importance of men in the durable manufacturing sector is probably not a reflection of the lesser marginality of these industries—or the differences would have decreased with the addition of controls.

When mining and construction industries are compared to nondurable-goods manufacturing, the results are different. For this comparison, the zero-order differences are larger than those that survive the controls for marginality—27% vs. 17% as shown in Table 2. Thus, in part, the

---

[11] The usual interpretation of F-ratios is not technically justified since the industries studied were "chosen" on the basis of availability of data, not through probability techniques. In fact, the data are intended to comprise the universe of industries—not a sample of it. The reason F levels are presented is that they are, for any given equation, a linear transformation of squared-part correlations or "unique" variance explained.

Table 2. Regression Results for Primary/Secondary Industries

	Dependent Variable					
	FPER (adjusted rate)			FOCC (compositional effect)		
Variable	B	Beta	F	B	Beta	F
Mining or construction Industry	−.123	−.437	24.4	−.047	−.136	1.49
Durable-goods Industry	−.058	−.391	21.2	−.072	−.393	13.5
Log Average Firm Size	−.001	−.022	.065	.015	.200	3.24
Log Fixed Assets Per Worker	−.052	−.565	27.6	−.067	−.587	18.8
Log Value Added Per Worker	−.008	−.038	.106	.019	.069	.029
Log 4-Firm Concentration Ratio	−.004	−.043	.312	.006	.052	.287
Log Regional Concentration	.015	.122	2.59	.012	.078	.669
Adjusted Male Earnings	-9.3×10^{-6}	−.123	1.56	2.4×10^{-5}	−.263	4.49
Proportion Males Who Are Nonwhite	−.457	−.288	14.1	−.340	−.173	3.20
Proportion Workers in South	−.015	−.037	.167	−.004	−.008	.005
	N = 80	R^2 = .717		N = 80	R^2 = .552	

seeming propensity of these industries (mining and construction) to hire men is associated with their lesser marginality. It must be emphasized that not all, or even the larger part, of these differences are attributable to the effects of marginality. The residual effects of mining or construction, a difference of 17 percentage points, is one of the larger effects in the model and is due to factors other than those generally used to define "core" industries.

Additional insight into these relationships can be obtained from Table 3, which decomposes the total associations (correlations) between predetermined variables and the crude proportion female into several meaningful parts. Column 1 reports the total association. In column 3, the total *effects* of prior variables are shown; these are equivalent to effects in the reduced-form equation predicting the crude proportion female. Column 2, which is the difference between columns 1 and 3, refers to unexamined correlation attributable to joint effects of independent variables. Column 4 shows the total paths between independent variables and the crude proportion female which pass through FPER (or the adjusted proportion

Table 3. Decomposition of Effects and Selected Model Coefficients for Primary/Secondary Industries

I.	Decomposition of Effects				
	Effects				
Variable	(1) Total Association	(2) Unexam. Correlation	(3) Total	(4) Through FPER	(5) Through FOCC
Log Fixed Assets Per Worker	−.5634	.0365	−.5999	−.2622	−.3377
Log Average Firm Size	.1060	.0015	.1045	−.0105	.1150
Log Value Added Per Worker	−.3561	−.3783	.0222	−.0176	.0398
Adjusted Male Earnings	−.3380	−.1295	−.2085	−.0573	−.1512
Log 4-Firm Concentration Ratio	.0242	.0143	.0099	−.0199	.0298
Log Regional Concentration	.1980	.0967	.1013	.0565	.0448
Mining or Construction Industry	−.3553	−.0736	−.2817	−.2032	−.0785
Durable-Goods Industry	−.2126	.1946	−.4072	−.1814	−.2258
Proportion Workers in South	.0359	.0577	−.0219	−.0171	−.0048
Proportion Males Who Are Nonwhite	−.1110	.1221	−.2331	−.1336	−.0995

II. Selected Model Coefficients

$p_{Y_1 \cdot u} = .5320$

$p_{Y_2 \cdot v} = .6594$

$r_{uv} = .7742$

$p_{Y_3 Y_1} = .4644$

$p_{Y_3 Y_2} = .5751$

female), while column 5 indicates effects which are mediated by the occupational distributions of the various industries.

Of particular interest in this table are the findings for average firm size and 4-firm concentration ratio. Although the absolute values of these effects are small, the signs exhibit an interesting pattern. Both of these measures of marginality can be taken to represent the scale of operations of an industry. What these results show is that, as this scale increases, the occupational distributions of industries change in a way that tends to increase the number of female workers. At the same time, these changes have only small effects, in the opposite direction, on female employment within occupation categories. The first result upholds the earlier speculation that increased size is associated with increases in bureaucratization and the presence of female clerical workers. What is now apparent is that this tendency swamps out the extremely small countertendency for large-firm, concentrated industries to favor male workers within occupations.

Another revealing finding is that industry marginality variables which do have effects in the predicted direction on the crude proportion female—viz., capital intensity and male earnings—have the larger part of their influence through occupational structure, and not directly, on rates of female employment within jobs. This is evidence that a substantial part of the explained differences in female employment which are seemingly due to marginality factors are also susceptible to the influence of occupational sex-stereotyping, some of which certainly takes place at the societal level, not at the industrial level. In other words, much of the reason that women are underrepresented in high-paying and capital-intensive industries is their relative exclusion from the kinds of occupations (craft and/or professional?) which occur disproportionately in these industries. However, in both cases, there are smaller, reinforcing influences (in the same direction) on female employment within occupation categories in these industries, as well.

One surprising aspect of these findings is that the underrepresentation of women in mining and construction comes about mostly because of within-occupation deficits, not because of the occupational structures of these industries. The probable explanation is cultural; both of these industry groups are bastions of masculinity in the labor force and probably use men wherever feasible. Thus, in mining and construction, 6% of professionals are women, 57% of clerical workers are women, and 1% of laborers are women. The comparable percentages in manufacturing are 13, 62, and 15.

Because a number of potentially troublesome analytic decisions had to be made in carrying out this analysis, some measure of skepticism is perhaps warranted by these findings. That is—the dependent variables were expressed as a proportion; the key independent variables were specified in log form; and minority male employment was treated as an exogenous variable. Would other choices have produced more support for the marginality thesis?

The answers to these questions can be determined only by examining a number of alternative specifications of the basic model. In the interest of parsimony, comparative results will be reported only for the reduced-form equation. As mentioned above, an alternative to specifying the dependent variable in ratio form is to express it as a residual from the equation which predicts—from the total number of employees—the number of employed females. When this is done, average firm size and value added per worker shift from small-positive to small-negative effects; but these changes are counterbalanced by increased positive effects for the two concentration measures and by a substantially weakened effect for capital intensity.[12]

Additional comparisons can be made for models with modified independent variables, such as one which specifies pay levels by the concurrent use of median male earnings and proportion working full year as independent regressors. This change slightly enhances the negative effects on female employment of high male earnings, relative to other variables in the model, but their net effects are unchanged.[13] Alternative measures of employer size and capital intensity are available and these can also be used to verify the results. When proportion of medium and large establishments is substituted for average firm size, very little is changed. This measure of employer size has slightly greater *positive* effects on female employment than average firm size, but the other effects are undisturbed.[14] Returning average firm size to the equation shows new investment to be a somewhat less direct indicator of capital intensity, as it has slightly smaller relative effects than did fixed assets. Again, the effects of the remaining variables are unchanged.[15]

---

[12] The standardized regression coefficients in this equation are as follows (controlling for industry type, proportion of males nonwhite, and proportion of employment in South): 4-firm concentration ratio = .035; regional concentration = .148; average firm size = −.032; value added per worker = −.116; median male earnings (adjusted) = −.085; fixed assets per worker = −.142.

One additional bit of tinkering with the independent variables in this model involves entering measures of marginality in "pure" rather than "log" form. The results of this are similar to those of specifying the dependent variable as a residualized variable. The proportion of variance explained by the total model drops to .49; the relative importance of capital intensity is diminished; and the effects of average firm size and value added per worker change from small-positive to small-negative values.[16] These changes seem to result from the decreased zero-order correlation between the nonlog version of fixed assets per worker and proportion of employees who are female (−.56 to −.38). A plausible explanation of this drop is that, when the dependent variable is expressed as a proportion, additional increases in male employment with increasing capital intensity are subject to ceiling effects so that the curvilinear form is a better functional fit.

Finally, to guard against the possibility of a drastic error in model specification, a model was constructed in which the proportion of males who are nonwhite was treated as an endogenous variable and reciprocal causation between this variable and percent female was allowed. An additional independent variable—median years of schooling of male employees—was added to the model, to assist in identification of the model. That is, low educational attainments (as a proxy for requirements) were assumed to contribute to the percentage of nonwhites employed, though not to the percentage of females employed. (In 1973, the median years of school completed for white males was 12.4, for *all* females was 12.2, and for black males was 10.3.) The results support exactly the same conclusions as the results presented in Table 2.[17]

---

[13] The standardized regression coefficients in this equation are as follows: 4-firm concentration ratio = .009; regional concentration = .089; average firm size = .105; value added per worker = .029; median male earnings = −.295; proportion working full year = .194; fixed assets per worker = −.602. Some would argue that another *defining* characteristic of marginal industries is their propensity to provide unstable employment and that the "weeks worked" variable also should be entered in the model. There are two difficulties with this. First, doing so muddies the waters conceptually, insofar as employment instability generally is taken as a characteristic of secondary labor markets which are analytically distinct from peripheral or marginality industry structures. The second difficulty is that its effects are positive on female employment. The higher the proportion of men who work full year in the industry, the more likely women are to be employed there.

[14] The standardized regression coefficients in this equation are as follows: 4-firm concentration ratio = .004; regional concentration = .106; proportion medium and large establishments = .167; value added per worker = .016; median male earnings (adj. for percent working full year) = −.135; fixed assets per worker = −.616. As is the case in the model reported in footnote 11, controls were present for mining and construction, durable-goods manufacturing, proportion of employment in the South, and proportion of male employees who are nonwhite.

[15] Here the standardized regression coefficients are as follows: 4-firm concentration ratio = .049; regional concentration = .090; average firm size = .078; value added per worker = −.139; median male earnings = −.130; new capital investment per worker = −.491. The negative effects of value added per worker seem to be a direct consequence of the weaker effects of capital intensity.

[16] The standardized regression coefficients for the variables of interest are as follows: 4-firm concentration ratio = .060; regional concentration = .125; average firm size = −.084; value added per worker = −.160; median male earnings = −.151; fixed assets = −.283.

[17] The standardized regression coefficients for the complete structural form for females were as follows: 4-firm concentration ratio = .009; regional concentration = .107; average firm size = .076; value added per worker = .008; median male earnings =

Table 4. Regression Results for Tertiary Industries

	Dependent Variable					
	FPER (adjusted rate)			FOCC (compositional effect)		
Variable	B	Beta	F	B	Beta	F
Wholesale Trade Industry	−.121	−.635	15.3	−.150	−.593	13.5
Retail Trade Industry	−.057	−.322	3.17	−.034	−.074	.654
Transportation, Communication, or Utility	−.074	−.310	1.49	−.205	−.639	6.39
Log Average Firm Size	.016	.243	1.92	.050	.576	10.9
Log Fixed Assets Per Worker	−.024	−.304	1.99	.001	.008	.001
Log 4-Firm Concentration Ratio	−.006	−.093	.213	.007	.074	.135
Log Regional Concentration	−.024	−.198	2.24	−.003	−.018	.018
Adjusted Male Earnings	-5.2×10^{-6}	−.059	.124	8.6×10^{-4}	.075	.181
Proportion Males Who Are NonWhite	−.341	−.160	1.30	.302	.106	.581
Proportion Workers in South	−.343	−.204	1.87	−.201	−.090	.365
	N = 56	R^2 = .386		N = 56	R^2 = .392	

While further methodological manipulations are possible—e.g., changing to a residualized dependent variable and nonlog independent variables at the same time—the point of diminishing returns has certainly been reached, if not crossed. The preponderance of evidence shows high levels of fixed assets to have a strongly depressive effect on female employment in these sectors and shows high male earnings to have a mildly inhibiting influence in the same direction. However, more sweeping assertions about the relation of female employment to a broad range of marginality characteristics are definitely unsupported. In particular, the variables that measure the scale of operations of an industry—the number of workers per firm, market concentration, and geographic centralization—are only trivially related to total female employment in the predicted direction. In short, for goods-producing industries, the marginality thesis is only a partial truth. What seems especially misrepresentative about the use of "marginality" as a metaphor is the vision it conjures of female employers: petty capitalists tucked away in the nooks and crannies of the industrial structure and—due to the constant pressures of unrestrained competition, merely eking out a living.

As often noted, service industries, particularly those in the trade and service sectors, are higher on most dimensions of marginality than manufacturing industries. However, because there is a lot of internal variability in this sector (its gamut includes department stores and shoe repair shops, electric utilities and dairy stores), the key question is whether differences in industry marginality are associated with differences in the proportion of employees who are women.

Table 4 provides the information needed to examine five of the six hypotheses that have been proposed in the introduction. The relationship between value-added and female employment in tertiary industries cannot be investigated because of inadequate data.

−.216; fixed assets per worker = −.580; percent males who are minority = −.226; mining or construction industry = −.307; and durable-goods industry = −.412.

The comparable coefficients for the structural form predicting nonwhite male employment were: 4-firm concentration ratio = .011; regional concentration = −.120; average firm size = .008; value added per worker = .198; median male earnings = .232; fixed assets per worker = −.058; proportion of workers who are female = −.002; proportion of workers in South = .258; median years of education for males = −.761 and mining or construction industry = −.44.

As can be seen, the equation for nonwhite male employment was identified by excluding the durable-goods manufacturing variable and the equation for female employment was identified (in fact, overidentified) by excluding median years of education and proportion of workers in the South. It is interesting that, when nonwhite and female employment are allowed to be simultaneously determined, industries employing nonwhites also fail to fit the marginality stereotype.

What is apparent is that there is even less support for the marginality theory in this sector than in goods-producing industries. Hypothesis H_1 is completely controverted by the finding that increases in the number of employees per firm lead to increases in the adjusted proportion of women employed in an industry and to large increases in the "femininity" of the occupational composition of these industries. Regional concentration has consistently negative effects on the proportion of females employed, lending some support to H_5; but the effects are rather small. The other three marginality variables have effects whose signs are inconsistent across the two component variables and, with the exception of the effect of log-fixed assets on the adjusted proportion female, effects which are quantitatively minute.

The dummy variables for wholesale trade (and for transportation, communication, and utilities) have large negative effects, particularly on the occupational-composition component. For wholesale trade, the unstandardized effect in the reduced form is −.121 plus −.150 or −.271 (the excluded category is finance and services) which exceeds the zero-order difference of −.189. For transportation, communication, and utilities, the partial effect is −.279 which is larger than the zero-order effect of −.169. In essence, these comparisons indicate that, if these industries were further from the "core" in terms of firm size, etc., they would have even fewer female employees than is now the case.

As in goods-producing industries, the effects of these different aspects of marginality can be separated into their components. Thus, industries with larger average firm sizes employ higher proportions of women workers, an increase which primarily results from the differences in the occupational structure of large-firm industries (see Table 5). Another interesting sidelight to this analysis is that industries with high levels of fixed assets, with relatively high male earnings, and with high product-market concentration, all have fewer women workers than would be expected after adjustments are made for their occupational distributions, but that their occupational distributions tend to favor female workers. With the exception of industries with high levels of assets, these tendencies tend to cancel one another. There are also consistent tendencies for geographically centralized tertiary industries to employ fewer women.

As with goods-producing industries, these results must be evaluated against several threats to their validity. The two respecifications which have the greatest

Table 5. Decomposition of Effects and Selected Model Coefficients for Tertiary Industries

I.	Decomposition of Effects				
			Effects		
Variable	(1) Total Association	(2) Unexam. Correlation	(3) Total	(4) Through FPER	(5) Through FOCC
Log Fixed Assets Per Worker	−.2054	−.0701	−.1343	−.1393	.0050
Log Average Firm Size	.1266	−.3362	.4628	.1113	.3515
Log 4-Firm Concentration Ratio	−.1059	−.1083	.0024	−.0427	.0451
Log Regional Concentration	−.1086	−.0070	−.1016	−.0908	−.0108
Adjusted Male Earnings	.1665	.1481	.0184	−.0273	.0457
Transportation, Communication, or Utility	−.1704	.3615	−.5319	−.1418	−.3901
Wholesale Trade Industry	−.3203	.3503	−.6526	−.2906	−.3620
Retail Trade Industry	.1288	.3650	−.2362	−.1475	−.0887
Proportion Workers in South	−.1912	−.0430	−.1482	−.0934	−.0548
Proportion Males Who Are NonWhite	.0446	.0530	−.0084	−.0732	.0648
II.	Selected Model Coefficients				
	$p_{Y_1 \cdot u} = .7836$		$p_{Y_3 Y_1} = .4580$		
	$p_{Y_2 \cdot v} = .7794$		$p_{Y_3 Y_2} = .6101$		
	$r_{uv} = .7607$				

potential for upsetting the findings—i.e., changing the dependent variable to a residualized variable and entering the independent variables in nonlog form—produce even less support for the marginality thesis. When the residualized dependent variable is used, the moderately negative effect of fixed assets becomes positive, and when independent variables are entered in "raw" form, the effect of industry concentration becomes substantially positive. Thus, the results presented in the preceding tables, compared to those emerging from alternative model specifications, are ones that tend to case the marginal industry thesis in a relatively favorable light, dim as it may be.

Finally, let us consider the possibility that the findings would change if primary/secondary industries and tertiary industries were analyzed together. In a separate regression analysis where this change was made, the results confirm the previous conclusions. Having a large average firm size is moderately *positively* correlated with the employment of women (primarily through changes in occupational structure) and high levels of fixed assets are strongly negatively associated with female employment. Low-wage industries have slightly depressed levels of female employment and there is essentially no relation between female employment and economic or regional concentration.

Summary and Conclusions

A total of six hypotheses relating female employment to industry marginality have been examined with strong support emerging for one—women are less likely to be employed in primary and secondary industries with high levels of fixed assets per worker. There are slight tendencies for women to be underrepresented in primary/secondary industries where males have high earnings and in tertiary industries that are regionally concentrated or capital intensive. These results are important because they are found at a more refined level of aggregation than is typical of studies of this type and because they take into account the occupational structuring of the industries in question.

Along these lines, it is significant that the effects of marginality characteristics on the proportion female in an industry are often transmitted by the occupational structure. In some instances, such as in the relationship between average firm size and the "femaleness" of the occupational structure, the direction of the effect is opposite to the prediction made by the marginal-industry thesis. In others, the direction is consistent with the hypothesis, such as the deficit in female employment which corresponds to the occupational structuring of capital-intensive industries. In theory, therefore, a large component of this industrial patterning of employment by sex is susceptible to changes in the level of occupational segregation by sex. What this study indicates, though, is that a completely egalitarian occupational structure would have a mixed impact on women workers. On balance, they would enjoy gains by entering capital intensive industries in greater numbers; but they would encounter some industrial level setbacks as they would become more heavily represented in small-firm industries in the tertiary sector.

The finding that certain types of industries pose major barriers to female employment merits further attention. This is particularly true because these differences persist after controls for industry marginality have been made. That is—the continued relative shortage of women in wholesale trade; transportation, communication, and utilities; durable-goods manufacturing; and mining and construction is not because these industries are less marginal but for some other reason. For the first three groups in this list, the influx of women would be enhanced by more egalitarian occupational opportunities. However, in the case of mining and construction, the major part of the relative absence of women workers is not explained by the "maleness" of the occupational distribution but by their systematic exclusion across all job levels—perhaps through some combination of tradition and present discrimination.

In conclusion, two separate explanations can be offered for the failure of most measures of industry marginality to predict levels of female employment. One is that, in the economy, women do not play the role which is attributed to them by the theory of marginality—i.e., being a readily available group whom "core" employers regard as second best, and who are passed over in hiring by those with the resources to get their first choice, prime-age males. Instead, these findings depict a different portrait of women in the economy—a group who are as likely as men to be employed by the industrial "elite." And while goods-producing industries do vary in the extent to which they employ women, these variations reflect factors unrelated to an industry's marginality in the economy. Furthermore, in service industries, there is some evidence that powerful industries, as indicated by large and average firm sizes, choose women before men. In other words, if we accept a queue theory of employment, we can infer that, for service employers, women have some positive qualities that place them ahead of men in the line.

The second explanation suggests that there are different types of marginal industries and that there are different groups (segments) in the labor force "assigned to" each. According to this alternative, women might be described as that part of the secondary labor force which is suitable for industries which are high on the labor-intensive dimension of marginality. Or, more speculatively, it might be argued that nonwhites are the part of the secondary labor force which is allocated to industries which are marginal in the sense of being unconcentrated and decentralized. Note that this alternative is less thoroughgoing than some others, in that it does not require one to abandon basic premises about women as employees and about employers' beliefs concerning women as employees. That is—women can still be regarded as a second-best group; but we change our thinking about whom they are second-best "for" by complicating our definition of industry marginality.

While each of these alternatives suggests areas for additional research, both require new empirical *and* theoretical initiatives. Thus, it is unlikely that more powerful conclusions about the industrial role of women will emerge until we stop charging our theoretical batteries with the weak current of "core" and "periphery."

REFERENCES

Alwin, Duane F. and Robert M. Hauser
1975 "The decomposition of effects in path analysis." American Sociological Review 40:37–47.

Averitt, Robert F.
1968 The Dual Economy: The Dynamics of American Industry Structure. New York: W. W. Norton.

Beck, E. M., Patrick M. Horan, and Charles M. Tolbert, II
1978 "Stratification in a dual economy: a sectoral model of earnings determination." American Sociological Review 43:704–20.

Bibb, Robert and William Form
1977 "The effect of industrial, occupational, and sex stratification on wages in blue-collar markets." Social Forces 55:974–94.

Blau, Francine D., and Carol Jusenius
1976 "Economists' approaches to sex segregation in the labor market: an appraisal." Pp. 181–99, in Martha Blaxall and Barbara Reagan (eds.), Women and the Workplace. Chicago: University of Chicago Press.

Blauner, Robert
1964 Alienation and Freedom. Chicago: University of Chicago Press.

Blaxall, Martha and Barbara Reagan (eds.)
1976 Women and the Workplace: The Implications of Occupational Segregation. Chicago: University of Chicago Press.

Bluestone, Barry
1968 "Lower-income workers and marginal industries." Pp. 273–302 in Louis Ferman, Joyce Kornbluth, and Alan Haber (eds.), Poverty in America. Ann Arbor: University of Michigan.

Bluestone, Barry, William Murphy, and Mary Stevenson
1973 Low Wages and the Working Poor. Ann Arbor: Institute of Labor and Industrial Relations, University of Michigan.

Cain, Glen G.
1976 "The challenge of segmented labor market theories to orthodox theory: a survey." Journal of Economic Literature 14:1215–57.

Clark, Colin
1951 The Conditions of Economic Progress. 2nd ed. London: Macmillan.

Dalton, James A. and E. J. Ford, Jr.
197 "Concentration and labor earnings in manufacturing and utilities." Industrial and Labor Relations Review 31:45–60.

Edwards, Richard C.
197 "The social relations of production in the firm and labor market structure." Pp. 3–26 in Richard C. Edwards, Michael Reich, and David Gordon (eds.), Labor Market Segmentation. Lexington, Mass.: D.C. Heath.

Fuchs, Victor
1968 The Service Economy. New York: National Bureau of Economic Research.

Fuguitt, Glenn and Stanley Lieberson
1974 "Correlation of ratios or difference scores

having common terms." Pp. 128–44 in Herbert L. Costner (ed.), Sociological Methodology, 1973–1974. San Francisco: Jossey-Bass.

Fusfeld, Daniel R.
1973 The Basic Economics of the Urban Racial Crisis. New York: Holt, Rinehart, and Winston.

Gordon, David M.
1972 Theories of Poverty and Underemployment. Lexington, Mass.: Lexington Books.

Gross, Edward
1968 "Plus ca change . . .? The sexual structure of occupations over time." Social Problems 16:198–208.

Johnston, J.
1972 Econometric Methods. 2nd ed. New York: McGraw-Hill.

Lewis, H. G.
1963 Unionism and Relative Wages in the United States. Chicago: University of Chicago Press.

Lyle, Jerolyn and Jane Ross
1973 Women in Industry. Lexington, Mass.: Lexington Books.

O'Connor, James
1973 The Fiscal Crisis of the State. New York: St. Martins.

Oppenheimer, Valerie K.
1970 The Female Labor Force in the United States. Berkeley: University of California Press, Population and Monograph Series.

Scherer, F. M.
1970 Industrial Structure and Market Performance. Chicago: Rand McNally.

Schuessler, Karl
1974 "The analysis of ratio variables: opportunities and pitfalls." American Journal of Sociology 80:379–96.

Snyder, David and Paula Hudis
1976 "Occupational income and the effects of minority competition and segregation: a reanalysis and some new evidence." American Sociological Review 41:209–34.

Stolzenberg, Ross M.
1975 "Occupations, labor markets and the process of wage attainment." American Sociological Review 40:645–65.

U.S. Internal Revenue Service
1970 Statistics of Income—1967 Business Tax Returns. U.S. Government Printing Office: Washington, D.C.
1971 Statistics of Income—1967 Corporation Income Tax Returns. U.S. Government Printing Office: Washington D.C.

U.S. Bureau of the Census
1968 County Business Patterns, 1967. U.S. Summary CBP-67-1. U.S. Government Printing Office: Washington, D.C.
1970 Census of Business, 1967. Vol. V—Selected Services Area Statistics. U.S. Government Printing Office: Washington, D.C.
1971a Census of Business, 1967. Vol. I—Retail Trade Subject Reports. U.S. Government Printing Office: Washington, D.C.
1971b Census of Business, 1967. Vol. III—Wholesale Trade Subject Reports. U.S. Government Printing Office: Washington, D.C.
1971c Census of Manufactures, 1968. Vol. 1—Summary and Subject Reports. U.S. Government Printing Office: Washington, D.C.
1971d Census of Population, 1970. Vol. I—Characteristics of the Population; Part I—U.S. Summary. U.S. Government Printing Office: Washington, D.C.
1972a Census of Population, 1970. Industrial Characteristics. Final Report PC(2)-7B. U.S. Government Printing Office: Washington, D.C.
1972b Enterprise Statistics, 1967. Vol. I—Subject and Special Statistics. U.S. Government Printing Office: Washington, D.C.
1976a Census of Manufactures, 1972. Vol. I—Subject and Special Statistics. U.S. Government Printing Office: Washington, D.C.
1976b Census of Retail Trade, 1972. Vol. I—Summary and Subject Statistics. U.S. Government Printing Office: Washington, D.C.
1976c Census of Selected Services, 1972. Vol. I—Summary and Subject Statistics. U.S. Government Printing Office: Washington, D.C.
1976d Census of Wholesale Trade, 1972. Vol. I—Summary and Subject Statistics. U.S. Government Printing Office: Washington, D.C.

U.S. Department of Labor, Bureau of Labor Statistics
1977 Directory of National Unions and Employee Associations, 1975. U.S. Government Printing Office: Washington, D.C.

Villemez, Wayne
1977 "The functional substitutability of blacks and females in the labor market: a closer look." Sociological Quarterly 18:548–63.

Weiss, Leonard W.
1966 "Concentration and labor earnings." The American Economic Review 56:96–117.

Williams, Gregory
1976 "Trends in occupational differentiation by sex." Sociology of Work and Occupations 3:38–62.

THE FLEMISH MOVEMENT IN BELGIUM AFTER WORLD WAR II: A DYNAMIC ANALYSIS*

FRANÇOIS NIELSEN
University of Chicago

American Sociological Review 1980, Vol. 45 (February):76–94

This paper argues that two theoretical perspectives which have been used to explain resurgences of ethnic solidarity in modern societies, the *reactive ethnicity* model and theories of solidarity based on mechanisms of *competition* between groups for resources, are based on sets of incompatible assumptions and lead to opposite predictions with respect to the social circumstances under which ethnic resurgences occur in modern societies. Predictions of the reactive ethnicity and competition theories are tested by using as an indicator of ethnic solidarity the vote for the Flemish movement in Belgium after World War II, measured at the canton level. Hypotheses about the social bases of support for the Flemish movement are incorporated into a dynamic model of political mobilization represented by linear differential equations. The differential equations model is estimated using a pooling of cross-section and time-series data technique which is designed to correct for both the autocorrelation of disturbances and heteroscedasticity. Results with respect to the social bases of support for the Flemish movement reveal positive effects on ethnic solidarity of income and employment in the tertiary sector, no effect of employment in the primary sector, and a negative effect of industrialization. Overall, the empirical analysis lends support to the competition theories of solidarity and disconfirms the reactive ethnicity model. The dynamic nature of the mobilization process is also found to differ systematically across parties.

1. INTRODUCTION

In much of the sociological literature, the fate of ethnic and other ascriptive distinctions based on caste, race or religion is viewed as one of ultimate dissolution in modern societies. Systems of social organization based on ethnic distinctions, it is said, correspond to a relatively primitive level of development and are poorly adapted to the requirements of modernity. Therefore, they are doomed to be replaced by more adequate institutions during the modernization process. Both the functionalist and Marxist traditions agree with this prediction.

The functionalist argument, outlined by Lipset and Rokkan (1967:1–64), has also been labelled the *diffusionist* (Hechter, 1975) or *developmental* (Ragin, 1977) perspective. The functionalist model assumes that elements of the modernization process such as the spread of a market economy and bureaucratization entail the precedence of universalistic criteria that cut across ascribed statuses such as ethnicity. With modernization, the salience of ethnicity as a factor of the system of stratification and political cleavages is progressively eroded. Ethnic distinctions are superseded by more functional distinctions and integration mechanisms. This position is quite typical of the social change literature (Berger, 1971).

Marx's (1964) scheme of the historical evolution of capitalist societies implies similar predictions. Ethnic cleavages are particularly salient in a system of communal property, an early stage of economic organization (Marx, 1964:69). In the communal property system, the main factor of production is land collectively owned by the community, and tribal or

---

* Direct all communications to: François Nielsen; Department of Sociology; University of Chicago; Chicago, IL 60637.

I am grateful to Michael Hannan, Rachel Rosenfeld, Jacques Delacroix, Charles Ragin and an anonymous reviewer for useful comments on earlier versions of the materials presented in this paper. The opinions presented here are the sole responsibility of the author. An earlier version of this article was read at the 9th World Congress of Sociology in Uppsala, Sweden, August, 1978. The development of the estimation algorithm and collection of the data were helped by several institutions and individuals: NSF grant GS-32065, NIE contract 74-0123, Michael Aiken, Pierre Lenain, Pat Barchas, Mark Van Naelten, the Belgian Archives for the Social Sciences, the Consulate General of Belgium in San Francisco and the Ministry of the Interior.

ethnic loyalty is therefore an essential precondition of production. The rise of capitalism implies the progressive separation of the factors of production (land, labor, capital) and decreases the importance of community membership as a precondition. Capitalism also entails a progressive polarization of individuals into two classes defined with respect to the ownership of the means of production which irons out traditional cleavages based on ethnicity. Therefore, the salience of ethnic distinctions should decrease with modernization according to the Marxist model.

Both the functionalist and Marxist arguments predict that the salience of ethnic distinctions decreases with modernization.[1] Researchers writing outside the functionalist and Marxist traditions, such as Park (1950), often accept this view. Park's optimistic description of the race relations cycle suggests that ethnic boundaries ultimately disappear in all situations of permanent contact between groups. However, such predictions do not agree with the empirical evidence of resurgences of ethnicity in such advanced countries as Canada, Great Britain, France, Yugoslavia and Belgium. The implications of several influential theories are clearly unacceptable in view of the historical evidence.

The purpose of this paper is to investigate conditions under which there can be a resurgence of ethnic solidarity in modern states. In Section 2, I contrast the basic assumption of two classes of theories which predict such resurgences: the *reactive ethnicity* model proposed by Hechter (1975) and the *competition* theories of ethnic solidarity developed by several writers in a variety of substantive contexts (see the references in Section 2). In Section 3, I outline the history of the Flemish movement in Belgium and argue for the usefulness of that example for my purpose. Section 4 is devoted to a discussion of the methods with which electoral indicators of the success of the Flemish movement after World War II are analyzed. In that section, I propose a dynamic model of the rise of Flemish solidarity. Empirical estimates of the coefficients of the dynamic model are presented and discussed in Section 5. Finally, Section 6 is devoted to concluding remarks concerning the most important results of the analysis.

2. THEORIES OF ETHNIC SOLIDARITY

a. Reactive Ethnicity

Hechter (1975) proposes explaining the persistence of ethnic solidarity in modern societies within the framework of the reactive ethnicity model. He views resurgences of ethnic solidarity as reactions of the culturally distinct periphery against exploitation by the center. Reactive ethnic solidarity occurs in a modern setting when there is a cultural division of labor, or pattern of structural discrimination. Structural discrimination occurs (Hechter, 1974:1154) "when individuals are assigned to specific types of occupations and other social roles on the basis of observable cultural traits or markers. . . ." With cultural division of labor, ethnic boundaries tend to coincide with lines of structural differentiation, and collective action based on ethnic boundaries (ethnic solidarity) ensues. Structural discrimination (and ethnic solidarity, as a result) can exist regardless of the level of structural differentiation in the society as a whole.

Hechter (1974; 1975) argues furthermore that one of the major mechanisms leading to a cultural division of labor is internal colonialism. Modernization does not affect the population circumscribed by national boundaries evenly. Instead, modernization spreads from one or several centers towards the periphery. The center controls development in the periphery in such a way that the population in the periphery engages in activities which are the most complementary and profitable in terms of the requirements of the center and the least rewarding economically for the periphery. Whenever populations of the center and the periphery can be distinguished by cultural markers, mobilization of solidarity on ethnic lines is likely to occur.

---

[1] See Dahrendorf (1969) for an attempt to synthesize functionalist and Marxist perspectives with respect to collective action. For Dahrendorf, the boundaries of conflict groups coincide with lines of differentiation in a functionally determined authority structure. If one accepts Dahrendorf's assumption, both the functionalist and Marxist arguments may be roughly summarized as follows: Since the criterion of ethnicity is not functionally relevant for the allocation of authority in industrial societies, the likelihood that conflict is based on ethnic boundaries decreases with modernization.

It is necessary to distinguish carefully between the internal colonialism and cultural division of labor arguments in Hechter's (1974; 1975) reactive ethnicity model. The internal colonialism argument is the description of a historical scenario leading to a situation in which ethnic boundaries and occupational cleavages coincide. The cultural division of labor argument provides the mechanism by which this coincidence of ethnic and occupational lines is translated into status group solidarity. As noted by Ragin (1976), the cultural division of labor model and the functionalist model (Lipset and Rokkan, 1967:1–64; Lipset, 1960) are identical: a group is more likely to express solidarity, or become mobilized, when several of the cleavages that distinguish it from the rest of society coincide. Such a reasoning, in the functionalist tradition, is best illustrated by Smelser's (1969:52) discussion of the conditions under which ethnic solidarity emerges in developing nations:

> The important structural feature of such an arrangement is that economic, political and racial-ethnic membership *coincide* with each other. Thus, any kind of conflict is likely to assume racial overtones and arouse the more diffuse loyalties and prejudices of the warring parties. (emphasis his)

Within the functionalist perspective, ethnic solidarity is the result of an accidental coincidence of ethnic boundaries with functional lines of differentiation. The reactive ethnicity model assumes that such coincidence is the result of a much more permanent type of arrangement: functional and ethnic cleavages coincide in situations of internal colonialism in which the center is able to keep the periphery in an economically subordinate position.

Hechter (1975:316) tests the reactive ethnicity model using electoral and census data for British counties. He measures ethnic solidarity as: ". . . the extent to which objective cultural distinctions account for the aggregate voting behavior of groups." More specifically, he utilizes as an indicator of solidarity the coefficient of determination (R^2) of the regression of Conservative voting on cultural residuals. These are the religious and linguistic nonconformity of counties purged of the effects of structural variables, such as population growth, urbanization and the proportions of the labor force in various occupational categories. He shows that the coefficient of determination associated with the cultural residuals remains high for the eight elections from 1885 to 1966 and argues that this evidence supports the predictions of the reactive ethnicity model, as opposed to the functionalist model. However, Hechter's (1975) research is plagued by a series of conceptual and analytical mistakes (Ragin, 1976; 1977).

First, Ragin (1977) has shown that Hechter's results do not hold when vote for Labour, instead of Conservative vote, is used as the basis of the measure of ethnic solidarity. For four elections from 1924 to 1966, Labour voting is practically unrelated to the cultural residuals. These results cast extreme doubts on Hechter's indirect measure of solidarity. This might have been expected given the fact that the measure of solidarity is so remotely connected with a direct measure of the participation of members of an ethnic group in a collective movement explicitly dedicated to the implementation of goals defined on ethnic lines as opposed to other cleavages.

Second, one of the major assumptions of the reactive ethnicity model is that an ethnic group is more likely to be solidary when it is disadvantaged vis-à-vis the rest of society. Such an assumption is necessary to establish a link between a situation of cultural division of labor and reactive ethnicity. Hechter (1975:43), for example, argues: "The greater the economic inequalities between collectivities, the greater the probability that the less advantaged collectivity will be status solidary, and hence, will resist political integration." There is no evidence in Hechter's (1975) book to demonstrate such a connection between relative disadvantage of the members of a group and ethnic solidarity.

Overall, therefore, there is very little empirical evidence for the usefulness of the reactive ethnicity model. However, this model does provide an explanation for resurgences of ethnicity in modern societies.

b. Competition Theories

Writers such as Deutsch (1953), Lieberson (1961), Harris (1964), Van den Berghe (1967), Barth (1969), Bonacich (1972) and Hannan (1979) have proposed theories of ethnic solidarity which involve, explicitly or not, mechanisms of competition between groups for resources. Implications of these theories and of the reactive ethnicity model with respect to resurgences of ethnic solidarity in modern societies are quite opposite. Space limitations do not permit me to present a detailed analysis of all these theories. Therefore, I focus on the version of the competition argument proposed by Barth (1969). (See Nielsen, 1978a, for a more complete discussion.)

Barth (1969) examines the conditions of stability of a polyethnic system. He argues that a system of ethnic relations is stable when the groups occupy distinct niches, or sectors of activity. Such a situation occurs when the groups engage in different types of productive activities. Coresidence is possible and is characterized by low interdependence and limited interactions between groups. Another stable situation is when the groups engage in the same activities but monopolize distinct territories. Conflict is then limited to border politics. Finally, the groups may occupy different and complementary niches. Such a cultural division of labor entails close interdependence and symbiosis, with low intensities of ethnic solidarity and conflict.

Unstable situations of ethnic contact are characterized by the fact that two or more groups compete, at least in part, for the same niche. Then, either one of the groups displaces the other or complementarity is increased by the restriction of group activities to distinct sectors. Situations of competition between groups correspond to high levels of solidarity and conflict. Note that the situations Barth (1969) considers to be stable are also the ones characterized by a cultural division of labor in Hechter's (1975) terminology. Barth (1969) predicts a low level of solidarity in such cases; Hechter (1975) predicts a high level of solidarity. The central mechanism of solidarity and conflict is therefore quite different.[2]

Barth's (1969) competition model suggests that the intensity of solidarity and conflict may increase with modernization. As modernization affects distinct ethnic groups in a traditionally stable system more homogeneously, assignments of individuals to occupations tend to be made increasingly on the basis of rational and achieved criteria that cross-cut ethnic boundaries. Members of different groups compete for the same occupations and value the same rewards. Niche boundaries and ethnic boundaries cease to coincide. The system of ethnic relations becomes unstable and the competitive tensions are manifested by a heightening of solidarity within groups and conflict between groups.[3]

Therefore, both the reactive ethnicity model and competition theories provide explanations of resurgences of ethnic solidarity and antagonism in developed societies. However, they associate such resurgences with very different structural arrangements. For the reactive ethnicity model, solidarity occurs when modernization produces a cultural division of labor in which a peripheral ethnic group is disadvantaged and exploited by the core. The competition theories associate solidarity with situations in which development diffuses more homogeneously over the national territory, the cultural division of labor breaks down and the distributions of occupations, rewards and social roles in the core and the periphery become more similar. I present a preliminary test of these conflicting hypotheses in Section 5 using Belgian data.

---

[2] Hechter (1975:37–8) cites Barth (1969) to support his discussion of the cultural division of labor model. I believe, however, that Hechter misinterprets the main thrust of Barth's argument. Hechter's confusion is rooted in the vagueness of his conceptual definition of ethnic solidarity.

[3] Harris (1964) and Van den Berghe (1967) also propose explanations of ethnic antagonism based on a breakdown of the cultural division of labor. Despite differences in their terminologies, competition mechanisms are crucial in both Harris's comparison of ethnic relations in Brazil and the United States and Van den Berghe's description of the paternalistic and competitive types of race relations.

3. THE CASE OF BELGIUM

In Section 2, I criticized the indirect nature of Hechter's (1975) measure of ethnic solidarity. (See also Ragin, 1977.) It seems that a satisfactory measure of solidarity should incorporate at least the following two concepts:

(1) the formulation of specific goals or claims defined on the membership of an ethnic group as opposed to, or in contradistinction with, other groups (such as social classes) in the social system; and
(2) a measure of the ideological and organizational mobilization of the group membership for the implementation of these specific claims.

The rise of the Flemish movement in Belgium is one historical instance in which such a direct measure of ethnic solidarity is available. The empirical analysis I propose in the next sections utilizes as an indicator of solidarity the electoral support for the Flemish nationalist parties (mostly the Volksunie) in the Flemish region of Belgium after World War II. The vote for the Flemish movement measures ethnic solidarity directly as the success of mobilization based on ethnic boundaries, as opposed to other types of cleavages. The remainder of this section is devoted to a brief historical outline of the rise of the Flemish movement.[4]

Since its independence in 1830, the political life of Belgium had been dominated by the Catholic/Secular cleavage, as in many western European countries. Conflicting views about the respective roles of the Church and the State led to the creation of two main parties: Catholic and Liberal. Industrialization and the implementation of universal male suffrage in 1919 precipitated the success of the "Parti Ouvrier Belge" (Belgian Workers' Party), later to become the Socialist party. The three parties dominated the electoral scene from the end of World War I to the early sixties. The Catholic party was powerful principally in the northern, Flemish-speaking and more rural areas. The Socialist stronghold coincided with the southern, early-industrialized and French-speaking part of the country.

As early as 1919, however, the legislative elections witnessed the first modest success of an explicitly ethnic party: the "Frontpartij." Its objective was to obtain cultural equality for the Flemish language in a Belgian society that was still heavily dominated by the French culture of the national elites in Brussels and of the state bureaucracy. The success of the Flemish movement under various party names grew continuously during the interwar period (with a small setback in 1932) to reach a maximum of 8.27% of the country's total vote in 1939[5] (De Smet et al., 1958). The movement was greatly diminished in the immediate post-World War II period due to some measure of collaboration of its leaders with the German occupants. It did not contest the elections in 1946 and 1950, and obtained only 3.59% of the vote in the Flemish constituencies in 1949 (2.06% of the national vote). With the creation of the "Christelijke Vlaamse Volksunie" (usually shortened to "Volksunie," or VU), the electoral support of Flemish nationalism resumed its steady prewar growth through every legislative election since 1958 to reach 18.8% in 1971 in the Flemish cantons (Hill, 1974).

---

[4] I do not attempt here to provide a detailed analysis of Belgian politics. An illuminating commentary is the one by Lorwin (1966). More recent treatments may be found in Heisler (1974) and Hill (1974). The last paper also contains an extensive bibliography and a review of the few attempts to analyze quantitatively Belgian political behavior, including a reanalysis of the data collected by Delruelle et al. (1970) in a 1968 survey. Finally, Zolberg's (1974) discussion of the origins of the linguistic conflict may be the most perceptive and illuminating statement available on this subject.

[5] When measured as a fraction of the vote in the Flemish constituencies, however, the Nationalist vote in 1939 corresponds to 15.05% of the electorate. The steady growth of the movement between the wars is quite impressive, although many commentators seem to have a tendency to minimize the importance of this phenomenon in view of post-World War II events.

The success of the ethnic-linguistic line of cleavage at a time when the regions of the country were becoming increasingly homogeneous with respect to social and economic structure has puzzled many observers. For example, Hill (1974:46) declares:

> . . . there is a paradox in this success. In former times, when the inequities suffered by the Flemish population were inestimably greater than in the period since the end of the Second World War, the electoral support of the Flemish nationalist parties was smaller. Furthermore, the period since the fulfillment of most of the formal demands of the movement in 1963 has seen major growth of the postwar Flemish nationalist party (VU).

Paradoxical or not, the growth of electoral support for the Flemish movement after World War II constitutes a clear instance of the resurgence of ethnic solidarity in a modern, industrialized society. The rise of Flemish solidarity is depicted in more details in Figure 1.

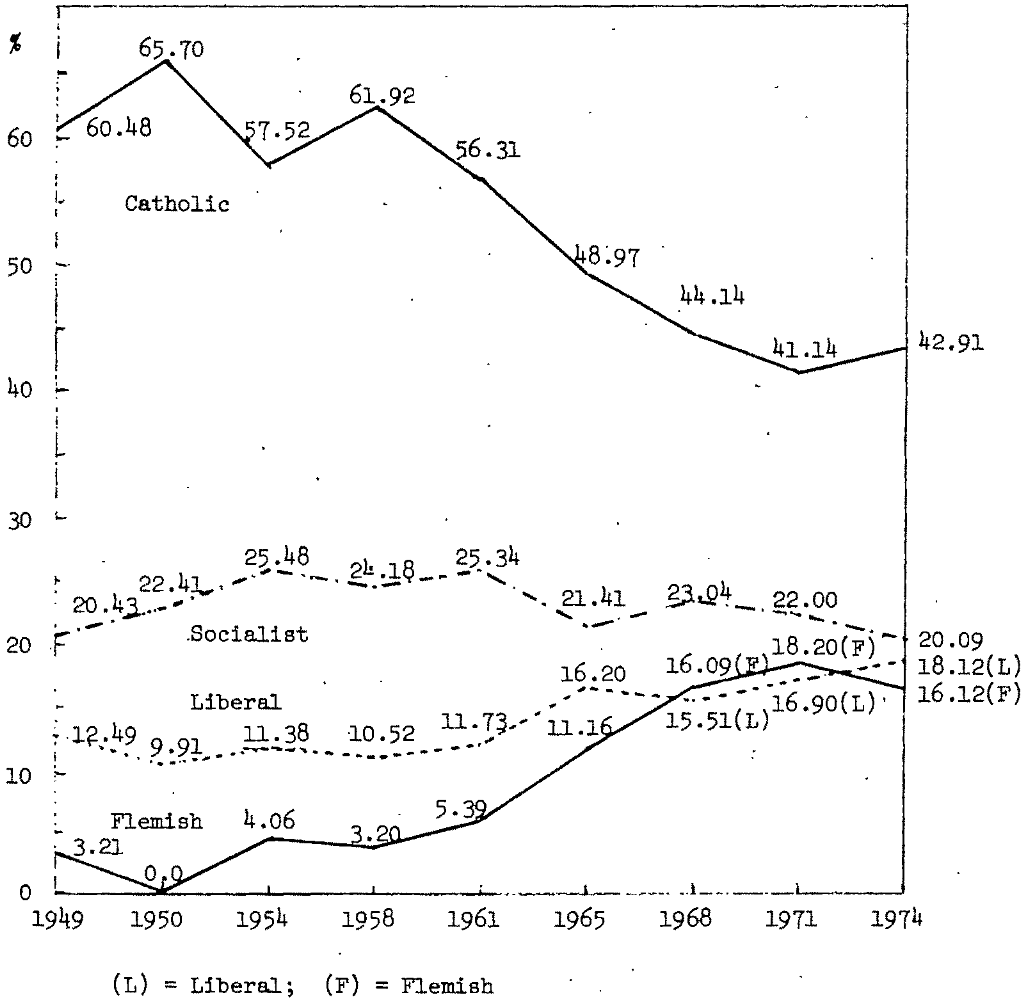


Figure 1. Evolution of support for the four major parties in Flemish cantons: 1949 to 1974. (Unweighted averages of canton percentages. N = 102.)

Figure 1 summarizes the evolution of support for the four major parties in the Flemish region during the nine elections from 1949 to 1974. The percentages of Figure 1 correspond to unweighted averages of support (in percent) for the four parties over 102 cantons (districts) of the Flemish region. The 102 cantons were selected on the basis of ethnic homogeneity (a very large majority of Flemish speakers).[6] I use unweighted averages of support because the canton is the unit of analysis in the model of mobilization estimated in Section 5.[7] Salient features of the figure are the continued dominance, but steady decline, of the Catholic party during the period (from 65.70% in 1950 to 42.91% in 1974) and the corresponding gain in support of the Flemish movement (from 0% in 1950 to 16.12% in 1974). Support for the Socialist party remains relatively constant during the period while Liberal support increases somewhat. The curves of Figure 1 provide a striking depiction of one instance of resurgence of ethnic solidarity in an advanced industrial society.

The curves depicting the evolution of supports for the Catholic party and the Flemish movement are almost mirror images of each other. This symmetry might suggest that the growth in support for the Flemish movement is the result of a simple transfer of votes from the Catholic party. The aggregate transfer might be viewed as produced by individual voters switching their preference or, more realistically over a twenty-year period, as produced in part by the failure of the older generations of voters to reproduce the same intensity of Catholic loyalty among younger cohorts reaching voting age. If the transfer hypothesis is true, the question arises whether Catholic voting is not an alternative avenue for the expression of ethnic feelings, so that the significance of the entire historical episode might be dismissed as the mere relabelling of ethnic solidarity over time. Then, a proper measure of solidarity should combine the votes for both the Catholic and Flemish parties.[8]

This is a serious issue since there is indeed evidence that Catholic voting did represent at one time a sectionalist response (in Hechter's 1975 terminology) for the Flemish electorate. First, most observers of Belgian politics agree that Catholic support included much that was generated by ethnic and regional feelings rather than the purely religious appeal of the party (e.g., Roy, 1968; Hill, 1974). Second, there is the traditional dominance of the Catholics in Flanders as contrasted with the Socialist dominance in the French-speaking parts of the country (except for some low-density rural areas). While this is not in itself a proof of the sectionalist appeal of the Catholic party in Flanders, it strongly suggests a regional-ethnic coloration of Catholic support.[9] Third, there is the evidence, some of which is presented later in this paper and some of which is based on survey data (Delruelle et al., 1970; Hill, 1974), of a high degree of heterogeneity of the social bases of support for the party. Heterogeneity is exactly what one would expect in situations of sectionalism, or the failure of national political integration (Hechter, 1975:218–9), in which political orientations are determined more by status-group membership than by social structural considerations such as class membership. Finally, during the earlier phase of growth of electoral support for the Flemish movement during the interwar period, Flemish success seems to have been concentrated in rural areas which were also the ones in which the Catholic party was most dominant. (For an observer with some knowledge of the social geography of Belgium, the easiest way to see this is to examine the detailed map of the evolution of political alignments at the canton level contained in De Smet et al., 1958.) Again, this historical pattern suggests a high degree of interchangeability of Catholic and Flemish votes as expressions of ethnic feelings.[10]

---

[6] See Nielsen (1978a:chap. 5, Appendix D) for a detailed discussion of the electoral data and criteria of selection of the sample of cantons. Electoral data are from De Smet et al. (1958) and Ministère de l'Intérieur (1965; 1968; 1974).

[7] If the Flemish area of Belgium as a whole, or the individual voter, were units of analysis, weighted averages of support (controlling for the size of the canton) would be the appropriate statistics. Canton averages presented in Figure 1 may also differ from published results on average supports in the Flemish region (e.g., Hill, 1974) because of the stricter selection of units. However, the figures presented by Hill and the ones in Figure 1 are very similar.

[8] I am grateful to an anonymous reviewer for requesting a proper clarification of this important issue.

[9] I do not have the data corresponding to the central and southern areas of the country to be able to carry out a residual analysis of party support similar to the ones performed by Hechter (1975) and Ragin (1977).

However, for the interchangeability and transfer hypotheses to hold, it would have to be the case that, at the canton level, Flemish success is roughly matched by a comparable loss of Catholic support. This is what the symmetry of the curves in Figure 1 suggests. But the curves in Figure 1 are based on electoral data aggregated at the regional level and may conceal a serious ecological fallacy: it is not, on the whole, because the aggregate evolutions of Catholic and Flemish supports are mirror images of each other that the same holds true at the canton level. As a matter of fact, it is not at all the case. In analyses described in detail elsewhere (Nielsen, 1978a and Nielsen, 1978b), I plotted the trends in support for both the Catholic and Flemish party during the 1954 to 1974 period against a measure of the political diversity of a canton in 1950. The trend in party support is computed as the slope of the regression line of support against time for each individual canton and constitutes a summary measure of the success or demotion of a party over the twenty-year period. The trend for the Catholic party was correlated .757 with political diversity, a correlation significant at the .001 level. In order for the transfer hypothesis to hold at the canton level, one would require an equally strong, but negative, correlation between the same exogenous variable, political diversity, and the trend for the Flemish party. The correlation between Flemish trend and diversity is only −.142, with the significance level .083. These results mean that, by and large, *the cantons in which the Catholics lost much support are not the same as the ones in which the Flemish party gained most.*[11] This result largely rules out the simple transfer hypothesis and the contention that Catholic and Flemish voting are alternative expressions of ethnic solidarity after World War II. At the very least, one has to admit that, even if Catholic and Flemish voting both represent expressions of status-group solidarity, they are very different ones indeed. This conclusion, together with the argument for a greater face validity of Flemish support as an indicator of solidarity presented above, justifies the focus on Flemish vote in the rest of this paper.

---

[10] It is quite possible that the history of the Flemish party success between the two wars is consistent with the reactive ethnicity interpretation. It may be that the social-structural correlates of ethnic solidarity are very different if one compares the pre-World War II (1919–1939) and current manifestations of the movement. The quantitative study of the Flemish movement between the wars has rich theoretical possibilities, but is complicated by the fact that the Belgian government was impoverished immediately after World War I and during the Great Depression, so that only the data from the 1920 and 1930 censuses which had immediate legal implications (e.g., for the allocation of seats in legislature) were published. (See the introduction in the first volume of the 1947 census: Institut National de Statistique, 1954.) It appears that more comprehensive data are still available but would have to be coded from the old handwritten records kept by the Institut. The electoral data for the period are readily available at the canton level from De Smet et al. (1958).

[11] Of course, I was as surprised by these results as any observer familiar with the current evolution of political alignments in Belgium is likely to be, since it seems to have been a consensus (sometimes embodied in the complaints by Catholic party leaders) that the Volksunie was draining away a large fraction of electoral support previously allocated to the Catholic party. However, I believe that the argument about the discrepancy between the relationships of Catholic and Flemish trends with the exogenous political diversity variable is rather strong. Its logic is isomorphic with the logic of instrumental variable estimation in statistical methodology. In this case, the transfer hypothesis implies a constraint on the coefficient of the relationship between Catholic and Flemish trends such that the coefficient between the two trends should be approximately equal to −1. Then, any exogenous (instrumental) variable which has a strong causal relationship with one of the trends (a purely mechanical, spurious relationship would do, too) should have an almost perfectly inverse relationship with the other trend. The numbers presented in the text (correlation coefficients) reveal that this is not the case. Note that the same conclusion would have been reached on the basis of different statistics: the slopes of the two plots are not at all the inverse of each other. The conclusion even holds under very qualitative criteria of scientific confirmation. While the plot of Catholic trend is strikingly linear, the plot of Flemish trend against diversity is as strikingly curvilinear. I argue elsewhere (Nielsen, 1978b) that this discrepancy may be due to the effect of the conduciveness of the system of political alignments on mobilization by *any* new party.

In the next section, I propose to analyze variations in electoral support for the Flemish movement and the three traditional parties, both over time and across socially heterogeneous environments, within the framework of a dynamic model of political mobilization. In order to estimate the coefficients of the mobilization model empirically, it is necessary to have indicators of the socioeconomic composition of the cantons. Census data are available for 1961 and 1970. Values of the socioeconomic indicators for each election year at the canton level were obtained by aggregating census figures at the commune level and intra- or extrapolation from the 1961 and 1970 data.[12] I discuss the socioeconomic indicators in more detail in Section 5.

4. A DYNAMIC MODEL OF ETHNIC SOLIDARITY

Given the data available (electoral results for nine elections and extrapolated census figures corresponding to the same time points), one could, in principle, carry out nine cross-sectional analyses of the social bases of Flemish support. However, mobilization takes time. A new party has to advertise its ideology and convince voters to switch their allegiances. The mobilization process is inherently dynamic. Coleman (1968) has shown that cross-sectional analyses of a dynamic process are misleading when the process has not reached equilibrium. An examination of Figure 1 suggests that Flemish support was away from equilibrium during most of the period under investigation. Therefore, my analytical strategy is to model explicitly the dynamics of mobilization for the Flemish movement and for the other three parties.

The simple dynamic model of mobilization assumes that the equilibrium support for a party in a canton depends on the composition of the social environment, as measured by a set of socioeconomic variables to be specified below. Since the exact form of the functional relationship between equilibrium support and the social environment is not known, I assume a linear one, which is the simplest possible:

$$P^* = c_0 + c_1S_1 + c_2S_2 \ldots + c_KS_K \qquad (1)$$

Equation (1) represents the equilibrium support P^* of a party in a canton as a linear function of K socioeconomic characteristics $S_1, \ldots, S_K$ of the canton. The equilibrium support of a party may be thought of as the carrying capacity of the party (a form of collective organization) in a social environment characterized by a particular set of values of the socioeconomic variables S. The carrying capacity corresponds to the maximum level of support achieved by a party in a social environment once the mobilization process is completed.

However, parties do not reach their carrying capacity of support immediately in a social environment, because mobilization is not instantaneous. Consequently, support for a party cannot react instantly to variations in the carrying capacity of support caused by changes in the composition of the social environment. Moreover, parties are likely to differ with respect to the speed with which they can recruit their potential memberships. To capture these dynamic aspects, I represent the growth in support for a party, or rate of mobilization, as a function of the discrepancy between current support and the carrying capacity:

$$dP/dt = -b\,(P^* - P) \qquad (2)$$

In Equation (2), P denotes the current level of support and P^* is the carrying capacity defined in Equation (1). dP/dt, the derivative of P with respect to time, represents the rate of mobilization. Equation (2) describes the way in which support for a party is assumed to grow or decline. The growth in support is proportional to the discrepancy between carrying capacity and current participation. The model assumes that b is negative. Then, −b is positive and support for the party increases (dP/dt is positive) when current support is smaller than the carrying capacity in the social environment. When current support P is larger than P*, support declines. In both cases, P reaches its equilibrium value P* as time elapses. The absolute value of the parameter b determines the speed at which the process of mobilization (or demobilization) takes place. A large absolute value of b means that the support adjusts quickly to its carrying capacity in the social environment; a small absolute value of b indicates that the mobilization process is slow. In the next section, I propose substantive interpretations of the coefficient b in terms of differences in the nature of the mobilization process across parties. Substituting (1) into (2) and rearranging the terms yields the full model:

---

[12] Census data in machine-readable form at the commune level for 1961 and 1970 were obtained through the Belgian Archives for the Social Sciences (B.A.S.S., 1976) and Dr. Mark Van Naelten of the Katholieke Universiteit te Leuven.

$$dP/dt = bP - bc_0 - bc_1S_1 - bc_2S_2 - \dots - bc_KS_K \qquad (3)$$

The differential Equation (3) represents the growth of support as a function of both current support P and the current values of K socioeconomic characteristics S of a canton. The solution of (3) is (Coleman, 1968):

$$P_t = P_0e^{bt} + (1-e^{bt})c_0 + (1-e^{bt})c_1S_1 + \dots + (1-e^{bt})c_KS_K \qquad (4)$$

Equation (4) provides the leverage needed for estimation of the coefficients of the dynamic model (assuming that the time interval between P_0 and P_t is equal to 1) since it is equivalent to:

$$P_t = b'P_{t-1} + c_0' + c_1'S_{1,t-1} + \dots + c_K'S_{K,t-1} + W_t \qquad (5)$$

where $b' = e^b, c_0' = (1-e^b)c_0', \dots, c'_K = (1-e^b)c_K$. In Equation (5), support at time t is a linear function of its value at the preceding election and the lagged values of the socioeconomic variables $S_1, S_2, \dots, S_K$. The coefficients of Equation (5), in which W_t denotes a random disturbance, can be estimated by regression techniques. The coefficients of the dynamic model (3) can be recovered quite simply from estimates of the coefficients of (5): $\hat{b} = \ln\hat{b}'$ and $\hat{c} = \hat{c}'/(1-\hat{b}')$. A similar procedure is used by Nielsen and Hannan (1977) in the context of a discrete time dynamic model (see also Doreian and Hummon, 1976).[13]

Equation (5) has the familiar form of a linear equation with lagged dependent variable. Given the data available, (5) could be estimated using electoral results, and the corresponding values of the socioeconomic variables, for any one of the eight pairs of election years. Rather than estimate (5) separately in eight different ways, I pooled all the data into a single model. This technique of pooling time-series and cross-section data has been discussed extensively by Hannan and Young (1977). It permits us to deal satisfactorily with an estimation problem that plagues almost all analyses of panel data in sociology: autocorrelation of disturbances. Disturbances are correlated when stable characteristics of a canton, such as the distance from the capital, have been omitted from the formula (1) for the carrying capacity of support for a party. These unmeasured features of the social environment are forced into the disturbance term W_t in (5). Since they presumably operate at every point in time, disturbances are correlated over time and with the lagged dependent variable. Consequently, ordinary least-squares estimators applied to (5) are biased and inconsistent. The possibility of bias in estimates of the coefficient b′ of the lagged dependent variable is particularly troublesome since b′ enters into the computation of all the other coefficients of the model (the c coefficients).

---

[13] Coleman (1968) proposes more complicated manipulations of the regression coefficients c′ in (5) in order to obtain estimates of the parameters bc of the differential Equation (3) in canonical form. However, Rosenfeld and Nielsen (1978) argue that the substantive interpretation of the parameters of a differential equation in canonical form is awkward. For my purpose, the coefficients to be interpreted substantively are the dynamic coefficient b and the coefficients c of equilibrium support. These can be recovered from the estimated regression coefficients of Equation (5) with computational formulas which involve only one subtraction and a few divisions and are simpler than the ones proposed by Coleman (1968).

Autocorrelation of disturbances is not the only estimation problem in this case. My preliminary analyses indicated that the absolute values of the residuals increase sharply with the size of the canton, as measured by the total number of valid votes. This finding suggests that the distribution of disturbances is heteroscedastic (the variance of disturbances is not constant). Heteroscedasticity implies that ordinary least-square estimators of (5) are inefficient. To control for both the auto correlation of disturbances and heteroscedasticity, I used a generalized least-squares estimator labelled WGLS ("weighted generalized least-squares"). The derivation of the WGLS procedure is quite technical. A detailed discussion may be found in Nielsen (1978a:chap. 6, Appendices A and B).[14]

In the next section, I present WGLS estimates of the coefficients of the dynamic model of mobilization for each one of the four major parties: the Catholic, Socialist, Liberal and Flemish parties. To control for the homogeneity of the process, I use only data pertaining to the seven election years from 1954 to 1975. This strategy is justified by the fact that the evolution of Flemish support in 1949 and 1950 seems atypical with respect to the dynamic model of mobilization (see Figure 1).

5. RESULTS

To analyze empirically the social bases of support for the Flemish movement, it is necessary to specify more precisely the socioeconomic variables S_K which determine the carrying capacity of support for the party (see Equation [1]). Among those available from BASS and Mark Van Naelten (see footnote 12), I selected a set of six variables chosen in such a way as to constitute as detailed a picture as possible of the phenomenon of modernization. The substantive aspects of these variables will be discussed in more detail with respect to various theories of ethnic solidarity in the presentation of the results below.

S_1 : number of housing units in one-family houses. This is a negative measure of urbanization—a high value of this variable indicates a low level of urbanization. (For a discussion of this indicator in the Belgian context see Jouret, 1972.) More traditional measures of urbanization, such as the proportion of individuals living in cities with populations greater than a given number, may be more familiar to sociologists but could not be used here since a single city may contain several units of analysis (cantons). Note that the variable is, quite literally, a negative measure of the extent to which housing units are stacked up on top of each other. Therefore, S_1 may be high both in wealthy suburbs and in rural environments.[15]

S_2 : male labor force in agriculture and fishing, an indicator of occupational concentration in traditional primary activities.

S_3 : floor area (in 100 m^2) used for industry, an indicator of industrialization.

S_4 : male and female labor force employed in commercial activities, a measure of employment in the tertiary sector.

S_5 : active population working outside the commune of residence. This is a measure of mobilization of the peripheral population in Deutsch's (1953) sense. S_5 captures the volume of movements of the population across the boundaries of the local setting and the density of interactions with individuals outside. While the mobilization indicator has no direct geographical meaning at the canton level (being computed by aggregating figures at the commune level) and measures, therefore, both the density of interactions within the periphery and between the periphery and the center, I included it as a control variable because the reactive ethnicity and competition theories both assume it is an important factor. Hechter (1975:43) argues explicitly that the density of within-community contacts should facilitate ethnic solidarity. Competition theories generally suggest that center-periphery contacts (and the increased likelihood of competition) facilitates mobilization on the basis of ethnicity.

---

[14] The WGLS procedure involves estimating various components of the variance of the error term in (5). The formulas I used are based on the work of Henderson, Sr. (1952; 1963), Henderson, Jr. (1971) and Searle (1971). The model also involves assumptions on the behavior over time of the exogenous variables S. Because the values of the exogenous variables were obtained by extrapolating 1961 and 1970 figures, it was impossible to compute accurate estimates of the change in the S variables between any two election years. Therefore, I chose to assume that the exogenous variables are relatively constant between elections. See Coleman (1968) for a discussion of the behavior of exogenous variables in a dynamic model.

[15] This problem of interpretation leads to the suspicion that there may be mechanisms underlying the measurement of social reality which, like the uncertainty principle in physics, imply that an increase in precision (in this case, a finer-grained measure of urbanization) has to be compensated somehow by a loss elsewhere (here, straightforward interpretability).

S_6 : total income (in millions Belgian francs).

The set of six indicators seems to provide a clear picture of the most important aspects of the process of modernization: increased urbanization is captured by S_1, the progressive shift in the occupational distribution from the primary to the tertiary sectors by variables S_2,S_3 and S_4. The mobilization indicator S_5 monitors the degree of insertion of the local community into the larger, national network of interactions. Finally, S_6 is a straightforward measure of the relative share of economic rewards a local community has been allocated as a result of the entire process. With that selection of independent variables, Equation (5) is suitably specified for empirical estimation. I estimated the coefficients of the model (5) with P_t measured as the number of votes for the Flemish movement at election t. For comparison purposes, and as a partial check of the validity of the mobilization model, I also estimated the coefficients for the Socialist, Liberal and Catholic parties. The weighted generalized least-squares estimates for the four parties are presented in Table 1.

a. The carrying capacity of support for the Flemish movement. It would be possible to assess the effects of the various characteristics of the social environment on Flemish support directly from an

Table 1. Regression of Party Support on Support at Previous Election and Socio-Economic Indicators. Weighted Generalized Least-Squares (WGLS) Estimates[a]

	Dependent Variables			
Independent Variables	Socialist Support	Liberal Support	Catholic Support	Flemish Support
Lagged party support	.834*	.856*	.673*	.608*
	(.020)	(.024)	(.030)	(.035)
S_1, housing units in one-family houses	.070*	.030	.130*	.011
	(.017)	(.017)	(.035)	(.017)
S_2, male labor force in agriculture and fishing	−.255*	.050	.677*	.058
	(.076)	(.078)	(.178)	(.079)
S_3, industrial floor-area ($100m^2$)	.034*	−.006	.085*	−.035*
	(.017)	(.017)	(.036)	(.017)
S_4, male and female labor force in commerce	.338*	−.083	.775*	.370*
	(.121)	(.109)	(.228)	(.113)
S_5, actives working outside commune of residence	.044*	.017	.176*	.046*
	(.018)	(.019)	(.042)	(.019)
S_6, income (millions BF)	−.616*	.343*	−1.580*	.544*
	(.138)	(.126)	(.277)	(.156)
Constant	43.88	55.57	63.06	−27.98
	(84.52)	(92.84)	(227.57)	(90.71)
R^2	.977	.941	.964	.921

No. of cases: 96.
No. of time periods: 6.
Total No. of cases in the pooled regressions: 576.
Weighting variable: No. of valid votes at time t.
* Significant at the .05 level.

[a] Electoral returns for 1958, 1961, 1965, 1968, 1971 and 1974 are used as dependent variables. Lagged party supports are measured from 1954 to 1971. Values of other independent variables are obtained by intra- and extrapolation of 1961 and 1970 census figures. These are estimates of the coefficients of Equation (5):

$$P_t = b'P_{t-1} + c_0' + c_1 'S_{1, t-1} + c_2 'S_{2, t-1} + c_3 'S_{3, t-1} + c_4 'S_{4, t-1} + c_5 'S_{5, t-1} + c_6 'S_{6, t-1} + W_t$$

examination of the regression coefficients in Table 1. However, it will be substantively illuminating to compare the effects of the socioeconomic variables across parties. In order to compare the coefficients of a dynamic model across equations, it is necessary to compute the coefficients c of the carrying capacities of support (Equation [1]). These can be recovered quite simply from the regression coefficients in Table 1 (see Section 4). The transformation amounts to a control for differences in the dynamics of mobilization across parties. Estimates of the coefficients c are presented in Table 2. However, coefficients corresponding to different variables are not standardized, and have different metrics (e.g., employment in agriculture and industrial floor area). Therefore it is useful, for comparing coefficients associated with different variables, to refer to the standard errors of estimate of the regression coefficients reported in Table 1.

I discuss the coefficients for the carrying capacity of support of the Flemish movement first. The coefficient of S_1 (housing units in one-family houses) is small (.028) and the corresponding regression coefficient is insignificant. Therefore, urbanization has no effect on ethnic solidarity, controlling for the other variables in the equation. (See the discussion of the indicator of mobilization above.) The coefficient of S_2 (employment in agriculture and fishing) is insignificant, too. This result is important for a comparison of the relative merits of the reactive ethnicity and competition theories of ethnic solidarity. Agriculture is the traditional specialty of Flanders within the Belgian cultural division of labor (Roy, 1968; Hill, 1974). According to the reactive ethnicity model, one would expect social environments which are most affected by differential development of the national economy and kept in the least rewarding and most traditional sectors of activity to be the most favorable to ethnic solidarity. This is not the case: employment in agriculture has no significant effect on the carrying capacity of support for the Flemish movement. This result may correspond to a crucial difference between the nature of ethnic solidarity in Flanders before and after World War II. (See footnote 10.)

The coefficient associated with S_3, the indicator of industrialization, is negative (−.089) and significant. Industrialization affects adversely the success of ethnic solidarity. This finding must be interpreted in light of the fact that the working class in Belgium has traditionally been strongly organized through the network of grass-roots organizations, such as trade unions, of both the Socialist party and the liberal wing of the Catholic party. Intense competition from both entrenched groups, with their specialized institutions, makes penetration difficult for a party with less specific appeal. Such a negative effect of industrialization on Flemish support might have been expected, since the formula for the carrying capacity of support in the dynamic model of mobilization incorpo-

Table 2. Estimated Coefficients of the Differential Equations Model of Mobilization[a]

	Dependent Variable			
Independent Variables	Socialist Support	Liberal Support	Catholic Support	Flemish Support
Dynamic parameter (b)	−.182*	−.156*	−.396*	−.498*
S_1 (urbanization)	.422*	.208	.398*	.028
S_2 (agriculture)	−1.536*	.347	2.070*	.148
S_3 (industry	.205*	−.042	.260*	−.089*
S_4 (commerce)	2.036*	−.576	2.370*	.944*
S_5 (mobilization)	.265*	.118	.538*	.117*
S_6 (income)	−3.711*	2.382*	−4.832*	1.388*
Constant	264.34	385.90	192.84	−71.38

[a] See Table 1 for definition of variables. These are the coefficients c of the carrying capacity of support (Equation (1)) and the dynamic coefficient b of Equations (2), (3) and (4) estimated from the regression coefficients in Table 1. The computational formulas are discussed in Section 4.

* The corresponding regression coefficient in Table 1 is significant at the .05 level.

rates the effects of competition for support by other parties. The presence of specialized and powerful solidary groups defined on class lines makes for continued salience of the class cleavage in industrial environments.

S_4 (commercial activities) is a measure of concentration of the working population in the tertiary sector. The predictions of the reactive ethnicity and competition theories with respect to the effect of this variable on ethnic solidarity differ considerably. The reactive ethnicity model suggests that, during the process of development, the core attempts to monopolize precisely those activities associated with the tertiary sector which are the most rewarding economically (Hechter, 1974; 1975). Ethnic solidarity is assumed to emerge in areas in the periphery which are denied the full benefit of modernization. Therefore, the reactive ethnicity model would predict a negative association between ethnic solidarity and employment in the tertiary sector. The opposite is true. The coefficient of S_4 is positive (.944) and significant (the corresponding regression coefficient is more than three times the standard error). Employment in the tertiary sector has a positive effect on ethnic solidarity. This finding is consistent with competition theories of solidarity (see Section 2). Competition theories assume that ethnic solidarity is high where the cultural division of labor breaks down. Employment in the tertiary sector measures involvement of the peripheral population in activities previously monopolized by members of the center and also measures a high intensity of competition. Therefore, ethnic solidarity should be positively associated with S_4.

The coefficient of S_6 (income) confirms this pattern. The coefficient of income is positive (1.388) and significant. Controlling for the distribution of the active population within the division of labor, we find that social environments which receive a larger share of the economic rewards of the modern system of production are also those in which ethnic solidarity is most successful. This amounts to a further disconfirmation of the reactive ethnicity model. Structural discrimination, as measured by economic disadvantage, inhibits rather than enhances ethnic solidarity. Such a result does not make sense within the exploitation logic of the reactive ethnicity model.

Finally, the indicator of mobilization (S_5) has a positive effect (.117) on ethnic solidarity. Such a finding is consistent with just about any theory of political mobilization on the basis of inclusive ethnic boundaries. The measure of mobilization captures the intensity of both intra- and intercommunity contact. Therefore, the positive effect is consistent with both the reactive ethnicity and competition theories of ethnic solidarity.

Clearly, the reactive ethnicity model of solidarity proposed by Hechter (1974; 1975) is inconsistent with these empirical results. Employment in agriculture and fishing, the traditional specialty of Flanders within the cultural division of labor, is unrelated to ethnic solidarity. Solidarity is enhanced, by contrast, by indicators of modernization such as employment in the tertiary sector and a large share of the rewards associated with the modern production system. The very same findings lend support to the competition theories of solidarity summarized in Section 2. An increased share of resources and a high proportion of the labor force in the most modern sector of activity facilitate mobilization on the basis of ethnic, as opposed to universalistic, boundaries. However, the negative effect of industrialization indicates that ethnicity is less successful in competition with universalistic (occupation related) bases of collective action in working-class environments.

As a partial check of the validity of the dynamic model of mobilization, I also computed the coefficients c of the carrying capacity of support for the Socialist, Liberal and Catholic parties (Table 2). If the results with respect to the social bases of Flemish support are to be trusted, results for the three other parties should be largely consistent with what is known about the traditional bases of support for these parties (see, for example, Hill, 1974).

When comparing the four columns of Table 2, the first, global impression is that coefficients of Catholic support corresponding to the three indicators of the occupational distribution (agriculture, industry and commerce) are larger than the coefficients of Flemish support (2.070, .260 and 2.370 for Catholics; .148, −.089 and .944 for the Flemish movement). This pattern is understandable in view of the fact (Figure 1) that Catholic support seems to stabilize at a higher level than the Flemish movement. Coefficients that can be interpreted as measures of the share of support for a party in a social environment partitioned into various disjoint categories are expected to be larger when average support for the party is larger.

The large effect of employment in agriculture (2.070) on Catholic support is consistent with the traditional predominance of the party in rural areas. The negative effect (−1.536) of this variable on Socialist support also confirms the historical weakness of the Socialists in these areas. The carrying capacities of both the Socialist and Catholic parties are high in industrial environments (.205 and .260, respectively, for industrial floor area).[16] This finding corresponds to the fact that the two parties compete for the participation of the working class in their grassroots organizations. The coefficients associated with employment in commercial activities, an indicator of the tertiary sector, reveal a high carrying capacity of support for both the Socialist and Catholic parties.

The coefficients associated with income are large and negative for both the Socialists and the Catholics while they are positive for the Liberal party and the Flemish movement. For the Liberal party, the coefficient of income is the only significant one in the equation—this fact suggests an elitist and undifferentiated basis of support. The positive effects of income on both Liberal and Flemish support is interesting. This finding suggests that the Liberal party, because of its longstanding position in favor of Belgian unity, and the Flemish movement represent the two conflicting sides of the subnational cleavage. (The two parties have been the most successful in Flanders after World War II. See Figure 1.) As the effect of structural discrimination decreases, or is reversed, the social environment becomes less favorable to the development of class-based mobilization in the periphery and more sensitive to issues related to subnationalism.

Overall, results with respect to the social bases of support of the Socialist, Liberal and Catholic parties largely support the traditional wisdom of Belgian politics. The Catholic party is dominant in rural environments, but also fares well in working-class areas and those with high employment in the tertiary sector. The Socialist party is most successful in the secondary and tertiary sectors. Liberal support is associated with wealth. Such results, insofar as they were expected on the basis of an independent political lore, enhance confidence in the dynamic model of mobilization and the results concerning the social bases of Flemish support.

b. Dynamic aspects of solidarity. The dynamic model of mobilization represented by Equations (3) and (4) (see Section 4) allows explicitly for differences in the speed of mobilization of support for various solidary groups. Speed of mobilization is captured by the magnitude of the absolute value of the coefficient b in Equations (2), (3) and (4). A large $|b|$ indicates that support for the solidary group adjusts quickly to its carrying capacity determined by the composition of the social environment. Substantive interpretations of the magnitude of $|b|$ differ when support for the group is growing or declining on the average. Estimates of the coefficient b for the four major parties in Flanders are presented in the first row of Table 2. All four estimated coefficients are negative, so that mobilization of support should lead to a stable equilibrium.[17]

---

[16] The absolute values of the coefficients associated with the measure of industrialization are typically lower than the coefficients associated with measures of employment in the primary (agriculture) and tertiary (commerce) sectors. This is due to the fact that the metrics of the measure of industrialization (100 square meters) and measures of employment in other sectors (number of individuals) differ.

[17] This finding constitutes a partial check of the validity of the model. A positive value of the coefficient b would imply a meaningless behavior of the political system. When $b > 0$, the model implies that support for the party will increase, or decrease, exponentially depending on the initial value of support above or below the equilibrium value determined by

Both the Socialist and Liberal parties have low absolute values of the coefficient b (.182 and .156, respectively). The small value of |b| for the Socialists, in view of the general decline of support for the party over the period, indicates an enduring strength of party identification. Identification of the Socialist electorate may be strong for several reasons. First, there is the commitment due to the participation in grass-roots organizations of the party such as trade unions, health care plans and credit unions. Second, the Socialist party is associated with anticlericalism in Flanders. Insofar as the psychological cost for the individual of switching from a religious identification has been high, the identification of Socialist voters within a largely Catholic population is stronger. This is a minority type of process (Durkheim, 1966:152–70). Third, evidence presented elsewhere (Nielsen, 1978a: chap. 5) suggests that Socialist support is heterogeneously distributed over the Flemish region and concentrated in working-class neighborhoods. Concentration in specific areas may generate stronger collective pressures against defection. Such contextual effects are often interpreted in terms of the intensity of class consciousness (Boudon, 1967:183–222; Segal and Meyer, 1969; Przeworski, 1974). The second and third arguments above apply also to the Liberal party. However, the network of grass-roots organizations of the party is less comprehensive and powerful than those of the Socialists and Catholics. Inertia in mobilization for Liberals may also result from the faithful support of the French-speaking minority in Flanders, given the unitarianist position of the party (Roy, 1968:162–3).

The two parties that have grown most (the Flemish movement) and declined most (the Catholic party) have the largest absolute values of the coefficient b (.498 and .396, respectively). In general, a large |b| means quick mobilization or demobilization for a solidary group. For a group with growing support, such as the Flemish movement, a large |b| indicates that the group can take advantage of a large pool of prospective members who are only marginally attached to older movements and constitute, therefore, potential defectors. Such a pattern might have been expected for the Flemish movement in view of the historical circumstances in which mobilization on the basis of ethnic lines took place after World War II. Parties based explicitly on the defense of Flemish interests had already succeeded in securing a sizeable support in the Flemish area before the war. Because of the disorganization of the movement after the war, the niche occupied by ethnic solidarity in the political environment was vacated, and was filled by other solidary groups. After the reorganization of the movement, much of the mobilization effort may have been remobilization of a membership poorly identified with second-best competitors. Another factor of swift mobilization may be the lack of investment in a *verzuil* (pillar) of grass-roots organizations competing with those of the major parties for intensive commitment of the population (Hill, 1974). Such a strategy allows for a low cost of participation and facilitates opportunistic recruitment.[18]

The high rate of demobilization for the Catholic party may involve more subtle analytical considerations. The results of Table 2 show that the Catholic party succeeds equally well in environments characterized by high employment in the primary, secondary and tertiary sectors. The Socialist party fares well in industrial and commercial environments but very badly in rural ones. The Flemish movement is especially successful in social environments characterized by a high level of employment in the tertiary sector. The social bases of support for the three parties may be rank-ordered according to their degree of heterogeneity. The Catholic party has a very heterogeneous basis of support, the Flemish movement a very specialized one. The Socialist party is somewhere in

---

exogenous variables. $b < 0$ implies a monotonic convergence of support to the equilibrium value.

[18] I am indebted to Jerome Black of the Department of Political Sciences, McGill University, for suggesting this interpretation of the speed of Flemish success, in a personal communication. His impression is based on records of numerous private conversations with leaders of the Volksunie.

between. (See also Delruelle et al., 1970, for additional evidence based on survey data.) The distinction between heterogeneous and specialized bases of support corresponds to a crucial distinction between generalist and specialist strategies in the population ecology of organizations (Hannan and Freeman, 1977; Nielsen, 1978c).

It may be that the success of a solidary group in keeping its level of participation when a new competitor is introduced in the system varies according to its strategy. Because of the heterogeneity of its basis of support, a generalist is likely to have a fringe membership poorly identified with some of the goals of the group. The new competitor may be able to attract the adherence of this fringe membership quickly. Weaker commitment of the membership of a generalist party would be translated in a dynamic context into a faster decline in competition with a solidary group mobilizing on the basis of an alternative cleavage. The large absolute value of b (.396) for the generalist Catholic party relative to the value of |b| (.182) for the specialized Socialist party is consistent with that hypothesis. However, the evidence presented in Section 3 largely rules out the simple hypothesis that the Flemish movement obtained most of its votes from defections from the Catholic party. The question which then arises is: "Where have all the Catholic votes gone?" to be sung to the well-known tune. Since a large part of these votes has *not* gone to the Volksunie, the only hypothesis left to entertain is a most intriguing one: most of the votes lost by the Catholic party must have been captured by the Socialists and Liberals. In the cantons in which this occurred, it represents a decrease in sectionalism for the Flemish electorate. On the other hand, the vote for the Volksunie must have come from somewhere: if not the Catholic party, then the Socialist and Liberal ones. This suggests that the evolution of political alignments in Flanders after World War II may have been quite complex. In some cantons, Catholic dominance was replaced by a more equal distribution of support between two parties defined on universalistic lines (the Socialists and Liberals), which corresponds to a decrease in sectionalism with modernization. In cantons which were already politically integrated (with low Catholic dominance), voters switched more enthusiastically to the modern form of ethnic solidarity represented by the Volksunie.

6. DISCUSSION AND CONCLUSION

Resurgences of subnational movements and ethnic conflict in modern states are difficult to explain within the classical theoretical frameworks of sociology. In this paper, I contrasted two categories of explanations of such resurgences which involve radically different assumptions. Hechter's (1975) reactive ethnicity model uses a logic of exploitation: ethnic solidarity is viewed as a form of rebellion of a disadvantaged group against the dominant ethnicity. The greater the inequality, the more intense the solidarity within the subordinate group. Solidarity is akin to class consciousness. Wallerstein (1975:24) summarizes the basic argument of the reactive solidarity model as follows: ". . . one can see the recrudescence of ethnonationalisms in industrialized states as an expression of class consciousness of lower caste-class groups in societies where the class terminology has been preempted by nation-wide middle strata organized around the dominant ethnic group."

Competition theories of solidarity suggest, to the contrary, that solidarity emerges as a result of competition between groups for the same occupations, statuses and economic rewards. Resurgences of ethnic solidarity should occur precisely where the pattern of structural discrimination breaks down and status inequalities between groups are diminished. My analysis of the rise of the Flemish movement in Belgium after World War II disconfirms the reactive ethnicity model and lends support to competition theories of ethnic solidarity. Solidarity is more intense in areas which are less discriminated against occupationally and economically.

REFERENCES

Barth, Fredrik (ed.)
1969 Ethnic Groups and Boundaries. Boston, Mass.: Little, Brown.

B.A.S.S. (Belgian Archives for the Social Sciences)
1976 Inventaire des archives disponibles. Louvain-la-Neuve, Belgium.

Berger, Brigitte
1971 Societies in Change. New York: Basic Books.

Bonacich, Edna
1972 "A theory of ethnic antagonism: the split labor market." American Sociological Review 37:547–59.

Boudon, Raymond
1967 L'analyse mathematique des faits sociaux. Paris: Plon.

Coleman, James S.
1968 "The mathematical study of change." Pp. 428–78 in H. M. Blalock and A. Blalock (eds.), Methodology for Social Research. New York: McGraw-Hill.

Dahrendorf, Ralf
1969 Class and Class Conflict in Industrial Society. Stanford: Stanford University Press.

Delruelle, Nicole, René Evalenko and William Fraeys
1970 Le comportement politique des électeurs belges. Brussels: Editions de l'Institut de Sociologie, Université Libre de Bruxelles.

De Smet, R. E., René Evalenko and William Fraeys
1958 Atlas des élections belges 1919–1954 (+Annexe statistique + Suppléments, 1958–1961). Brussels: Institut de Sociologie Solvay.

Deutsch, Karl W.
1953 Nationalism and Social Communication. Cambridge, Mass.: MIT Press.

Doreian, Patrick and Norman P. Hummon
1976 Modeling Social Processes. New York: Elsevier.

Durkheim, Emile
[1951] Suicide. New York: Free Press.
1966

Hannan, Michael T.
1979 "The dynamics of ethnic boundaries in modern states." Pp. 253–75 in Michael T. Hannan and John Meyer (eds.), National Development and the World System: Educational, Economic and Political Change, 1950–1970. Chicago: University of Chicago Press.

Hannan, Michael T. and John Freeman
1977 "The population ecology of organizations." American Journal of Sociology 82: 926–66.

Hannan, Michael T. and Alice A. Young
1977 "Estimation in panel models: results on pooling cross-sections and time-series." Pp. 52–83 in D. Heise (ed.), Sociological Methodology. San Francisco, Calif.: Jossey-Bass.

Harris, Marvin
1964 Patterns of Race in the Americas. New York: Walker.

Hechter, Michael
1974 "The political economy of ethnic change." American Journal of Sociology 79:1151–78.
1975 Internal Colonialism: The Celtic Fringe in British National Development. Berkeley: University of California Press.

Heisler, Martin O.
1974 "Institutionalizing societal cleavages in a cooptive polity: the growing importance of the output side in Belgium." Pp. 178–220 in Martin O. Heisler (ed.), Politics in Europe. New York: McKay.

Henderson, C. R., Sr.
1952 "Specific and general combining ability." Pp. 352–70 in John W. Gowen (ed.), Heterosis. Ames: Iowa State College Press.
1963 "Selection index and expected genetic advance." Pp. 141–63 in Statistical Genetics and Plant Breeding. Publication No. 982. Washington: National Academy of Sciences.

Henderson, Charles R., Jr.
1971 "Comment on 'The use of error components models in combining cross-sections with time series data.'" Econometrica 39:396–401.

Hill, Keith
1974 "Belgium: political change in a segmented society." Pp. 29–107 in Richard Rose (ed.), Electoral Behaviour: A Comparative Handbook. New York: Free Press.

Institut National de Statistique
1954 Recensement général de la population au 31 décembre 1947. Vol. 1. Brussels: Institut National de Statistique.

Jouret, Bernard
1972 Définition Spatiale du phénomène urbain bruxellois. Brussels: Editions de l'Université Libre de Bruxelles.

Lieberson, Stanley
1961 "A societal theory of race and ethnic relations:" American Sociological Review 26:902–10.

Lipset, Seymour M.
1960 Political Man. New York: Doubleday.

Lipset, Seymour M. and Stein Rokkan
1967 Party Systems and Voter Alignments. New York: Free Press.

Lorwin, Val R.
1966 "Belgium: religion, class and language in national politics." Pp. 147–87 in Robert A. Dahl (ed.), Political Oppositions in Western Democracies. New Haven: Yale University Press.

Marx, Karl
1964 Precapitalist Economic Formations. E. J. Hobsbawn (ed.). Trans. Jack Cohen. New York: International Publishers.

Ministère de l'Intérieur
1965 Elections législatives. Chambre des représentants. Résultats officiels (officieux) des élections du 26 mars 1961 et du 23 mai 1965. Brussels: Ministère de l'Intérieur.
1968 Elections législatives. Chambre des représentants. Résultats officiels des élections du

31 mars 1968 et du 7 novembre 1971. Brussels: Ministère de l'Intérieur.
1974 Elections législatives. Chambre des représentants. Résultats officiels du 7 novembre 1971 et du 10 mars 1974. Brussels: Ministère de l'Intérieur.

Nielsen, François
1978a "Linguistic conflict in Belgium: an ecological approach." Ph.D. dissertation, Department of Sociology, Stanford University.
1978b "Structural conduciveness and ethnic mobilization: the Flemish movement in Belgium." Paper presented at the annual meeting of the American Sociological Association, San Francisco, California.
1978c "A population ecology model of collective action." Paper presented at the 1978 meeting of the Southern Sociological Society, New Orleans.

Nielsen, François and Michael T. Hannan
1977 "The expansion of national educational systems: tests of a population ecology model." American Sociological Review 42:479–90.

Park, Robert E.
1950 Race and Culture. Glencoe: Free Press.

Przeworski, Adam
1974 "Contextual models of political behavior." Political Methodology 1 (Winter):27–61.

Ragin, Charles
1976 "Review of Michael Hechter's 'Internal colonialism: the Celtic fringe in British national development.' " Social Forces 55:553–4.
1977 "Class, status, and 'reactive ethnic cleavages': the social bases of political regionalism." American Sociological Review 42:438–50.

Rosenfeld, Rachel A. and François Nielsen
1978 "Differential equation models of structural change: the problem of substantive interpretation." Paper presented at the 9th World Congress of Sociology, Uppsala.

Roy, Albert du
1968 La guerre des Belges. Paris: Seuil.

Searle, S. R.
1971 "Topics in variance component estimation." Biometrics 27:1–76.

Segal, David R. and Marshall W. Meyer
1969 "The social context of political partisanship." Pp. 217–32 in Mattei Dogan and Stein Rokkan (eds.), Social Ecology. Cambridge, Mass.: MIT Press.

Smelser, Neil J.
1969 "Mechanisms of change and adjustment to change." Pp. 33–54 in William A. Faunce and William H. Form (eds.), Comparative Perspectives on Industrial Society. Boston: Little, Brown.

Van den Berghe, Pierre L.
1967 Race and Racism: A Comparative Perspective. New York: Wiley.

Wallerstein, Immanuel (ed.)
1975 World Inequality: Origins and Perspectives on the World System. Montreal: Black Rose.

Zolberg, Aristide R.
1974 "The making of Flemings and Walloons: Belgium: 1830–1914." Journal of Interdisciplinary History 22:178–235.

RECONCILING RACE AND CLASS DIFFERENCES IN SELF-REPORTED AND OFFICIAL ESTIMATES OF DELINQUENCY*

DELBERT S. ELLIOTT AND SUZANNE S. AGETON
Behavioral Research Institute, Boulder

American Sociological Review 1980, Vol. 45 (February):95–110

This paper addresses the general question of whether or not the satisfactory resolution of the methodological criticisms of self-report research will result in greater consistency between self-reported and official data with respect to the race and class distributions of delinquent behavior. We review the specific methodological criticisms of self-report delinquency (SRD) research; discuss the use of a new SRD measure in a national youth study; compare the race/class findings of this study with previous SRD research and with official arrest data; and examine the epidemiological and theoretical implications of these findings. Both class and race differentials are found in this study. It appears likely that the differences between these findings and those in earlier SRD studies are a result of differences in the specific SRD measures used. Additionally, these findings suggest a logical connection between SRD and official measures, and they provide some insight into the mechanism whereby official data produce more extreme race and class (as well as age and sex) differences than do self-report measures. The results of this study also have implications for previous tests of theoretical propositions which used self-report delinquency data. In short, prior self-report measures may not have been sensitive enough to capture the theoretically important differences in delinquency involvement.

Problems of conceptualization, definition, and measurement continue to plague researchers interested in the epidemiology and etiology of delinquency. While most would acknowledge the conceptual distinction between delinquent behavior and official responses to delinquent behavior, these distinctions are not clearly maintained in the measurement of delinquency or in the interpretation and analysis of specific delinquency data. This problem is clearly illustrated in the current controversy over the validity of self-reported (as compared with official) estimates of the incidence and distribution of delinquency in the general adolescent population. Put simply, there are those who argue that police and arrest records provide more accurate and reliable estimates of the social correlates of delinquent behavior than do self-report surveys; others hold the opposite view.

Self-report measures of delinquency provide a different picture of the incidence and distribution of delinquent behavior than do official arrest records. Both types of data indicate significant age and sex differentials, but the magnitude of these differences is much smaller with self-reported data than with official arrest data (Williams and Gold, 1972; Gold and Reimer, 1974; Elliott and Voss, 1974; Illinois Institute for Juvenile Research, 1973; Bachman et al., 1970; 1971; 1978).

At the center of the controversy, however, is the fact that self-report studies generally find no differences in delinquent behavior by class or race (Gold and Reimer, 1974; Elliott and Voss, 1974; Williams and Gold, 1972; Hirschi, 1969; Bachman et al., 1970; 1971; 1978; Illinois Institute for Juvenile Research, 1973), while studies relying upon police and court data report significant differences by both class and race (Wolfgang et al., 1972; Elliott and Voss, 1974; Williams and Gold, 1972; Gordon, 1976; West, 1973; Short and Nye, 1957–1958).

To date, attempts to reconcile this apparent discrepancy between official and self-reported findings have taken one of two approaches. Most recently, researchers have challenged the strength of the empirical evidence for the class differ-

---

* Address all communications to: Delbert S. Elliott, Director; Behavioral Research Institute; 2305 Canyon Boulevard; Boulder, Colorado 80302.

This study was supported by the Center for Studies of Crime and Delinquency, NIMH (MH27552), and the National Institute for Juvenile Justice and Delinquency Prevention, LEAA (78–JN–AX–0003). We gratefully acknowledge the assistance of Judy Beth Berg-Hansen in the preparation of the manuscript.

ential in official data. Tittle and Villemez (1977) and Tittle et al. (1978) reviewed earlier published research findings and concluded that the class differences in official data are not clearly established and that the widespread belief in an inverse relationship between class and crime is not based upon sound empirical research findings. Hindelang et al. (1978) have also concluded that police records of juvenile offenses are not strongly or even moderately related to socioeconomic status.[1] On the other hand, the race differential in official data has not been seriously challenged, to our knowledge.

The second and most frequent approach has been to challenge the methodological adequacy of the self-report technique and the adequacy of self-report research. Specifically, critics of self-report research contend:

(1) There are problems inherent in the method itself which make it inaccurate and unreliable. These problems include deliberate falsification, inaccurate recall, and forward and backward telescoping.

(2) There are problems with the construction of measures used in self-report research and with the procedures for administering the measures. These problems concern the lack of representativity in items, item overlapping, imprecise response sets, and the lack of anonymity of respondents.

(3) There are problems with generalizing from self-report studies, due to the almost exclusive reliance upon small, unrepresentative samples.

This paper is concerned with this second approach to reconciling official and self-reported findings with respect to class and race. We will not deal with those problems inherent in the self-report method itself, except to note that available research seems to support both the validity and reliability of the method (Nye and Short, 1956; Erickson and Empey, 1963; Hirschi, 1969; Gold, 1966; Dentler and Monroe, 1961; Hardt and Bodine, 1965; Hardt and Hardt, 1977; Farrington, 1973; Elliott and Voss, 1974; Clark and Tifft, 1966). Instead, we will deal with the correctable problems, i.e., the construction of measures and their administration, as well as representativity and sample size. The general question we will address here is whether or not the satisfactory resolution of these methodological issues in the construction and administration of self-report measures will result in greater consistency between self-reported and official data. More specifically, will the satisfactory resolution of these problems produce race and class differentials in self-reported estimates of delinquent behavior?

The discussion that follows will focus on: (1) the methodological criticisms of previous self-report delinquency (SRD) research; (2) the use of a new SRD measure in a national youth study; (3) a comparison of the race/class findings of this study with previous SRD research and with official arrest data; and (4) the epidemiological and theoretical implications of these findings.

PROBLEMS WITH SRD RESEARCH

Instrument Construction

Much of the controversy over self-report measures involves problems with instrument construction. Primarily, criticism centers on three issues: (1) the question of the representativeness of items employed in SRD measures; (2) problems of item overlap; and (3) limited or ambiguous response sets.

The major criticism concerns the unrepresentativity of the items selected (Hindelang et al., 1975; Nettler, 1974; Farrington, 1973). Trivial and nonserious offenses (e.g., cutting classes and disobeying parents) are often overrepresented, while serious violations of the criminal code (e.g., burglary, robbery, and sexual assault) are frequently omitted. In addition, many SRD measures tend to overrepresent certain behavioral dimensions (e.g., theft) to the exclusion of other relevant delinquent acts. As a result of such selection processes, most existing SRD measures have a restricted focus and do not represent the full range of delinquent acts; this limits the appropriateness of these scales as general measures of delinquent behavior.

---

[1] Hindelang et al. (1978) have also recently studied the extent to which sex and race are differentially related to self-reported and official records of delinquency, and some of their conclusions with regard to the race discrepancy will be cited later.

Another problem is the overlapping nature of items often included in SRD measures, which results in inaccurate estimates of frequency due to duplicate counts of certain events. For example, many SRD measures include a "shoplifting" item, a "theft under $5" item, and a "theft $5–50" item. A single theft event could logically be reported on two of these items. The presence of both a "cutting school" and a "cutting class" item represents another form of measurement redundancy since cutting school necessarily involves cutting classes. This problem is not easily overcome, since a given behavioral event in fact may involve more than a single offense. Nevertheless, item overlapping creates a potential source of error in estimating the volume of delinquent behavior from SRD measures.

The type of response sets typically employed with SRD measures has been another source of criticism. One major concern has been the frequent use of normative response categories such as "often," "sometimes," and "occasionally." This type of response set is open to wide variations in interpretation by respondents, and precludes any precise count of the actual number of acts committed.

Other response sets used to estimate the number of behaviors (e.g., "never," "once or twice," and "three times or more") have been challenged on the grounds that they are not precise categories for numerical estimation, and that numerical estimates based upon such categories may severely truncate the true distribution of responses. With the above set, for example, any number of behaviors in excess of two is collapsed into a single "high" category. While this procedure may allow for some discrimination between youth at the low end of the frequency distribution, it clearly precludes any discrimination at the high end. Thus, a youth involved in three shoplifting offenses during the specified period receives the same "score" as a youth involved in 50 or 100 shoplifting events during the period. This limited set of categorical responses appears particularly problematic when the reporting period involves a year or more and when such items as using marijuana; drinking beer, wine or liquor; and carrying a concealed weapon are included in the SRD measure.

Administration Procedures

The manner in which the measures are administered is also a problematic area for self-report delinquency research. Here the issue concerns: (1) anonymous vs. identified respondents, and (2) questionnaire vs. interview formats.

Many researchers have argued that anonymity has to be guaranteed or youth will not admit certain offenses—probably the more serious, stigmatizing ones. Research on this question suggests that there is slightly more reporting of offenses under conditions of anonymity, but that anonymous/identified differences are slight and statistically insignificant (Corey, 1937; Christie, 1965; Kulik et al., 1968). These findings have led Dentler (as cited in Hardt and Bodine, 1965) to comment that the necessity for anonymity is overemphasized, and that it may in fact lead to reduced involvement by respondents and careless or facetious answers.

On the matter of interview vs. questionnaire formats, the discussion again involves the issue of anonymity, and the belief that self-administered questionnaires are more likely to produce accurate responses than personal interviews. One recent self-report delinquency research study compared results from structured interviews and self-administered check lists, where anonymity was guaranteed under both conditions (Krohn et al., 1974). While seven of the eight offenses were admitted more often under check-list conditions than under interview conditions, none of the differences was significant at the .05 level. Even when education, sex, class, and IQ were controlled, no significant differences were obtained.

Some researchers, most notably Gold (1966), have argued that the interview format has significant advantages for delinquency research in that it permits clarification of specific behaviors and, consequently, the ability to more correctly classify illegal acts. In general, however, there is still controversy over the effects of specific administration procedures when the research is directed toward illegal or socially disapproved behaviors.

Sampling Design and Generality

Another problematic area for self-report delinquency research is that of the generality of findings. Here the question focuses primarily on the adequacy of the sampling designs for: (1) inferences to the adolescent population, and (2) comparisons with official data.

In most cases, SRD measures have been administered to small, select samples of youth, such as high school students in a particular local community or adolescents processed by a local juvenile court. The samples are rarely probability samples, and generalizations (about the adolescent populations sampled) cannot be made with any known degree of accuracy. Only two published studies involve national probability samples.[2]

A further concern is that few cohort studies using normal populations have incorporated a self-report measure in their instruments. This means that the age and sex gradients of SRD measures are not known, a critical fact if this measurement approach is to become more refined and useful. Furthermore, since the studies using self-report measures have almost always been cross-sectional ones, little is known about the dynamics of self-reported behavior over time.

Finally, Empey (1978) notes that national self-report studies have not been conducted on an annual basis and, as a result, it is not possible to discern trends across time or to make direct comparisons with other standard delinquency data such as the Uniform Crime Reports (UCR) or the National Crime Panel (NCP).

THE NATIONAL YOUTH SURVEY

We will now report on a national youth study in which we have attempted to deal with the previously noted methodological criticisms of self-report delinquency research. Our aim will be to see if these improvements in the quality of self-report research have any impact on self-report findings relative to findings from official data.

The National Youth Survey involves a five-year panel design with a national probability sample of 1,726 adolescents aged 11–17 in 1976.[3] The total youth sample was selected and initially interviewed between January and March, 1977, concerning their involvement in delinquent behavior during the calendar year 1976. The second survey was completed between January and March, 1978, to obtain delinquency estimates for the calendar year 1977. The third, fourth, and fifth surveys will also be conducted between each January and March of the years 1979, 1980, and 1981.

The data reported herein are taken from the first survey, completed in 1977. The estimates presented are thus for delinquent behavior during the calendar year 1976.

Construction of New SRD Measure

In constructing the SRD measure for this study, we attempted to obtain a representative set of offenses. Given our interest in comparing SRD and UCR estimates, we began by listing offenses included in the UCR. Any specific act (with the exception of traffic violations) involving more than 1% of the reported juvenile arrests for 1972–1974 is included in the SRD measure.

In addition to the list of specific offenses, the UCR contains a general category, "all other offenses," which often accounts for a high proportion of the total juvenile arrests. To cover the types of acts likely to fall within this general category, and to increase the comprehensiveness of the measure, two general selection criteria were used to choose additional items. First, items which were theoretically relevant to a delinquent lifestyle or subculture as discussed in the literature were selected for inclusion in this measure (Cohen, 1955; Cloward and Ohlin, 1960; Miller, 1958; 1966; Yablonsky, 1962; Short and Strodtbeck, 1965). Thus, additional items—such as gang fighting, sexual intercourse, and carrying a hidden weapon—are included. Second, a systematic review of existing SRD measures was undertaken to locate items that tapped specific dimensions of delinquent behavior not previously included.

---

[2] These are Gold's 1967 and 1972 National Surveys of Youth and the Youth in Transition Study (Bachman et al., 1970; 1971; 1978). Both studies are limited by a restricted set of SRD items and truncated response sets. The Youth in Transition Study also has problems associated with overlapping reporting periods for its reported SRD measures.

[3] The National Youth Survey is funded by the Center for Studies of Crime and Delinquency, NIMH (MH27552), and the National Institute for Juvenile Justice and Delinquency Prevention, LEAA (78–JN–AX–0003). Current funding covers the first three of the five projected annual surveys. The first survey was funded solely by NIMH, the second and third by NIMH and LEAA jointly.

We believe the resulting set of 47 items to be both more comprehensive and more representative of the conceptual universe of delinquent acts then found in prior SRD measures used in major, large-scale studies. The item set includes all but one of the UCR Part I offenses (homicide is excluded); 60% of Part II offenses; and a wide range of "other" offenses—such as delinquent lifestyle items, misdemeanors, and some status offenses. The vast majority of items involve a violation of criminal statutes. (See Appendix A.)

Two separate response sets are being used. Respondents initially are asked to indicate how many times during the past year they committed each act. If an individual's response to this open-ended question involves a frequency of 10 or more, interviewers then ask the youth to select one of the following categorical responses: (1) once a month, (2) once every 2–3 weeks, (3) once a week, (4) 2–3 times a week, (5) once a day, or (6) 2–3 times a day.[4] A comparison of the two response sets indicates high agreement between frequency estimates given in direct response to the open-ended question and frequency estimates based upon the implied frequency associated with the midpoint of the category selected.[5]

A specific attempt was also made to eliminate as much overlap in items as possible. None of the items contains a necessary overlap as in "cutting school" and "cutting class." Although some possible overlap remains, we do not feel it constitutes a serious problem with this SRD measure.

The SRD measure asks respondents to indicate how many times, "from Christmas a year ago to the Christmas just past," they committed each offense. The recall period is thus a year, anchored by a specific reference point relevant to most youth. The use of a one-year period which coincides almost precisely with the calendar year allows for direct comparison with UCR data, NCP victimization data, and some prior SRD data. It also avoids the need to adjust for seasonal variations, which would be necessary if a shorter time period were involved.

Administration Procedures

For the present study, the research design (a longitudinal panel design) precludes a guarantee of anonymity. Therefore, our major concern is to guarantee respondents that their answers will be confidential. This assurance is given verbally as well as being contained in the written consent form signed by all youth and their parents. In addition, a Certificate of Confidentiality from the Department of Health, Education, and Welfare guarantees all respondents that the data and the interviewers will be protected from legal subpoena.

The interview format was selected over the self-administered questionnaire format for several reasons. First, we share Gold's belief that the interview situation (if properly structured to protect confidentiality) can insure more accurate, reliable data. Second, the necessity of securing informed consents from all subjects and the complexity of the present research require, in our judgment, a personal contact with the respondents. Once this contact is made, it seems logical to use the interviewer to facilitate the data collection process and to improve the quality of the data obtained. Finally, some of our previous research suggests that the differences in responding to SRD items in a questionnaire as opposed to an interview format are not significant (Elliott et al., 1976).

---

[4] The categorical response set has led to the identification of some highly episodic events, e.g., 20 shoplifting offenses, all occurring within a two-month period during the summer (an initial response of 20; a categorical response 2–3 times a week, and an interviewer probe revealing that the offenses all occurred during the summer).

[5] The only exception involves the last two (high frequency) categories. At this end of the frequency continuum, estimates based upon the midpoint of the category are substantially higher than the frequency response given directly. The open-ended frequency measure thus appears to provide a more conservative estimate of number of delinquent acts, and the estimates reported here are based upon this response.

The Sample

The 1977 National Youth Survey employed a probability sample of households in the continental United States based upon a multistage, cluster sampling design. The sample contained 2,375 eligible youth aged 11–17 in 1976. Of these, 1,726 (73%) agreed to participate in the study and completed interviews in the 1977 survey.[6] A comparison of the age, sex, and race of eligible youth not interviewed with participating youth indicates that the loss rate from any particular age, sex, or racial group appears to be proportional to that group's representation in the population. Further, with respect to these characteristics, participating youth appear to be representative of the total 11 through 17-year-old youth population in the United States as established by the U.S. Census Bureau (Huizinga, 1978).

Summary of New Measure

In sum, the current SRD measure addresses many of the central criticisms of prior SRD measures.[7] It is more representative of the full range of delinquent acts than were prior SRD measures and involves fewer overlapping items; it also employs a response set which provides better discrimination at the high end of the frequency continuum and is more suited to estimating the actual number of behaviors committed. The choice of a one-year time frame with a panel design involving a one-year time lag is based upon both conceptual and practical concerns. Compared with the other SRD measures, the measure involves a moderate recall period, captures seasonal variations, and permits a direct comparison with other self-report and official measures which are reported annually. And, finally, the study involves a national probability sample of youth aged 11–17.

ANALYSIS OF DATA

Subscales

An earlier paper (Ageton and Elliott, 1978) presented design effects and estimates of the frequency of each specific item on the SRD measure with .95 confidence intervals for the total sample and by age, sex, race, and class. The analysis in this paper focuses upon the total SRD measure and a set of specific subscales. The frequency estimates are based upon the open-ended response set, which provides slightly more conservative frequency estimates than does the categorical response set.

---

[6] At each stage, the probabilities of selection were established to provide a self-weighting sample. Seventy-six primary sampling units were selected, with probability of selection being proportional to size. This sampling procedure resulted in the listing of 67,266 households, of which approximately 8,000 were selected for inclusion in the sample. All 11- through 17-year-old youth living in the selected households were eligible respondents for the study. The selected households generated a total of 2,375 eligible youth. Of these, 649 (27%) did not participate in the study due to (1) parental refusal, (2) youth refusal, or (3) the youth being considered inappropriate for inclusion in the study (e.g., severely mentally retarded). In general, based upon a comparison with 1976 U.S. Census Bureau estimates, the resulting sample of participating youth does appear representative of American youth with respect to age, sex, and race (U.S. Bureau of the Census, 1977). For a detailed description of the sample, see Huizinga, 1978, and Ageton and Elliott, 1978.

[7] While we are concerned with the issues of validity and reliability of the self-report method, we will not deal with these issues here beyond reporting that: (1) we are obtaining data on official police contacts for each respondent and intend to compare self-reports of delinquent behavior with official records of police contact and arrest; and (2) the reliability (internal consistency) of the new SRD measure is quite high (Alpha = .91) for the 1977 survey. We are planning further validity tests with these data, but have nothing to report, yet.

The subscales are based upon Glaser's (1967) offense typology. This particular classification scheme was selected because it offers a logical categorization of offense types while permitting a clear distinction between serious and nonserious crimes.

Glaser's typology encompasses four major types of crime: (1) predatory crimes, (2) illegal service crimes, (3) public disorder crimes, and (4) crimes of negligence. With the exception of category four, all Glaser's types were used.[8]

Furthermore, Glaser acknowledges an additional category called status crimes to encompass those behaviors which at special times and/or for particular classes of people have been illegal. The status crimes category in this national youth study includes all behaviors in the SRD measure which are illegal only when the individual involved is a minor.

In addition, a separate category entitled hard drug use was created to distinguish this type of drug involvement from that of alcohol and/or marijuana use, which are subsumed within the status crimes category and public disorder crimes category, respectively.

Finally, within the predatory crimes category, a distinction was made between crimes against persons and crimes against property. This differentiation separates violent crimes against people from other serious offenses which do not involve confrontation with another person.

Thus, the final offense typology is composed of the following six subscales:

(1) predatory crimes against persons (sexual assault, aggravated assault, simple assault, and robbery);
(2) predatory crimes against property (vandalism, burglary, auto theft, larceny, stolen goods, fraud, and joyriding);
(3) illegal service crimes (prostitution, selling drugs, and buying/providing liquor for minors);
(4) public disorder crimes (carrying a concealed weapon, hitchhiking, disorderly conduct, drunkenness, panhandling, making obscene phone calls, and marijuana use);
(5) status crimes (runaway, sexual intercourse, alcohol use, and truancy);
(6) hard drug use (amphetamines, barbiturates, hallucinogens, heroin, and cocaine).

Table 1 presents the mean frequency of self-reported delinquency by race[9] and class[10] for the total SRD measure and for each of the subscales. The statistical tests involve a one-way analysis of variance on these means.[11]

---

[8] Category Four, crimes of negligence, was excluded because it contains predominantly automobile infractions which were not included in the SRD measure.

[9] Because of the small number of Mexican-American respondents (N = 72) and some obvious clustering effects for this ethnic group, the accuracy of the estimates of variances is questionable, and we have not included this group in the analysis. The same situation holds for the residual "other" category (N = 32). Comparisons are, thus, limited to whites (Anglos) and blacks.

[10] The social class measure employed in this analysis is the Hollingshead two-factor index (Hollingshead and Redlich, 1958) as applied to the principal wage earner in each youth's family. Hollingshead Classes I and II—involving primarily professional managerial occupations and college level educations—are collapsed to make the "middle" class category. Class III—primarily owners of small businesses, clerical workers, and persons in sales occupations and skilled manual occupations, with high school or some college work completed—constitutes the "working" class category.

Classes IV and V—primarily semiskilled persons and those in unskilled manual occupations with high school or lower levels of education—make up the "lower" class category.

[11] It should also be noted that the statistical tests are based upon a simple random sample design. The effect of our departure from this design is probably in the direction of inflating the F values. However, the design effects are small, suggesting that the effect of this departure is not a serious one. For example, the average design effects on items in the total SRD measure are as follows: Males = 1.13; Females = 1.12; 11–12-year-olds = 1.14; 13–15-year-olds = 1.08; 16–17-year-olds = 1.05; Middle Class = 1.05; Working Class = 1.22; Lower Class = 1.09; Whites = 1.14 and Blacks = 1.15. Further, unless otherwise specified, statistical significance refers to probabilities $\leq .05$. Because of our sample sizes, the statistical tests employed are rather powerful. An examination of Table 1 indicates very large variances and, even when significant differences in means are found, it is clear that the distributions being compared overlap substantially.

Race and Class Differentials

Unlike most previous self-report studies, we find significant race differences for total SRD and for predatory crimes against property. Blacks report significantly higher frequencies than do whites on each of these measures. In both cases, the differences in means are substantial. With respect to total offenses, blacks report three offenses for every two reported by whites. For crimes against property, blacks report more than two offenses for every offense reported by whites.

While there is a substantial difference in mean scores on the crimes against persons scale, it is not statistically significant. The difference in the total SRD score appears to be primarily the result of the very high level of property crimes reported by blacks.

We also observe a class differential for total SRD and for predatory crimes against persons. For total SRD scores, the difference is between lower socioeconomic status youth and others; i.e., there does not appear to be any difference between working- and middle-class group means.

The differences are greater, and the trend is more linear, for the crimes against persons scale means than for total SRD. Lower-class youth report nearly four times as many offenses as do middle-class youth and one-and-one-half times as many as working-class youth. There is also a substantial class difference in the mean number of crimes against persons, but this difference is not statistically significant. There are clearly no substantial differences in means for any of the remaining subscales.

A two-way analysis of variance (race × class) was also completed with the total SRD measure and each of the subscales. In every case, the direct effects observed in the one-way analysis of variance were replicated and no significant interaction effects were observed.

COMPARISONS WITH PRIOR STUDIES

Our basic concern in this analysis will now be to ascertain: (1) the comparability

Table 1. Mean Frequency of Self-Reported Delinquency by Race and Class

	Total Self-Reported Delinquency			Predatory Crimes Against Persons		Predatory Crimes Against Property		Illegal Service		Public Disorder		Hard Drug Use		Status Offenses	
	N	$\bar{X}$	SD	$\bar{X}$	SD	$\bar{X}$	SD	$\bar{X}$	SD	$\bar{X}$	SD	$\bar{X}$	SD	$\bar{X}$	SD
RACE															
White	1357	46.79	161.37	7.84	58.42	8.93	42.87	1.85	22.36	14.98	62.37	1.26	14.03	14.84	49.21
Black	259	79.20	277.38	12.96	76.59	20.57	106.27	1.71	22.83	16.50	67.78	.18	1.35	16.19	53.94
F		6.68		1.50		8.79		.09		.13		1.55		.16	
Probability		⩽.01		NS		⩽.001		NS		NS		NS		NS	
CLASS															
Lower	717	60.42	220.24	12.02	72.68	13.50	78.60	2.19	30.29	14.32	58.75	1.20	14.64	14.27	46.54
Working	509	50.63	186.19	8.04	67.12	9.40	38.68	1.36	10.92	16.21	78.92	.73	4.02	14.47	42.36
Middle	494	50.96	79.88	3.32	11.31	7.25	23.17	1.56	14.07	13.81	42.44	1.37	15.68	15.66	58.33
F		2.94		3.11		1.94		.25		.22		.35		.13	
Probability		⩽.05		⩽.05		NS		NS		NS		NS		NS	

of the general findings reported here with other SRD findings, and (2) the extent to which these findings might offer some insight into the discrepancy between self-report and official measures of delinquency.

While we have not reported findings from this study relative to the age and sex distribution of delinquent behavior, they are generally consistent with those from other large-scale self-report studies (Hirschi, 1969; Williams and Gold, 1972; Illinois Institute for Juvenile Research, 1973; Gold and Reimer, 1974; Elliott and Voss, 1974). However, the findings relative to the distribution by race and class are not consistent with these earlier studies, none of which report significant differences in the mean frequencies of self-reported offenses by race or class.[12]

In this study, the black/white differential in total offenses is nearly 2:1; and for predatory crimes, over 2:1. Class differences in total offenses are significant and, while the differences are not large, they are in the traditionally expected direction, with lower-class youth reporting higher frequencies. The trend in the previously referenced self-report studies is generally in the opposite direction.

In this study, class differences are clearer on the predatory crimes against persons scale than for total SRD scores; the trend is more linear, in the expected direction, and of greater magnitude (lower:middle = 3:1).

In an effort to determine if this basic difference in findings is due to the differences in the SRD measures employed, we undertook several additional analyses. We "re-scored" the frequency estimates in two ways to approximate the frequency ranges used in the earlier studies. Since our focus is upon those scales where significant class and race differences are observed, our discussion will be limited to the total self-report measure and the two predatory subscales.

First, a comparison of the proportions of youth reporting one or more offenses on each of these scales reveals no statistically significant class or racial differences. Second, all frequencies of three and above on each item in the SRD measure were re-scored as a 3 to approximate the frequency ranges used in the Richmond Youth Study (Hirschi, 1969), the Delinquency and Dropout Study (Elliott and Voss, 1974), and the 1967 and 1972 National Surveys of Youth (Williams and Gold, 1972; and Gold and Reimer, 1974). Frequency scores on items were then summed across items to generate new scale frequencies and means. With one exception, an analysis of race and class differences on these frequency scores reveals no significant differences. The one exception involves a small (but significant) class difference on the predatory crimes against persons scale.[13] The class differential in this case is in the same direction but substantially smaller than that observed with the original scoring.

These two analyses indicate that the extended frequency range used in this study does, in fact, contribute to the difference in findings relative to race and class. Had we used the proportion of youth committing one or more offenses, or a 0–3 range similar to that used in the 1967 and 1972 National Surveys of Youth, our conclusions relative to class and race differences would have been similar to those reported for the earlier self-report studies.

High-Frequency Offenders

Since the above analyses indicate that differences were not occurring at the low end of the frequency distribution (with one exception), we examined race and class differences at several points along the original frequency distribution, with a particular interest in the high end of the

---

[12] Williams and Gold (1972) report a white/black race differential for seriousness of self-reported delinquency behavior, but no difference in frequency. They also report a significantly higher seriousness for higher socioeconomic status (SES) boys, but no differences in frequency by SES. We have not considered seriousness here but, to the extent that our predatory crime subscales reflect the more serious offenses, our data would support the race differential and contradict the direction of the class differential in seriousness as reported by Williams and Gold.

[13] Mean frequencies were as follows: Lower = 2.02; Working = 1.74; Middle = 1.38.

Table 2. Percentage of Respondents Reporting Specific Levels of Delinquency by Race and Class

	Total Self-Reported Delinquency				
	Race		Class		
Number of Offenses Reported	White %	Black %	Lower %	Working %	Middle %
0–24	71.8	67.6	71.7	72.3	70.9
25–49	11.0	8.1	10.6	9.4	11.5
50–199	13.1	15.4	11.4	14.4	14.4
200+	4.1	9.8	6.3	3.9	3.2
	Predatory Crimes Against Property		Predatory Crimes Against Persons		
	Race		Class		
Number of Offenses Reported	White %	Black %	Lower %	Working %	Middle %
0–4	70.6	70.7	77.3	80.0	84.6
5–29	24.1	22.7	18.2	16.1	13.8
30–54	3.4	2.4	1.7	2.1	.8
55+	1.9	4.2	2.8	1.8	.8

frequency continuum. The results of this analysis are presented in Table 2.

The data in Table 2 indicate that the original differences, by both race and class, are due in large part to the relative differences at the high end of the frequency continuum. For example, at the low end of the frequency distribution, the white/black ratios are close to 1:1; but at the high end of the continuum the ratios are greater than 1:2.[14] For class, the lower-class to middle-class ratios at the low end are again close to 1:1; but at the high end the ratio is 2:1 for total SRD and over 3:1 for predatory crimes against persons.

Not only are the relative *proportions* of blacks and lower-class youth higher at the high end of the frequency continuum but also, within the high category, blacks and lower-class youth report substantially higher *frequencies* than do whites and middle-class youth.

Lower-class youth in the high category on the total SRD measures (scores ⩾ 200) report over one-and-one-half times as many offenses as do middle-class youth in this category. On the predatory crimes against persons scale, lower-class youth in the high category (scores ⩾ 55) report nearly three times as many offenses as do middle-class youth in this category. Among those in the high category on the predatory property scale, blacks report over one-and-one-half times as many offenses as do whites.

The one exception to the above generalization is that blacks and whites within the high category on the total SRD measure have approximately the same mean frequencies. The black/white differential on this measure thus appears to be more simply the result of differences in proportions of blacks and whites in the high category.

In any case, blacks and lower-class youth are found disproportionately among high frequency offenders. Prior self-report measures were unable to detect this differential involvement because they used response sets which are sensitive to differences only at the low end of the frequency continuum. We thus conclude that the use of open-ended frequency responses does, in fact, contribute to our finding both class and race differentials.

Range of Offenses in SRD Measure

We also believe that the broader range of offenses included in our SRD measure has some effect on these findings. An item-by-item analysis indicates that the particular set of items included in a measure can have a major impact on observed ethnic and class differentials. For example, the predatory crimes against persons scale includes four types of offenses: sexual assault, aggravated assault, simple assault, and robbery. Mean frequencies, by race, on the three types of assault offenses are as follows:

Offense Type	Whites	Blacks	Ratio
Sexual Assault	.03	.15	1:5
Aggravated Assault	.12	.50	1:4
Simple Assault	2.07	1.05	2:1
Total Assault	2.22	1.70	1:1

With respect to all three types of assault, these data indicate a major difference in frequency, by race. They suggest that measures which include only simple assault items do not have a proper representation of assault items. Given the tendency of prior self-report studies to exclude the more serious assault items, findings from such studies are likely to minimize the racial differences regarding assaultive behavior.

---

[14] For purposes of discussion, the low end of the frequency distribution refers to total SRD scores ⩽ 24 and predatory crime against persons and property scores ⩽ 4. The high end refers to total SRD scores ⩾ 200 and predatory crimes against persons and property scores ⩾ 55.

Even when all three types of assault are represented, an analysis involving a summary assault measure would obscure, in this case, important differences by type of assault. For example, the magnitude of sexual and aggravated assault offenses is so small, compared with that of simple assault offenses, that the inclusion of the latter in a summary measure conceals major differences in the other items and even reverses the direction of the differential.

While assault items have been used for illustrative purposes, we believe the problem to be a general one. The inclusion of both very high and very low frequency items in a single measure may obscure important differences, often differences in seriousness. This is because nonserious offenses tend to be reported frequently, while serious offenses are reported infrequently.

Weighting SRD Items

For some purposes, it may be instructive to weight each item so that its potential contribution to the total score is the same as that of any other item. In this case, no single high frequency item can dominate the overall score. One procedure for accomplishing this weighting is to transform item frequency scores into standard scores and then add them into a total scale score. This transformation was performed, and the results are presented in Table 3.

With respect to class differences, this transformation confirms the earlier findings observed with raw frequencies. Significant class differences are observed for total SRD scores and predatory crimes against persons scores, but not for predatory crimes against property scores. In both cases, the significance levels are higher when item scores are transformed into standard scores prior to summing.

Table 3. Mean Standardized Scores for Selected Scales by Race and Class

	Total Self-Reported Delinquency	Predatory Crimes Against Persons	Predatory Crimes Against Property
RACE			
White	−.2452	−.0860	−.0072
Black	1.5317	.4038	−.0117
F	3.870	8.179	.001
Probability	≤.05	≤.005	NS
CLASS			
Lower	.7375	.2194	−.0905
Working	.1131	−.0746	.1501
Middle	−1.3341	−.2486	−.080
F	3.771	5.159	1.975
Probability	≤.02	≤.006	NS

With regard to racial differences, the black/white difference on the total SRD measure remains, but it is weaker than that observed with raw frequency scores. More importantly, with this transformation the race difference on the predatory property crimes scale (observed earlier) is not replicated. There is essentially no black/white difference in property crimes when separate property offenses are adjusted so as to have the same magnitudes. This change suggests that the higher raw frequency predatory property scale mean for blacks is largely the result of higher frequencies on selected offense items. A review of item specific means confirms that this is indeed the case.

The mean number of vandalism offenses reported by blacks is over three times that of whites, and their mean number of evading payment offenses is over five times that of whites. These two nonserious offenses are relatively high frequency offenses for all youth. The individual item means for blacks are equal to or lower than those of whites on all serious offenses (felonies) included in this scale. Further, with one exception (family theft), means for blacks on all theft items are equal to or lower than those of whites. Thus, the initial racial differences on predatory property offenses are the result of large differences in vandalism (property destruction) and evading payment offenses, and not differences in theft offenses or serious property offenses.

Whereas there are no statistically significant racial differences in predatory crimes against persons offenses when unadjusted frequencies are used, the use of standard scores results in significant differences, with blacks reporting significantly higher scores. The direction of the racial differential is the same as that observed with the use of unadjusted frequencies, but the differential is stronger and statistically significant when standard scores are used.

As noted earlier in the discussion of specific assault items, simple assault is a relatively high frequency offense compared with the other offenses against persons, and whites report substantially higher frequencies than blacks on this offense. On the more serious offenses against persons, magnitudes are relatively low, but blacks report substantially greater frequencies than do whites on most of these items. The only exception involves robbery; here, the black and white means are identical. The transformation of item scores thus results in a greater racial differential than that observed with unadjusted frequencies.

The review and analysis of prior SRD studies by Hindelang et al. (1978) led them to conclude that racial differentials increase with the severity of the offense. The data in our National Youth Survey provide only partial support for that generalization. With respect to offenses against persons, the black/white differential is indeed greater for serious (as compared with nonserious) offenses. However, with respect to property offenses this is not the case. In fact, the opposite is true; i.e., the differential is greater for nonserious property offenses.

When unadjusted item scores are used, there are no significant class by race interactions on any of the self-reported delinquency scales. With the transformation to standard item scores, a significant class by race interaction is observed on the predatory crimes against persons measure. The results of the two-way analysis of variance are presented in Table 4.

Table 4. Two-Way Analysis of Variance: Mean Predatory Crimes Against Persons Scale Scores (Standardized) by Race and Class

	CLASS		
RACE	Lower	Working	Middle
White	.013	−.030	−.267
Black	.828	−.253	−.143

Main Effects	Sum of Squares	DF	F	Significance Level
Race	39.818	1	6.256	≤.01
Class	45.149	2	3.547	≤.03
Interaction				
Race x Class	40.944	2	3.216	≤.04

Main effects are observed for both race and class. For whites, the major class difference is between middle-class youth and others (as was the case with unadjusted frequency scores). For blacks, the differences between lower-, working-, and middle-class youth are relatively greater, but the major difference is between lower-class youth and others. Working-class black youth report relatively fewer offenses against persons than do middle-class blacks, but this difference is not great. The interaction effect appears to be primarily the result of the relatively higher mean score for lower-class black youth. This analysis suggests a relatively high involvement of lower-class black youth in serious offenses against persons.

IMPLICATIONS OF FINDINGS

Item Selection and Response Sets

Three major points need to be considered relative to the selection and analysis of items in a self-report measure. First, it is critical that the items selected be representative of the offense areas to be covered. Special attention should be directed to this selection process, since the inclusion (or exclusion) of specific items appears to have implications for research findings, at least with regard to the social correlates of delinquent behavior.

Second, when attempting to make incidence estimates, it is important that response sets reflect the total frequency range. In this study, significant class and race differences were found only at the high end of the frequency range and, in most cases, would not have been observed had the traditional response sets been employed.

Third, from both theoretical and epidemiological perspectives, there is a danger in summarizing items into general categories. The present data indicate that using summary measures may obscure important differences by type of offense, and suggest that a more accurate picture is obtained by employing offense specific as well as summary measure analyses.[15]

It seems clear, from our data presentation and discussion, that the criticism of many SRD measures is well-founded; and, further, that generalizations about the social correlates of "delinquent behavior" as derived from measures with a limited response set and item representativity may be very misleading and are probably unwarranted. In addition, the way in which specific items are combined into summary measures has some bearing upon outcomes, and caution must be exercised in the interpretation of general summary measures.

Relationship between SRD and Official Data

In sum, it appears likely that the differences between the findings reported here and those from earlier self-report studies are, in fact, the result of differences in the specific SRD measures employed. The findings also suggest a logical connection between self-report and official measures of delinquency which, at least in part, accounts for the observed differences in the class and race distributions of these two measures.

The consistent findings of earlier self-report studies have led many sociologists and criminologists to the conclusion that race and class differences in arrests are primarily the result of processing biases and have little or no basis in behavior (Turk, 1969; Quinney, 1970; Williams and Gold, 1972; Taylor et al., 1974). The findings from the 1977 National Youth Survey suggest some behavioral basis for the observed class and race differences in official processing. In this sense, the National Youth Survey data are more consistent with official arrest data than are data from most prior self-report studies.

Further, these findings provide some insight into the mechanism whereby official actions produce exaggerated race and class (as well as age and sex) differences in delinquent behavior when compared with self-reported differences in normal adolescent populations. On both logical and empirical grounds, it seems reasonable to argue that the more frequent and serious offenders are more likely to be arrested (Short and Nye, 1957–1958; Erickson and Empey, 1963; Illinois Institute for Juvenile Research, 1973; Elliott and Voss, 1974; Empey, 1978), and that the youth population represented in official police statistics is not a representative sample of all youth.

Self-report studies are capturing a broader range of persons and levels of involvement in delinquent behavior than are official arrest statistics. Virtually all youth report some delinquent activity on self-report measures, but for the vast majority the offenses are neither very frequent nor very serious. Police contacts, on the other hand, are most likely to concern youth who are involved in either very serious or very frequent delinquent acts. Police contacts with youth thus involve a more restricted segment of the general youth population.

The findings discussed previously indicate that race and class differences are more extreme at the high end of the frequency continuum, that part of the delinquency continuum where police contacts are more likely.[16] In fact, at this end of the frequency continuum, self-report and official correlates of delinquent behavior are relatively similar. While we do not deny the existence of official processing biases, it does appear that official correlates of delinquency also reflect real differences in the frequency and seriousness of delinquent acts.

The results of this self-report delinquency study also have implications for previous tests of theoretical propositions which used SRD data. Stated simply, earlier self-report measures may not have been sensitive enough to capture the theoretically important differences in delinquency involvement. Given the truncated frequency distributions and the restricted behavioral range of earlier self-report measures, the only distinctions possible were fine gradations between relatively nondelinquent youth.

---

[15] While summary measures are reported herein, offense-specific analyses were presented in a previous paper (Ageton and Elliott, 1978).

[16] Although we have not presented the data here, age and sex differences are also greater at the high end of the SRD frequency continuum.

For example, earlier distinctions were typically among (1) youth with no reported offenses; (2) those with one or two offenses; and (3) those with three or more offenses. Given the extensive frequency distribution observed in this study, there is reason to question whether or not such a trichotomy would capture meaningful distinctions among offending youth. The most significant difference may not be between the nonoffender and the one-time offender, or even between the one-time and multiple-time offender. Equal or greater significance may be found between those reporting over (or under) 25 nonserious offenses, or between those reporting over (or under) five serious offenses.[17]

The ability to discriminate more fully among many levels and types of involvement in delinquent behavior introduces much more variance into the delinquency measure. That ability also allows for the identification of more extreme groups of offenders, and that identification may be particularly relevant for tests of theoretical propositions.

APPENDIX A

SELF-REPORTED DELINQUENCY AND DRUG-USE ITEMS AS EMPLOYED IN THE NATIONAL YOUTH SURVEY

How many times in the last year have you:

1. purposely damaged or destroyed property belonging to your *parents* or other family *members*.
2. purposely damaged or destroyed property belonging to a *school*.
3. purposely damaged or destroyed *other property* that did not belong to you (not counting family or school property).
4. stolen (or tried to steal) a *motor vehicle*, such as a car or motorcycle.
5. stolen (or tried to steal) something worth more than $50.
6. knowingly bought, sold or held stolen goods (or tried to do any of these things).
7. thrown objects (such as rocks, snowballs, or bottles) at cars or people.
8. run away from home.
9. lied about your age to gain entrance or to purchase something; for example, lying about your age to buy liquor or get into a movie.
10. carried a hidden weapon other than a plain pocket knife.
11. stolen (or tried to steal) things worth $5 or less.
12. attacked someone with the idea of seriously hurting or killing him/her.
13. been paid for having sexual relations with someone.
14. had sexual intercourse with a person of the opposite sex other than your wife/husband.
15. been involved in gang fights.
16. sold marijuana or hashish ("pot," "grass," "hash").
17. cheated on school tests.
18. hitchhiked where it was illegal to do so.
19. stolen money or other things from your *parents* or *other members of your family*.
20. hit (or threatened to hit) a *teacher* or other adult at school.
21. hit (or threatened to hit) one of your *parents*.
22. hit (or threatened to hit) other *students*.
23. been loud, rowdy, or unruly in a public place (disorderly conduct).
24. sold hard drugs, such as heroin, cocaine, and LSD.
25. taken a vehicle for a ride (drive) without the owner's permission.
26. bought or provided liquor for a minor.
27. had (or tried to have) sexual relations with someone against their will.
28. used force (strong-arm methods) to get money or things from other *students*.
29. used force (strong-arm methods) to get money or things from a *teacher* or other adult at school.
30. used force (strong-arm methods) to get money or things from *other people* (not students or teachers).
31. avoided paying for such things as movies, bus or subway rides, and food.
32. been drunk in a public place.
33. stolen (or tried to steal) things worth between $5 and $50.
34. stolen (or tried to steal) something at school, such as someone's coat from a classroom, locker, or cafeteria, or a book from the library.
35. broken into a building or vehicle (or tried to break in) to steal something or just to look around.
36. begged for money or things from strangers.
37. skipped classes without an excuse.
38. failed to return extra change that a cashier gave you by mistake.
39. been suspended from school.
40. made obscene telephone calls, such as calling someone and saying dirty things.

How often in the last year have you used:

41. alcoholic beverages (beer, wine and hard liquor).
42. marijuana—hashish ("grass," "pot," "hash").

---

[17] For example: for 1976, 5–10% of the most frequent offenders reported over 200 total offenses each, including over 30 serious offenses against persons.

43. hallucinogens ("LSD," "Mescaline," "Peyote," "Acid").
44. amphetamines ("Uppers," "Speed," "Whites").
45. barbiturates ("Downers," "Reds").
46. heroin ("Horse," "Smack").
47. cocaine ("Coke").

REFERENCES

Ageton, Suzanne S. and Delbert S. Elliott
1978 The Incidence of Delinquent Behavior in a National Probability Sample of Adolescents. Project Report Number 3. HEW Grant Number MH27552, The Dynamics of Delinquent Behavior: A National Survey. Boulder: Behavioral Research Institute.

Bachman, Jerald G., Swayzer Green, and Ilona D. Wirtanen
1970 Youth in Transition: Vol. II. Ann Arbor: Institute for Social Research, University of Michigan.
1971 Youth in Transition: Vol. III. Ann Arbor: Institute for Social Research, University of Michigan.

Bachman, Jerald G., Patrick O'Malley, and Jerome Johnston
1978 Youth in Transition: Vol. VI. Ann Arbor: Institute for Social Research, University of Michigan.

Christie, Nils
1965 Summary of His Report to the Conference. Pp. 1–3 in Robert H. Hardt and George E. Bodine, Development of Self-Report Instruments in Delinquency Research: A Conference Report. Syracuse: Syracuse University Youth Development Center.

Clark, John P. and Larry L. Tifft
1966 "Polygraph and interview validation of self-reported deviant behavior." American Sociological Review 31:516–23.

Cloward, Richard A. and Lloyd E. Ohlin
1960 Delinquency and Opportunity. Glencoe, Ill.: Free Press.

Cohen, Albert K.
1955 Delinquent Boys: The Culture of the Gang. Glencoe, Ill.: Free Press.

Corey, Stephen M.
1937 "Signed versus unsigned questionnaires." Journal of Educational Psychology 28:144–8.

Dentler, Robert A. and Lawrence J. Monroe
1961 "Social correlates of early adolescent theft." American Sociological Review 26:733–43.

Elliott, Delbert S. and Harwin Voss
1974 Delinquency and Dropout. Lexington, Mass.: D. C. Heath.

Elliott, Delbert S., Suzanne S. Ageton, Margaret Hunter, and Brian Knowles
1976 Research Handbook for Community Planning and Feedback Instruments (Revised). Vol. 1. Prepared for the Office of Youth Development, Department of Health, Education, and Welfare, HEW-OS-74-308. Boulder: Behavioral Research and Evaluation Corporation.

Empey, LaMar T.
1978 American Delinquency. Homewood, Ill.: Dorsey.

Erickson, Maynard L. and LaMar T. Empey
1963 "Court records, undetected delinquency and decision-making." Journal of Criminal Law, Criminology and Police Science 54:456–69.

Farrington, David P.
1973 "Self-reports of deviant behavior: predictive and stable?" Journal of Criminal Law and Criminology 64:99–110.

Glaser, Daniel
1967 "National goals and indicators for the reduction of crime and delinquency." Annals of the American Academy of Political and Social Science 371:104–26.

Gold, Martin
1966 "Undetected delinquent behavior." Journal of Research in Crime and Delinquency 3:27–46.

Gold, Martin and David J. Reimer
1974 Changing Patterns of Delinquent Behavior among Americans 13 to 16 Years Old—1972. Report Number 1 of the National Survey of Youth, 1972. Ann Arbor: Institute for Social Research, University of Michigan.

Gordon, Robert A.
1976 "Prevalence: the rare datum in delinquency measurement and its implications for the theory of delinquency." Pp. 201–84 in Malcolm W. Klein (ed.), The Juvenile Justice System. Beverly Hills: Sage.

Hardt, Robert H. and George E. Bodine
1965 Development of Self-Report Instruments in Delinquency Research: A Conference Report. Syracuse: Syracuse University Youth Development Center.

Hardt, Robert H. and Sandra Peterson-Hardt
1977 "On determining the quality of the delinquency self-report method." Journal of Research in Crime and Delinquency 14:247–61.

Hindelang, Michael J.
1978 "Race and involvement in common law personal crimes." American Sociological Review 43:93–109.

Hindelang, Michael J., Travis Hirschi, and Joseph G. Weis
1975 Self-Reported Delinquency: Methods and Substance. Proposal Submitted to the National Institute of Mental Health, Department of Health, Education, and Welfare, MH27778.
1978 Social Class, Sex, Race and the Discrepancy between Self-Reported and Official Delinquency. Grant Number MH27778-03, Center for Studies of Crime and Delinquency, National Institute of Mental Health, Department of Health, Education, and Welfare.

Hirschi, Travis
1969 Causes of Delinquency. Berkeley: University of California.

Hollingshead, August B. and Frederick C. Redlich
1958 Social Class and Mental Illness. New York: Wiley.

Huizinga, David
1978 Description of the National Youth Sample. Project Report Number 2. HEW Grant Number MH27552, The Dynamics of Delinquent Behavior: A National Survey. Boulder: Behavioral Research Institute.

Illinois Institute for Juvenile Research
1973 Juvenile Delinquency in Illinois. Chicago: Illinois Department of Mental Health.

Krohn, Marvin, Gordon P. Waldo, and Theodore G. Chiricos
1974 "Self-reported delinquency: a comparison of structured interviews and self-administered checklists." Journal of Criminal Law and Criminology 65:545–53.

Kulik, James A., Kenneth B. Stein, and Theodore R. Sarbin
1968 "Disclosure of delinquent behavior under conditions of anonymity and nonanonymity." Journal of Consulting and Clinical Psychology 32:506–9.

Miller, Walter B.
1958 "Lower class culture as a generating milieu of gang delinquency." Journal of Social Issues 14:5–19.

Miller, Walter B.
1966 "Violent crimes in city gangs." Annals of the American Academy of Political and Social Science 364:97–112.

Nettler, Gwynn
1974 Explaining Crime. New York: McGraw-Hill.

Nye, F. Ivan and James F. Short, Jr.
1956 "Scaling delinquent behavior." American Sociological Review 22:326–31.

Quinney, Richard
1970 The Social Reality of Crime. Boston: Little, Brown.

Short, James F., Jr., and F. Ivan Nye
1957–1958 "Reported behavior as a criterion of deviant behavior." Social Problems 5:207–13.

Short, James F., Jr., and Fred L. Strodtbeck
1965 Group Process and Gang Delinquency. Chicago: University of Chicago.

Taylor, Ian, Paul Walton, and Jock Young
1974 The New Criminology. New York: Harper and Row.

Tittle, Charles R. and Wayne J. Villemez
1977 "Social class and criminality." Social Forces 56:474–502.

Tittle, Charles R., Wayne J. Villemez, and Douglas A. Smith
1978 "The myth of social class and criminality." American Sociological Review 43:643–56.

Turk, Austin
1969 Criminality and the Legal Order. Chicago: Rand McNally.

U.S. Bureau of the Census
1977 Population Estimates and Projections. Series P-25, No. 643.

West, D. J.
1973 Who Becomes Delinquent? London: Heinemann.

Wolfgang, Marvin, Robert Figlio, and Thorsten Sellin
1972 Delinquency in a Birth Cohort. Chicago: University of Chicago.

Williams, Jay R. and Martin Gold
1972 "From delinquent behavior to official delinquency." Social Problems 20:209–29.

Yablonsky, Lewis
1962 The Violent Gang. New York: MacMillan.

RESEARCH NOTES

THE GEOGRAPHY OF SOVIET CRIMINALITY*

LOUISE SHELLEY
American University

American Sociological Review 1980, Vol. 45 (February):111–122

A distinct geographical distribution of criminality exists in the Soviet Union as a result of the measures of social control exercised over the mobility and residence of the population, and that differentiates Soviet crime from that of both developing and developed countries. In the USSR increasing urbanization is not correlated with higher rates of criminality. As a result of the internal passport system and the difficulties of moving to large cities, the rates of criminality in the Soviet Union have shifted from the most populous cities of the USSR to the newly established cities of the far eastern and far northern parts of the USSR and to the rapidly expanding smaller urban centers.

INTRODUCTION

This paper attempts to analyze the geographical distribution of Soviet criminality which results from the measures of social control exercised over the mobility and residence of the population and which differentiates Soviet crimes from that of both developing and developed countries. This paper does not attempt a general overview of the problems of Soviet criminality and for that purpose the author recommends *Deviance in Soviet Society* (Connor, 1972).

This analysis is based on a year and a half's study in the Soviet Union, extensive reading of both published and classified Soviet criminological scholarship, and conversation with numerous scholars in the USSR. The conclusions reached in this article regarding the effect of population controls on crime are the author's own. Soviet scholars are incapable of reaching such conclusions because of a combination of ideological and intellectual constraints. This dual limitation exists because comments on internal population controls by Soviet scholars would be interpreted as criticism of the regime. Because Soviet scholars lack knowledge of the crime patterns of other societies or of the uniqueness of their own demographic controls—knowledge that would enable them to evaluate the effect of these controls on the criminality of their society—they find it impossible to draw comparable conclusions. Although based entirely on data collected and analyzed by Soviet scholars, this article synthesizes their findings to provide conclusions not contained in their research.

Contemporary Soviet Crime Research

It is necessary to understand the nature and training of the Soviet criminological community before their data and research efforts are analyzed and interpreted. Criminological research in the USSR is conducted by a large group of scholars studying under many different auspices and under complex ideological constraints. The large community of criminal law scholars who analyze the dynamics of Soviet criminality receive supplementary education in social science methods prior to their initiation of criminological research (Solomon, 1978:33–41). They conduct research in order to provide information to decision-makers in government for the formulation of criminal and general social policy (Solomon, 1978:41–50).

---

* Direct all communications to: Louise Shelley; 201 Cassell; School of Justice; American University; Washington, D.C. 20016.

The research for this paper was done while studying in the Soviet Union on IREX and Fulbright-Hays Fellowships.

A travel grant was provided by the American Sociological Association so that this paper could be presented at the World Congress of Sociology at Uppsala, Sweden, in 1978.

After a hiatus of twenty-five years of scholarship the small core of Soviet criminologists that reemerged to resume criminological research after Stalin's death has succeeded in training a new generation of researchers in problems of criminality (Solomon, 1974:132–5). While crime research first reemerged in the mid-1950s in Moscow, it has spread not only to the central and far eastern parts of the Russian republic but also to the Baltic republics and Ukraine as well. Criminology is taught in law schools throughout the country and an increasingly sophisticated group of researchers has emerged in the past decade.

Though Soviet criminologists are accorded considerable respect and influence in the USSR, crime researchers are still plagued by the problem of confidentiality of data. While older and established Soviet criminologists are allowed direct access to the data in the procurator's office (the procurator is responsible for the proper execution of the law from the stage of police investigation through the review of the sentence) (Hazard et al., 1969:64), and in police files, and are themselves permitted to collect their own research data at labor camps and other sites, younger scholars do not have the same degree of access to crime data or to the highly informative classified criminological research documents.

The older, respected criminological scholars (almost all Communist party members) are given ample access to primary research data, and their findings are used by policy-makers to a degree unknown in the United States (Solomon, 1978:107). Most of these influential criminologists are not university-based but are instead employed by research institutes affiliated with the Academy of Sciences, the Procuracy and the Ministry of Internal Affairs (MVD), or the police.

The preeminent criminological research institute in the Soviet Union for years was the All-Union Institute for the Prevention of Crime and the Elaboration of Preventative Measures under the auspices of the USSR Procuracy and the Supreme Court (Solomon, 1978:57). Many of the articles cited in this paper are based on research conducted during its prime. This institute has recently come under increasingly strict ideological control and consequently does not presently have the intellectual freedom to conduct much objective, analytical research. Its former place in criminological research has been increasingly assumed by the scholars at the MVD or at the police institute[1] who are provided significant financial incentives in order to produce necessary classified research. To do this, they are given unrestricted research access and intellectual freedom to make intelligent recommendations as the basis for policy planning.

The research of both the university and institute-based criminologists in the Soviet Union may not have the quantitative sophistication of that produced by their Western colleagues but their conclusions seem to be soundly based on the data available to them. Their analysis could, however, extend further than it does if researchers were able to apply more sophisticated quantitative techniques or had greater access to an international body of criminological scholarship. Because of obstacles to research, Soviet criminologists have developed the unusual ability (seemingly characteristic of social science researchers operating in closed societies) to synthesize information available to them through official data collecting channels with that which they obtain through their informal contacts with the police and courts. Their research conclusions are subsequently more authoritative than might be expected of scholars working in such an incapsulated and highly secretive research environment.

Crime Data Used

Soviet crime data are not published, but police and court statistics are made available to trusted researchers by the central offices of the police and the Procuracy. Available data share some of the same problems of comparable American data. Crime statistics collection is not centralized (Juviler, 1976:133) but provided by different local authorities that have a vested interest in proving their effectiveness in controlling crime to the Party bureaucracy. Therefore, Soviet police reduce the number of reported crimes to demonstrate their success in solving criminal cases.

---

[1] Private conversations with Soviet scholars.

The authors cited in this article not only had access to Soviet crime data, and confidential crime studies, but also had the ability to collect their own data. They use the data as indicators of national crime trends rather than as definitive statements of absolute crime rates. Authors in the Soviet publications mentioned base their analyses on national and regional crime figures. Many of the individual researchers base their scholarly conclusions not only on available crime data but also on studies of populations of both prisons and labor camps, and studies of released convicts, thereby providing increased verification for their conclusions.

Crime data in the USSR do not have the reliability of statistics of smaller homogeneous societies that do not share the Soviet ideological commitment to the elimination of criminality. Yet, Soviet scholars are aware of the limitations of these data and use them with caution and conduct corroborating studies whenever feasible. The data referred to in their articles are based on some of the finest of contemporary Soviet crime research. Almost all the work cited here is unclassified and is available in Soviet libraries.

The following analysis will focus on the laws and policies that affect population residence and the subsequent effect they have had on the geographical distribution of Soviet crime. Geographical distribution will be examined in terms of rural and urban differences, and the impact of migration on crime.

Laws and Policies Affecting Residence in the Soviet Union

Passport regulations and population policies govern the lives of all Soviet citizens. These laws and policies determine the stability and size of the population in different regions of the country as well as the internal mobility of all citizens of the USSR. By controlling travel and residence within the country, Soviet authorities are able incidentally to influence the distribution of criminality within the country. The complex regulations governing the mobility of the Soviet population have resulted in a geography of crime that is different from that of other countries of similar economic and social development.

The residence of Soviet citizens is determined by a complex combination of birthplace, the internal passport system, marriage, and employment opportunities. There is no easy mobility between different settlements and cities in the Soviet Union. A Soviet citizen may spend his entire life in the same community if marriage or a job do not permit him to change his place of residence.

The internal passport carried at all times by Soviet citizens has been in existence since 1932. Before that time, Soviet citizens could generally reside in a city or town of their choice and could travel freely internally without documents. The internal passport received now by all Soviet citizens at age sixteen, with the exception of collective farm workers who are recently eligible (Matthews, 1972:53–4), indicates the individual's street address and town or village of residence, the only place where the holder of the passport may legally reside.

The internal passport controls mobility as well as residence; it is required to purchase plane tickets, long distance train tickets, and a room in a hotel. Individuals without passports are restricted exclusively to local travel. Therefore, the necessity of constantly showing a passport impedes the travel of wanted criminals.

> The passport is issued by the local police and is updated periodically. If the holder wishes to change his place of residence permanently he must get the formal permission of the militia in his chosen area before he moves in. This is only a formality in small towns and the less desirable spots where there is little pressure on public amenities, but may present a major difficulty in places like Moscow, Leningrad and republican capitals, which have more to offer their inhabitants. It is no secret that outsiders are all but excluded from permanent residence in Mos-

> cow, for example, some people find that registration there can be procured only by bribery. (Matthews, 1972:54)

As a result of the difficulties of migrating to a major city, the large number of internal migrants settle in the small and medium-sized towns in geographically remote areas where they can easily obtain residence permission from the police (Matthews, 1972:53–4). Permission to live in the major cities is secured only through employment there or marriage to a resident. High party and cultural positions and jobs in the police and secret police are among the categories of employment that carry with them permission to live in a major urban center such as Moscow, Leningrad, or Kiev.

Mobility for collective workers who do not possess internal passports is much more complex. They need to obtain a pass from the local village council to go anywhere for more than a month (Matthews, 1972:54). Permanent withdrawal from a collective farm and transfer to an urban setting is even more difficult. Many young male peasants who choose to leave collective farms never return to the village thereafter and enter their army service directly after high school. After army service, they move to construction sites in the far east and far north where they receive internal passports. With this document they can register permanently in a town or medium-sized city.

The low priority given to agriculture by Soviet authorities, combined with limited access to major urban settlements, causes the large number of migrants to settle in smaller cities with less desirable locations. The influx of many rural migrants with little training for urban life results in severe disorientation and high rates of criminality. The Soviet passport system has both negative and positive effects on the crime rates. While the settlement policies result in high rates of criminality in certain areas, the restrictions on internal mobility increase the certainty of detection.

The internal mobility and possible residences of former criminals are even more limited than those of ordinary citizens. All previous residences are recorded on the internal passport, including time spent in labor camp or prison (Lipson and Chalidze, 1977:175). Criminals from major urban centers, such as Moscow, who have served sentences of at least five years automatically lose the right to register at their former residence upon their return from labor camp or prison, a policy in effect since the 1920s (Gernet, 1927:160). Exceptions to this rule can be granted by the local police but these are rarely provided, and the police sometimes refuse to register those who have served sentences of less than five years.

The offender convicted of a major offense is severely limited in his choice of residence upon termination of his sentence. Individuals who are especially dangerous recidivists (convicted at least four times) and individuals convicted of armed robbery, serious assaults against the person, group rape, and premeditated murder as well as many crimes against the state and economic order "are not to be granted residence permits in cities, districts and regions listed in decisions of the Soviet government until the cancellation or annulment through established procedure of their record of conviction" (Lipson and Chalidze, 1977:185). Individuals not permitted residence in these areas cannot be "employed by enterprises, institutions and organizations located in those areas" (Lipson and Chalidze, 1977:186). Minors, serious invalids, pensioners, and women with young children are exempted from these orders. Exceptions can be made to these restrictions only with the "permission of Ministries of Internal Affairs of union and autonomous republics, Boards of Internal Affairs of krai ("territory") and oblast' ("region") Executive Committees of Soviets of Workers' Deputies" (Lipson and Chalidze, 1977:186).

Serious offenders and recidivists are thus denied the right to return to major cities or reside in the surrounding communities. As a result, the major urban centers house few experienced criminals and cannot serve as schools for youthful offenders. Medium-sized cities, on the other hand, are recipients of a disproportionate number of first-time serious offenders and recidivists.

The internal passport policies distort the usual distribution of criminality in an industrialized country. Major cities have comparatively low rates of criminality while smaller cities in undesirable locations contain a disproportionate number of past and current offenders. Soviet population policies and the internal passport system have shifted the distribution of Soviet crime away from the highly populated western cities of the RSFSR (Russian Soviet Federated Socialist Republic, the largest of the fifteen Soviet republics) to the more urbanized Baltic republics and to the far eastern and far northern administrative districts, primarily within the RSFSR.

RURAL CRIME

The rural farm areas of the Soviet Union have the lowest level of criminality in the country. This is explained by the population structure of collective and state farms, and the low economic priority given Soviet agriculture by Soviet policy makers, as well as by the traditional patterns of rural life and the inaccessibility of the Soviet countryside.

Soviet agricultural workers are employed either on state or collective farms. On state farms, workers are paid a monthly wage for their labor and have internal passports that permit them to travel freely within the country. Workers on collective farms share in the farm's profits and have no internal passports, thus denying them the right to travel without specially obtained permission. As a result, collective farm workers earn less than state farm workers and have virtually no opportunity for internal travel within the USSR. Collective farms are the predominant form of agricultural enterprise in the Soviet Union.

The combination of low wages, hard work, and absence of internal passports makes life on collective farms very undesirable. The male population on Soviet farms, already reduced by great Soviet casualties during World War II, has been further decimated by the sizable exodus of Russian youth and other Slavic nationalities from rural areas. This internal migration will be discussed in greater depth later in this paper. The general absence of men, who are responsible for over 85% of Soviet criminality (Solopanov and Kvashis, 1971:6), from collective and state farms and the disproportionate representation of older women help explain the low rate of criminality in the Soviet countryside.

Traditional patterns of rural life also contribute to the low level of criminality. Crime has traditionally been lower in rural areas because of the tight social controls exerted by members of the rural community on each other and the general economic homogeneity of rural inhabitants. Informal social controls in rural life are still significant in preventing criminality, and rural inhabitants cooperate with the police in reporting crimes.

The inaccessibility of the Soviet countryside contributes significantly to the low rate of rural criminality by deterring nonnative criminals from wandering into rural areas. As the few privately owned cars are possessed almost entirely by the Soviet elite, the minimal public transportation in rural areas and the poor condition of rural roads minimize access to the countryside and curtail the possibility of preparation of crimes by nonresidents. The limited possessions of rural residents and the difficulties in reaching and escaping from rural areas compound the improbability that urban dwellers will commit crimes in the countryside.

Theft of government and social property, violent crimes and *samogon* ("the production and sale of home-brew") have traditionally been the characteristic forms of criminality in the rural parts of the Soviet Union (Lichnost' Prestupnika, 1971:96–7). Theft of government and social property remains the predominant form of rural theft. Rural inhabitants continue theft of government and state property to the degree that they did in the 1930s. Rural residents 40 years ago comprised two-thirds of the Soviet population and were responsible for 62% of the crimes committed in this category, while 30 years later the 45% of the Soviet population residing in rural areas committed 42% of the offenses in this category (Lichnost' Prestupnika, 1971:86; Naradnoe Khoziaistvo, 1969:7).

Violent crime in the last century was, disproportionately, a rural phenomenon. By the 1920s, urban dwellers were responsible for more violent crime than their proportional representation in the population. With the continued exodus of young males from rural areas, there has been a further reduction in the level of rural violence. While violent crime is still more common in rural areas than crimes against personal property (Lichnost' Prestupnika, 1971:114), the traditional rural propensity toward violent crimes is not as marked as a century ago.

Bootlegging is the only traditionally rural crime that continues on a mass scale. This offense, traditionally the domain of older women, has been the most common female offense since the pre-Revolutionary period (Gernet, 1924:185–6). Women with limited financial resources resulting from low monthly salaries or even lower retirement pensions (sometimes as little as $25 a month) turn to this form of criminality as an easy means of increasing their incomes.

Soviet agricultural and manpower policies have caused a dramatic decline in the level of criminality in the countryside and an increase in crime in areas of recent settlement by formerly rural inhabitants. Traditional forms of criminality are no longer characteristic of the rural environment though traditional methods of peasant social control still prevail.

MIGRATION AND CRIME

The relationship of migration and criminality is currently a subject of great scholarly interest in the Soviet Union. The annual migration of 4–6% of the population, approximately 8,000,000 people, represents a cumulative problem of significant proportions (Babaev, 1974:9). The severe adjustment problems of migrants that often result in criminality have become a subject of concern to Soviet policymakers and researchers.

The rate of urban growth of medium-sized cities in the past two decades has been sizable. Between 1959 and 1970, the number of cities with populations of over 100,000 increased by 73 (Sokolov, 1973:150). The migration that has occurred has resulted primarily from the migration of young males aged 16–29 into urban areas. The movement of these young people has been of such proportions that it has measurably reduced the average age of the urban population (Sokolov, 1973:153). Soviet criminologists have closely monitored the effect of this most potentially active criminal group on recorded rates of criminality in urban areas. They have concluded, based on detailed sophisticated studies, that there is a strong correlation between the increase in internal migration and a recorded increase in urban crime, They anticipate that with further growth in cities there will be a still further rise in recorded crime rates (Sokolov, 1973:152).

Soviet researchers studying crime in the RSFSR have learned that a region with a high coefficient of migrants is characterized by a higher coefficient of crime (Babaev, 1968:86). This observation reflects the problem that migrants, primarily individuals of rural background, find difficulty in making the adjustment to an urbanized or culturally different environment far removed from their family and their rural traditions. These recent migrants face the basic problem of acclimatizing themselves to the absence of controls in their impersonal new place of residence while all their lives they have been accustomed to the strict social controls of their traditional rural communities.

There are two fundamental types of internal migration in the Soviet Union. Rural migrants move either to heavily populated regions or to lightly populated areas that Soviet authorities believe would benefit from an increase in population (Babaev, 1968:86–7). Each of these types of moves involves its own problems of adjustment and its own potential for criminality.

Movement to an already inhabited region requires migrants to adjust to already established cultural and social patterns. After their arrival in Soviet cities, individuals from rural areas, unacquainted with urban ways, have difficulty in locating housing and proper employment (Babaev, 1968:87). During the strenuous period of acclimation, migrants frequently face difficult situations that sometimes resolve themselves in acts of criminality.

Migration to a previously lightly inhabited region brings with it a different array of problems. Rural Russian migrants are frequently assigned residences in other republics or in different areas of the Russian republic with non-Slavic inhabitants of totally different cultural, linguistic and ethnic backgrounds (Babaev, 1968:87). These migrants, who have never previously been exposed to individuals of different cultures, suffer many of the consequences of culture conflict described by Thorsten Sellin (1938:63–70). The conflicting norms of the Russian migrants and the native inhabitants frequently result in violent conflicts and other criminal acts. While Soviet authorities comment that Russian influx into traditionally Moslem areas sometimes has the beneficial effect of reducing criminal vestiges of the past (for example, Soviet law outlaws traditional Moslem marriage and social practices), more often this severe dislocation has criminological consequences.

Soviet researchers have attempted to understand the effects of these two kinds of migration by analyzing population and crime trends in areas recently settled by migrants. The work conducted at the All-Union Institute for the Study of Crime and the Elaboration of Preventive Measures, under the auspices of the Soviet Procuracy, has been in progress for 15 years.

Two studies conducted by the Institute, the nation's leading center for criminological research, in the 1960s examined the relationship between population migration into different areas and the level of criminality. Data was collected on the level of population influx and crime rates for all the oblasts (major territorial subdivision directly below republic size) of the RSFSR and the autonomous republics of RSFSR, regions inhabited originally largely by non-Russian nationalities.

In 1968 seven of the eight oblasts with the greatest number of migrants (Tiumen, Magadan, Kamchatka, Yakutsk, Tuvin, Komi, Krasnoyarsk and Primorskii [coastal]) the rate of criminality was highest for the republic (Babaev, 1968:88). In the 20 oblasts with the least influx of population, 13 of the 20 regions were in the lower half of all RSFSR oblasts in their rates of criminality. The same relationship between patterns of criminality and migration were observed in an earlier study conducted between 1963 and 1965. All oblasts which had an observed decline in population had also experienced a reduction in the crime rate. This indicates that net emigration from a particular oblast' resulted in a lower rate of criminality, but an increase in population resulting from migration into a particular area was correlated with a concomitant increase in criminality (Babaev, 1968:88–9).

Babaev, previously a leading researcher of the Procuracy Institute on the subject of migration and crime, concludes from these results: "It is impossible to state that there are sufficient examples where the number of residents declined but crime was not reduced" (1968:88). Soviet statistics lead to the conclusion that the rate of crime per capita is reduced only when the population declines and there is no significant population migration into the area.

Migrants might arrive in a particular community either independently or under organizational sponsorship. The form in which they arrive and the size of the local population are significant in determining the effect of their migration on local crime rates. When large numbers of migrants arrive in one administrative region at the same time, they contribute a disproportionately large percentage of the total crime rate. However, when small groups arrive in a community their rates of criminality do not differ markedly from the rest of the population (Babaev, 1968:89). In 1966, in Novoselorskii and Birilusski regions, where large groups of migrants had arrived in large numbers, yet represented only a small proportion of the total population, they nevertheless contributed 33% and 30% of all criminals, respectively.

The social structure of a particular region is also important in explaining the impact of migrants on the local crime rate. The crime situation of Sakhalin oblast' and Yakutsk ASSR is used by Soviet authorities to show the impact of community composition and social services on the dynamics of criminality. Yakutsk, with nearly 50% of its population residing in rural areas, was ill-prepared for the influx of large numbers of recent migrants. This is demonstrated by the fact that, in the years 1963–66, urbanized Sakhalin with 13% more population than Yakutsk (SSSR Administrativno-territorial'noe delenie, 1963:66) had an approximately equal number of known offenses (Babaev, 1968:89). Prior urbanization and community preparedness help reduce some of the negative consequences of intense population migration to a particular area.

More recent research has been done by Babaev on the social surroundings and personalities of criminals who were migrants or who were permanent residents. The study was based on 1,000 inmates in labor camps housing criminals of differing degrees of severity of criminal acts. Of the subjects in the study, 15.3% were migrants—had not lived in their previous location for more than one year—and the remaining 84.7% were permanent residents (Babaev and Antonian, 1975:5). For research purposes the migrants were divided into those who had been residents for more than six months and those who had lived at their last location for less than six months.

In almost all categories, significant differences were found between those who were permanent residents and those who were migrants. Forty-two percent of the permanent residents were married while 75% of the migrants were single (Babaev and Antonian, 1975:7). Thirty-eight percent of the new settlers lived with their parents while 57% of the permanent residents who were in labor camps had resided with their parents. Eight percent of the new settlers lived with their spouses while 30% more of the permanent residents lived with their families. The researchers concluded that the separation of the migrants from parents and family had a negative effect on rates of criminality (Babaev and Antonian, 1975:13).

The migrants had considerably worse housing conditions than the permanent residents. Thirty-eight percent of the migrants lived in their own apartment or house while an additional 35% of the permanent residents enjoyed such living conditions. Thirty-two percent of the migrants of at least six-month longevity and 30% of the most recent arrivals resided in dormitories at the time they committed their offense (Babaev and Antonian, 1975:12). It is evident that the inferior quality of residential accommodations has a direct effect on the increased tendency to commit crime.

The criminologists, like the demographers, found that the migrants had a higher educational level than the permanent population. Only 4% of the most recent migrants had only a fourth-grade education while the corresponding figures for the less recent arrivals and permanent residents was 12% and 10%, respectively. The same relationship held true at all other educational levels as well. Sixty-eight percent of the most recent migrants had finished 5–9 years of school (there are 10 grades in the Soviet educational system) while the comparable figure for the permanent residents was 63% and for the recent settlers 58% (Zaionchovskaya, 1972:105–8). At the high school and college level, migrants and recent settlers again outstripped the resident population. Thirty percent of the first group, as opposed to 26% of the second level, completed high school or had received some college education (Babaev and Antonian, 1975:6–7). The fact that the migrants had such educational advantages and still lapsed into criminality indicates that the better jobs and higher wages available to the migrants as a result of their superior training did not act as a sufficient deterrent to their criminality. The problems of the migrants' adjustment were of such magnitude as to outweigh the financial and employment advantages of their new environment.

The migrants also had distinguished themselves by the high percentage that had previous convictions. Eighty-two percent of the migrants surveyed in labor camps had previous convictions while comparable figures for the most recent arrivals and the permanent residents were 76% and 59%, respectively (Babaev and Antonian, 1975:8). These patterns of previous criminality were further supported by the association patterns of the migrants. Both categories of migrants were much more likely to associate regularly with individuals with prior convictions. These associations were developed at the migrants' place of residence (Babaev and Antonian, 1975:16–17).

The results indicate that despite the initial training and educational advantage possessed by the migrants, their living conditions, removal from their families and patterns of association all contributed to their disproportionately high rate of criminality. The number of migrants arriving in a community at a particular time and the extent to which they are absorbed into their new place of residence are reflected in their rates of criminality.

While the increased criminality of rural migrants in a new unfamiliar urban environment is an internationally known problem, the Soviet case differs significantly from that of other societies. The rural emigrants eager to leave the collective and state farms are denied access to the largest cities in the Soviet Union. Therefore, these young men settle either in medium-sized cities or relatively unsettled culturally different regions. In both cases, these young, frequently unattached men in the age most likely to commit offenses face serious adjustment problems that frequently result in the commission of crimes.

The migrants commit a significant proportion of the crime recorded in medium-sized cities. As a result, unlike the other industrialized and industrializing countries, the Soviet Union is experiencing its most severe growth in criminality not in the large but in the medium-sized cities of the USSR. Soviet residential policies have resulted in a geographical distribution of crime in the USSR that is significantly different from that of other countries of similar economic and social development.

URBAN CRIME

International statistics demonstrate that the greater the degree of urbanization of a country, the higher the level of criminality. The Soviet Union also conforms to the internationally established correlation between the level of urbanization and the recorded crime rates. In the years 1963–66, the ten republics with the highest levels of crime are regions with the highest levels of urbanization. Estonia and Latvia, both republics with high rates of criminality, are 65% and 62% urbanized, respectively (Sokolov, 1973:51).

The intensive influx into urban areas that began during the Krushchev period continues to the present. As was previously mentioned, most of the migration is into newly built cities (Babaev, 1972:98) because entry into the larger, more established cities is much more difficult for all potential migrants. During this period Voronezh and Tula, medium-sized Russian cities, grew by 73% and 78%, respectively, and the capital of Estonia grew by 86%, while the larger capital of the Ukraine, Kiev, experienced only a growth of 60% during these years (Babaev, 1968:89). Between 1959 and 1970 there was an overall growth in the urban population of 36 million (Babaev, 1972:100). As the urban population in the Soviet Union rose to 136 million in 1970, during the same eleven-year period the number of rural and small-town residents declined markedly. The number of cities with populations larger than 50,000 rose from 304 to 409 (Babaev, 1972:100).

In almost all cases this population influx had a negative effect on recorded rates of criminality. Recorded increases in crime as a result of increasing population were observed not only in the western parts of the RSFSR but in far eastern RSFSR, in the far east, eastern and western Siberia, in the Baltic republics of Estonia and Latvia, and in the Slavic republic of Byelorussia. In one urbanized region of Byelorussia where the population increased by two-thirds in ten years, the crime rate grew at a rate greater than that of the population. The criminal justice system did not keep pace with the growth in crime; while the recorded number of offenses in the Lidsko region increased significantly, the number of people brought to trial did not rise proportionately (Kondrashkov, 1966:38).

The highest recorded rates of criminality in the Soviet Union are not found in the largest cities, as in the United States, but are found in the medium-sized cities. Kiev and Leningrad, cities with low rates of population growth, have lower crime rates than Tobolsk which has grown rapidly in recent years (Babaev, 1972:98). The stability of a city's population and not its size appears to be the more significant factor in determining the rate of criminality in the USSR. Vladivostok, the city with the highest recorded crime rates in the Soviet Union,[2] though a large city, is a port with a highly transient population—a fact which appears to account for the high rate of street crime.

While crime in western Europe and the United States is greatest in the largest cities and less in medium-sized cities, this relationship does not hold true in the Soviet Union. The largest cities of the Soviet Union—Moscow, Leningrad and Kiev—have much lower recorded conviction rates than the national average, and lower rates of criminality than the more newly formed cities with smaller populations (see Table 1) (Kuznetsova, 1971; Ostroumov, 1970; Babaev, 1972; Sokolov, 1973). The high crime rates in these medium-sized cities can be attributed to the mass of migrants unaccustomed to urban life.

Table 1. Conviction Rates for American and Soviet Cities and the USSR

	(Rate per 100,000) Conviction Rate
1) Soviet cities over 1.5 million[a] (Moscow, Leningrad, Kiev)	442
2) Cities with population under 0.5 million[b] (Tobolsk, Tartu, Vladivostok, secondary cities of Latvia)	500
3) American Cities[c] (2,925 cities with total population of 39 million)	2573
4) USSR (National average, 1976)[d]	1045

Sources: [a] Derived from selected crime statistics (Kuznetsova, 1971). The total conviction rate is based on an extrapolation of these statistics, using the overall distribution of crime, by category of offense (Ostroumov, 1970:248). This also includes information in Babaev (1972).

[b] Derived from information in Sokolov (1973) and from private sources with access to Soviet criminal statistics.

[c] Derived from Table 5.13, Disposition of persons formally charged by the police, 1975 (U.S. Department of Justice, 1977:541).

[d] Derived from Neznansky (1979:47).

Soviet researchers have not only examined overall trends towards urbanization but have studied the dynamics of criminality within the urban environment. Soviet research findings conform with those of Polish researchers who discovered a concentration of crime in certain regions of a city. Such research was replicated in the Russian city of Orel in 1971.

The researchers divided the city into three regions, but in the report of their findings they did not explain how these subdivisions of Orel were selected. The three regions were analyzed in terms of their indices of crime, recidivism, dangerous criminality and the retention rate of individuals on charges of petty hooliganism. One of the three regions scored highest in terms of all indicators while the third region had the lowest index in three of the four categories analyzed. The offenders did not often commit their crimes near their places of residence but, rather, in different regions of the city (Mogilevskii and Suslov, 1973:114).

Similar statistics have been collected for Moscow as well as for other Soviet cities and reveal the same localization of crime in certain regions of each city. In Moscow, recent studies have shown that crime is concentrated in the older sections of the city that have traditionally had high rates of criminality and in the new regions of the city that house workers who have only recently emigrated from the countryside. Many cities have established such internal maps of the distribution of criminality but they are not always based on methodologically sound criminological research (Mogilevskii and Suslov, 1973:114).

These studies of the distribution of criminality within the urban environment are performed to understand the social forces that influence crime and to correlate the level of criminality with other forms of deviant behavior, such as alcoholism and suicide. By determining the regions of greatest social instability, Soviet scholars hope to localize the urban regions in need of social and community help (Mogilevskii and Suslov, 1973:115).

---

[2] Soviet dissertation defense at the Criminal Law Department, Law Faculty, Moscow State University, Moscow, Spring, 1975.

Suburbia, consisting of prosperous residences surrounding major urban centers as they exist in the United States and much of western and eastern Europe, are unknown in the Soviet Union. The residential areas surrounding Soviet cities are inhabited by young workers and their families who could not obtain residential permits to live in the major cities. There is a high rate of criminality among young workers who commute lengthy hours to their schools and jobs in the city. Criminologists have called for increased social control of the mobility of these youth in the outlying urban areas.

An interesting study of these youthful commuters was conducted over a decade ago in Leningrad. Through their statistical studies, researchers confirmed their hypothesis that the percentage of persons aged 15–18 committing crime was higher among those who commuted to work and study in Leningrad than those who resided near their schools and places of residence. This difference in levels of criminality of the two groups was even higher than anticipated (Derviz, 1971:65).

The crime committed by these youthful offenders was not associated with their places of residence or work but was a result of their daily four-hour commutation that removed them from parental control and provided time for gambling and drinking. Study during their hours of commutation was impossible because of the noise, poor light, and poor ventilation on the commuter trains (Derviz, 1971:66). Researchers noted a much earlier age of entry into sexual relations, a much higher rate of alcohol use, and a much larger influence of adult criminals (Derviz, 1971:68) on these individuals than on youths residing within the city limits.

During their long hours of transit, the youthful commuters made the acquaintance of drunks, parasites, and recidivists. These undesirable individuals had a strong influence on the young potential criminals as reflected in the fact that 25–30% of these minor-age suburban Leningraders committed their crimes under the direction or provocation of adults (Derviz, 1971:67). The crime problem of these youthful offenders was so great that they were responsible for much of the crime committed by minors within the city of Leningrad (Derviz, 1971:70).

This initial study of the crime of youthful commuters in Leningrad has led to further research in this area. The problem observed in Leningrad is not local but is a growing problem in the Soviet Union as more individuals (because of the passport system and the limitation on growth of cities) settle in the outlying urban areas and are forced to commute long distances to work and school.

CONCLUSION

The passport system and the difficulties of moving to large cities have affected the Soviet crime problem in two ways. First, they have shifted the greatest degree of criminality away from the big cities of the Soviet Union to the newly established cities of the far east and the far north and to the rapidly expanding smaller urban centers. Second, the communities surrounding major cities are not the privileged retreat of the prosperous from urban problems but are the homes of young men who are forced to commute long hours, under unfavorable conditions, to their schools and jobs. However, their crimes are not committed in the suburbs, but in the urban communities to which they commute. The population controls and internal passport system in the USSR have resulted in a distribution of crime which is fundamentally different from that found in most industrialized countries.

This analysis of the geography of crime in the Soviet Union illustrates one of Durkheim's principles: "There is no occasion for self–congratulation when the crime rate drops noticeably below the average level, for we may be certain that this apparent progress is associated with some social disorder" (Durkheim, 1964:72). A reduction in the crime rate of major Soviet cities has been achieved only at the social cost of reducing personal freedom of movement and causing increased problems of criminality in more remote parts of the country.

REFERENCES

Babaev, M.M.
1968 "Kriminologicheskie isseledovaniia problem migratsii naselenia." Sovetskoe gosudarstvo i pravo 3:86–90.
1972 "Kriminologicheskaia otsenka sotsial'no-ekonomicheskikh i demograficheskikh faktorov." Sovetskoe gosudarstvo i pravo 6:97–102.
1974 "Demograficheskie protsessy i problemy territorial'nykh razlichii prestupnosti." Voprosy bor'by s prestupnost'iu 21:3–15.

Babaev, M. M. and Iu. M. Antonian
1975 "Sotsial'naia sreda i lichnost' prestupnikov-migrantov i postoiannykh zhitelei." Voprosy bor'by s prestupnost'iu 22:3–10.

Connor, Walter
1972 Deviance in Soviet Society. New York: Columbia University Press.

Derviz, O. V.
1971 "Rabota ili ucheba vne mesta postoiannogo zhitel'stva—odin iz faktorov prestupnosti nesovershennoletnikh." Pp. 64–70 in N. P. Kan and M. D. Shargorodskii (eds.), Prestupnost' i ee preduprezhdenie. Leningrad: Izd. Leningradskogo Universiteta.

Durkheim, Emile
1964 The Rules of Sociological Method. New York: Free Press.

Gernet, M. N.
1924 Prestupnyi mir Moskvy. Moscow: Pravo i Zhisn'.
1927 Prestupnost' i samoubiistvo vo vremia voiny i posle nee. Moscow: Tsentral'noe Statisticheskoe Upravlenie SSSR.

Hazard, J. N., Shapiro, I, and P. B. Maggs
1969 The Soviet Legal System. New York: Oceana.

Juviler, Peter
1976 Revolutionary Law and Order. New York: Free Press.

Kondrashkov, N. N.
1966 "Analiz raionnoi statistiki prestupnosti." Voprosy preduprezh-deniia prestupnosti 3:35–46.

Kuznetsova, N. F. (ed.)
1971 Sravnitel'noe kriminologicheskoe issledovanie prestupnosti v Moskve v 1923 i 1968–1969 gg. Moscow: Izd. Moskovskogo Universiteta.

Lichnost' Prestupnika
1971 Moscow: Iuridicheskaia Literatura.

Lipson, Leon and Valery Chalidze (eds.)
1977 Papers on Soviet Law, 1:175, 185–6.

Matthews, Mervyn
1972 Class and Society in Soviet Russia. New York: Walker.

Mogilevskii, R. S. and Iu. A. Suslov
1973 "Gorodskaia prestupnost' kak ob'ekt kriminologicheskikh issledovanii." Pravovedenie 5:110–16.

Narodnoe Khoziaistvo SSSR v 1968.
1969 Moscow: Statistika.

Neznansky, Fridrich
1979 "Statistika prestupnosti v SSSR." Posev 5:46–52.

Ostroumov, S. S.
1970 Sovetskaia sudebnaia statistika. Moscow: Izd. Moskovskogo Universiteta.

Sellin, Thorsten
1938 Culture Conflict and Crime. New York: Social Science Research Council.

Sokolov, A. F.
1973 "Urbanizatsiia i nekotorye voprosy bor'by s prestupnost'iu." Sbornik uchenykh trudov [series]: Problemy sovetskogo ugolovnogo prava i kriminologiia 28:150–7. RSFSR: Sverdlovsk.

Solomon, Peter H.
1974 "Soviet Criminology: Its Demise and Rebirth." Soviet Union I, part 2:122–40.
1978 Soviet Criminologists and Criminal Policy. New York: Columbia University Press.

Solopanov, Iu. V. and V. E. Kvashis
1971 Retsidiv i retsidivisty. Moscow: Ministerstvo Vnutrennikh del SSSR.

SSSR Administrativno-territorial'noe delenie soiuz-
1963 nykh respublik. Moscow: Izvestiia Sovetov Deputatov Trudiashchikhsia SSSR.

U.S. Department of Justice
1977 Source book of Criminal Justice Statistics. Washington, D.C.: U.S. Government Printing Office.

Zaionchkovskaia, Zh. A.
1972 Novosely v gorodakh. Moscow: Statistika.

ANALYZING COLLAPSED CONTINGENCY TABLES WITHOUT ACTUALLY COLLAPSING*

PAUL D. ALLISON
Cornell University

American Sociological Review 1980, Vol. 45 (February):123–130

For a variety of reasons, log-linear models are sometimes fit to "collapsed" contingency tables which sum over the categories of one or more variables in the original table. I show here that the same results can be readily obtained by fitting a log-linear model to the full table, without actually collapsing it. All that is necessary is to exclude certain parameters pertaining to the collapsed variable. This approach is particularly useful for the analysis of recursive systems of categorical variables.

There are several situations in which it is desirable to take a contingency table with a given number of cells and "collapse" it into a smaller contingency table by summing over some or all of the categories of one or more variables. It may be that some of the categories are thought to be theoretically or practically equivalent. Or perhaps one of the variables is considered to be irrelevant to the analysis at hand. Most importantly, collapsing is an essential feature of Goodman's (1973) method for the log-linear analysis of recursive systems of categorical variables.

For example, suppose we have four categorical variables—A, B, C, and D—and we assume that A is causally prior to B, C, and D; B is causally prior to C and D; and C is causally prior to D. Goodman proposes that one first analyze the four-way table, with D taken as the dependent variable. Then one collapses over D to obtain a three-way table, with C taken as the dependent variable. Finally, one collapses over C to study the effect of A on B.

There are disagreements as to the appropriateness of this method. Reynolds (1977) is troubled by the fact that significant associations can appear in the collapsed table even when there are no such associations in the full table, and suggests that this may produce misleading results. However, Gillespie (1978) and Goodman (1979) have countered that in a recursive system it is the collapsed table that more accurately describes the true causal structure. Thus, when looking at the effects of A and B on C, it is misleading to further cross-classify on a subsequent dependent variable D.

Let me emphasize at the outset that I completely agree with Goodman and Gillespie. There is simply no way to represent a recursive causal system by a single log-linear model for the full multiway table. The method of successive collapsings, on the other hand, seems entirely appropriate for such recursive systems.

Nevertheless, collapsing can be tedious, especially when the original table is large. Even when one has a computer program to do the collapsing, it can be inconvenient to shift back and forth between different collapsed tables. I will show here that, while the method of collapsing is a correct way to analyze recursive systems, it is not strictly necessary. One can obtain identical results by fitting a succession of models to the full multiway table. Not only does this reduce computation and increase flexibility, but it also offers important insights into the model itself. Further, this method—while especially useful for recursive systems—also has important implications for other situations in which collapsing seems desirable. The method is based on formal results obtained by Goodman (1971) and by Bishop et al. (1975), though they did not develop these particular implications in their work. I

---

*Direct all communications to: Paul D. Allison; Department of Sociology; 323 Uris Hall; Cornell University; Ithaca, NY 14853.

For helpful comments, I am indebted to Roger Avery, Edward Hackett, and Reuben Snipper. An earlier version of this paper was presented at the annual meeting of the American Sociological Association, Boston, 1979.

Table 1. Attitudes toward Jobs for Blacks by Education, Region, and Year (Cell Frequencies)

		1946		1963	
		Attitude			
		Favorable	Unfavorable	Favorable	Unfavorable
South	Grade	52	206	71	44
	High	64	163	126	31
	College	42	51	55	14
Non-South	Grade	311	365	148	61
	High	439	374	410	56
	College	204	83	201	8

begin with an example but, further on, I will set down some general principles and indicate their mathematical justification.

The data to be analyzed consist of a four-way contingency table reported by Davis (1974), who adapted it from Schwartz (1967). The variables are year (1946, 1963), region (South, non-South), education (grade school, high school, college), and attitude toward jobs for blacks (favorable, unfavorable).[1] I will represent these variables by the letters A, B, C, and D, respectively, and I assume that they constitute a recursive causal system in which causal priority corresponds to alphabetical order. The table is reproduced here as Table 1.

Since the analysis of the four-way table, with D taken as dependent, is the same for both Goodman's method and the method proposed here, I do not report any of those results. Suppose, now, that we have already completed that analysis and we want to look at the effects of A and B on C. Goodman says to collapse the four-way table by summing over the categories of variable D to create a three-way table, ABC. Then, one can proceed to fit appropriate log-linear models to this collapsed table. Five models for the collapsed table are presented in Table 2A, along with their respective degrees of freedom and the likelihood ratio chi-square statistic (denoted by G^2).[2] The models are specified by the minimal set of marginal tables fitted to the data; but each of these marginal tables also corresponds to a set of parameters in the log-linear model. As is usual when one of the variables is taken to be dependent on the others, I have fitted the marginal table (AB) for the independent variables in each model. This ensures that the fitted model is equivalent to a logit model for the dependent variable. For further discussion of this point, see Fienberg (1977:79–81) or Gillespie (1978). A cursory examination of these models leads to the conclusion that model 2 is the simplest model that fits the data reasonably well. It says that A and B both have main effects on C, but there is no higher-order interaction.

Now, let us do it a different way. In Table 2B, I present results from fitting the same five models to the full four-way table. Since variable D is not included in any of the fitted marginal tables, this is equivalent to setting equal to zero all parameters pertaining to variable D in each of the fitted models. At first glance, the results seem to be markedly different. The chi-square statistics are all over 800, indicating that none of the five models fits the data well. However, a closer look reveals that *differences* in chi-square values for any two models are identical in Tables 2A and 2B, and that the same is true for differences in degrees of freedom. Moreover, since we know that the saturated model for the three-way table must have a chi-square of zero with zero degrees of freedom, we can use this as a baseline to recover the same chi-square values and degrees of freedom that we got in Table 2A. To accomplish this, we simply subtract the results for model 1 from the results for each of the succeeding models. This is shown in the last two columns of Table 2B.

---

[1] The last variable is the dichotomized response to the question, "Do you think Negroes should have as good a chance as white people to get any kind of job or do you think white people should have the first chance at any kind of job?"

[2] All models were fit by maximum-likelihood estimation using the program GLIM.

Table 2. Results from Fitting the Same Models to the Collapsed Table and to the Complete Table

A. Models Fitted to ABC Table

Fitted Marginals	G^2	d.f.
1. ABC	0	0
2. AB, AC, BC	2.4	2
3. AB, AC	21.4	4
4. AB, BC	72.9	4
5. AB, C	89.3	6

B. Models Fitted to ABCD Table

Fitted Marginals	G^2	d.f.	$G^2-863.0$	d.f.−12
1. ABC	863.0	12	0	0
2. AB, AC, BC	865.4	14	2.4	2
3. AB, AC	884.4	16	21.4	4
4. AB, BC	935.9	16	72.9	4
5. AB, C	952.3	18	89.3	6

These results demonstrate that we can easily fit models to collapsed tables without actually collapsing, and still get the same chi-squares. What about parameter estimates? These are also the same, even without any sort of adjustment. Table 3 gives estimated coefficients and their standardized values for model 2—the simplest model which fits the data—computed under both procedures.[3] The estimates are all identical—with the exception of the grand mean (or intercept) which has no substantive interpretation, anyway.

Table 3. Coefficient Estimates for Model 2 in Table 2*

Parameter	Estimate	Estimate/Standard Error
A	.366	4.49
B	−.997	−9.88
C1	.066	.75
C2	.826	10.65
A.B	−.216	−2.67
A.C1	.759	7.43
A.C2	.197	2.10
B.C1	.292	2.64
B.C2	−.074	−.69

* In the full table, the intercept is 4.62. In the collapsed table, the intercept is 5.31

To complete the example, let us examine the effect of variable A on variable B. Goodman says to collapse over both C and D and, then, to fit models to the two-way AB table. In Table 4, I have fitted both the saturated model (which allows for an effect of A on B) and the model of independence, which asserts that A has no effect on B. The model of independence can be rejected at the .05 level, and the estimated log-linear parameter for the effect of A on B is .17. Table 4 also displays results from fitting the same models to the full four-way table. The chi-squares are enormous, but the difference in chi-square (between the "saturated" model and the model of independence) is 4.5, the same as that in the

Table 4. Results from Fitting the Same Models to a Collapsed Table and to the Complete Table

Fitted Marginals	G^2	d.f.
Models for the AB Table:		
1. AB	0	0
2. A, B	4.5	1
Models for the ABCD Table:		
1. AB	1408.0	20
2. A, B	1412.5	21

---

[3] These estimates were produced by the program GLIM. Readers whose experience with programs for estimating log-linear models is limited to ECTA may find these estimates somewhat puzzling. Unlike ECTA, GLIM uses an algorithm known as iteratively reweighted least-squares which produces maximum-likelihood estimates equivalent to those produced by ECTA. However, GLIM uses a different parametrization of the model; whereas ECTA constrains the sums of certain parameters to be equal to zero, GLIM arbitrarily constrains one of the same set of parameters to be equal to zero (Holt, 1979). This difference is analogous to the difference between the parametrization of analysis of variance and the parametrization of multiple regression with a set of dummy variables as independent variables. Also, unlike ECTA, GLIM produces standard-error estimates for the parameters of unsaturated models which were used here to calculate standardized parameter values (each estimate is divided by its standard error). In spite of these differences, the point made by Table 3 is the same for GLIM or ECTA; you get the same parameter estimates whether you fit the model to the full table or to the collapsed table.

two-way table. The estimated parameter for the effect of A on B is also the same.

At this point, the general principle should be clear: to analyze a collapsed table without actually collapsing, simply fit to the full table the same marginal tables that you would have fitted to the collapsed table. The parameter estimates and their standard errors will be identical to those obtained from the collapsed table. The chi-square and the associated degrees of freedom will be higher; but differences in chi-square and degrees of freedom between models will be the same. To obtain the identical chi-square values, fit to the full table the "saturated" model for the collapsed table and then subtract its chi-square and degrees of freedom from those of the unsaturated models. Note that these results hold only for maximum likelihood estimates and for the likelihood ratio chi-square. For Pearson's goodness-of-fit chi-square, the equality will be only approximate.

Proof

At this point, the reader who is not interested in the mathematical detail may wish to skip ahead to the sections on additional applications. The equality of differences in chi-square is easily proved, using results in Goodman (1971). Consider models 1–5 in Table 2B. Goodman shows that the likelihood ratio chi-square for each of these models can be partitioned into two components: (a) the chi-square for testing the hypothesis that D is conditionally equiprobable, given variables A, B, and C; (b) the chi-square for a hypothesis about the ABC marginal table. The particular hypothesis tested in (b) corresponds to the fitted marginal tables. Since the component due to (a) is the same for all five models, it immediately follows that differences in chi-square must be equal to the differences in chi-square obtained by analyzing the ABC table directly. The generalization to higher-dimensioned tables is straightforward.

The equality of the estimated parameters is slightly more difficult to prove. Consider a three-way table for variables A, B and C. I will show that, if all parameters pertaining to variable C are set equal to zero, the estimated parameters pertaining to variables A and B are the same whether calculated from the three-way table or from the two-way table obtained by collapsing over variable C. First, some notations:

x_{ijk}—Observed cell frequency for level i of variable A, level j of B, and level k of C.

x_{ij+}—Observed cell frequency for table collapsed over C.

$\hat{m}_{ij}$—Maximum-likelihood estimate of expected cell frequency under a given model for the two-way table.

$\hat{m}_{ijk}$—Maximum-likelihood estimate of expected cell frequency under an equivalent model applied to the three-way table (all parameters pertaining to C must be set equal to 0).

$\hat{m}_{ij+}$—Frequencies obtained by collapsing $\hat{m}_{ijk}$ over C.

Estimated parameters are always algebraic functions of the estimated expected frequencies. What we have to show, therefore, is that the parameters obtained from $\hat{m}_{ijk}$ are the same as those obtained from $\hat{m}_{ij}$. The proof is in two steps. First, I show that the parameters obtained from $\hat{m}_{ijk}$ are the same as those obtained from $\hat{m}_{ij+}$. Then, I show that $\hat{m}_{ij+} = \hat{m}_{ij}$.

Step 1. In the table of frequencies, $\hat{m}_{ijk}$, C is independent of the variables A and B since we have explicitly set to zero all parameters linking C to A and B. We can therefore invoke the collapsibility theorem of Bishop et al. (1975:47), which says that if one set of variables is independent of another set, we can collapse over the first set without affecting the parameters pertaining to the second set. This means that we get the same estimated parameters from the $\hat{m}_{ij+}$ as from the $\hat{m}_{ijk}$.

Step 2. A basic property of maximum-likelihood estimation of log-linear models is that the estimated expected frequencies must sum to the fitted marginal tables (Bishop et al., 1975:69). Since we are fitting the same marginal tables to both the three-way and the two-way tables, obviously both $\hat{m}_{ijk}$ and $\hat{m}_{ij}$ must sum to the same marginal tables. It follows that $\hat{m}_{ij+}$ and $\hat{m}_{ij}$ must also sum to the same marginal tables. It is also the case (Bishop et al., 1975:69) that there can be only one set of estimated cell frequencies that both satisfies the internal constraints implied by the model and sums to the fitted marginals. Hence, $\hat{m}_{ij+} = \hat{m}_{ij}$.

This result is immediately generalizable to higher dimensional tables. Suppose we have a table for variables P, Q, R, S, and T and we wish to collapse over variables S and T. We can treat S and T as a single joint variable, C, and treat P and Q as a single joint variable, A. Letting R=B, we can apply the preceding proof directly.

Collapsing over Independent Variables

To this point, I have focused on the issue of collapsing over one or more dependent variables in a recursive system. These results also shed light on what it means to collapse over an independent variable. Consider, again, the example of a four-way table with variables A, B, C, and D and suppose that D is considered to be dependent on the other three variables in a logit-type model. In such a model, it is convenient to divide the log-linear parameters into two groups:

(a) Parameters that pertain to the dependent variable and its relationship with the independent variables;
(b) Parameters that pertain only to the independent variables and the relationships among them.

Although the parameters in group (b) are always estimated in a dependent variable model, they are not ordinarily of any substantive interest (Gillespie, 1978). This is because they measure relationships that are conditional on the levels of the dependent variable and, thus, have no causal interpretation.

Now, suppose that we want to estimate models which (among other things) assert that there is no effect of C on D. There are two ways to do this:

1. Set to zero all parameters which pertain to both C and D (equivalently, do not fit any marginal tables which include both C and D as variables) for any models fitted to the ABCD table.
2. Collapse over variable C to create an ABD table; then estimate models for this new table.

These two methods are equivalent in two senses. First, the estimated effects of A and B on D will be the same for both methods. Second, tests of hypotheses about the effects of A and B on D will give identical results for both methods. In other words, any conclusions we draw about the parameters in group (a) are identical under either procedure. This will not be true of the parameters in group (b) but, as I have already indicated, these do not generally have any substantive interpretation.

To illustrate these points, let us return to the data in Table 1. In Table 5, I present the goodness-of-fit statistics for four models fitted to the ABCD table and for four models fitted to the ABD table. All of these models can be interpreted as logit models for variable D as the dependent variable. (For that to be true, it was necessary to fit the marginal table for the independent variables in each model.) Models 1–4 correspond to models 5–8, respectively, in their assertions about the effects of variables A and B on variable D. This is not immediately apparent since the chi-squares (and their associated degrees of freedom) for models 1–4 are substantially higher than those for models 5–8. Yet, the differences in chi-square (and degrees of freedom) among models 1, 2, 3, and 4 are identical to the corresponding differences among models 5, 6, 7 and 8. Thus, tests of hypotheses about the effects of A and B on D yield the same results under either procedure.

In the lower panel of Table 5, I present parameter estimates for models 2 and 6. Both models allow for main effects of variables A and B on variable D but exclude any higher-order interactions. I have separated the parameters into group (a) those which pertain to variable D, and group (b) those which do not. The group (a) estimates are clearly the same under both procedures. In group (b), corresponding parameter estimates not only differ under the two procedures, but also, there are many parameters estimated under model 2 that do not appear at all in model 6 (i.e., all the parameters pertaining to the relationships between variable C

Table 5. Results for Models which Delete the Effects of an Independent Variable

Fitted Marginals	G^2	d.f.
Models for ABCD Table:		
1. ABC, ABD	143.8	8
2. ABC, AD, BD	147.5	9
3. ABC, AD	292.6	10
4. ABC, D	738.0	11
Models for ABD Table:		
5. ABD	0	0
6. AB, AD, BD	3.7	1
7. AB, AD	148.8	2
8. AB, D	594.2	3

Parameter Estimates for Models 2 and 6:

		Model 2		Model 6	
	Parameter	Estimate	Estimate/S.E.	Estimate	Estimate/S.E.
(a)	D	1.90	22.5	1.90	22.5
	A.D	−1.78	−19.9	−1.78	−19.9
	B.D	−1.01	−11.8	−1.01	−11.8
(b)	GM	3.30	32.7	4.74	58.6
	A	1.60	13.7	1.98	23.5
	B	−.30	−1.9	−.14	−1.5
	A.B.	−.41	−2.2	−.57	−6.3
	C1	.00	0.0		
	C2	.80	9.6		
	A.C1	.86	7.1		
	A.C2	.24	2.2		
	B.C1	.51	2.8		
	B.C2	.02	.1		
	A.B.C1	−.35	−1.5		
	A.B.C2	−.17	−.8		

and variables A and D). As we have repeatedly observed, however, the group (b) parameters are not relevant to the causal model.

We have shown that models 1–4 are equivalent to models 5–8 with respect to estimating and testing the effects of A and B on D. Yet, clearly, there is an advantage to fitting models to the full table rather than to the collapsed table. While model 5 completely ignores the relationship between C and D, model 1 explicitly asserts that C has no effect on D. Hence the goodness-of-fit statistic gives a direct test of that hypothesis. With a chi-square of 143.8, and 8 degrees of freedom, this hypothesis must obviously be rejected. If we had only collapsed the table over variable C—without performing this test—we would have been led to the misleading conclusion that model 6 fits the data very well. It may fit the collapsed table, but the corresponding model does not fit the full table.

The reader may have noticed that what is being asserted here about the equivalence of collapsing over independent variables, and setting effect parameters equal to zero, differs somewhat from the earlier discussion about collapsing over dependent variables. In the earlier section, we set to zero *all* the parameters pertaining to a given variable, and we asserted that the estimates for *all* the remaining parameters would be identical to those obtained from the collapsed table. Here, however, we set to zero only those parameters describing the effect of a given variable on the dependent variable, and we assert that the parameters describing the effects of other variables on the dependent variable are the same as those obtained from the collapsed table. While this is a different claim, the proof is quite similar. The assertion that differences in chi-square are the same for models 1–4 and models 5–8 follows from Goodman's (1971) demonstration that the chi-squares in models 1–4 can be additively decomposed into two parts: (1) a chi-square for testing the hypothesis that D is conditionally independent of C, given A and B; and (2) a chi-square for some hypothesis about the marginal table ABD. The equality of parameter estimates again follows from Bishop et al.'s (1975:47) collapsibility theorem. The proof is the same as that given earlier, except that a more general version of the collapsibility theorem is invoked.[4]

The more limited claim of this section could actually have been used in the preceding section where the aim was to collapse over dependent variables. Thus, to estimate the effects of A and B on C from the ABCD table, we could have set to zero only those parameters pertaining to both D and C instead of excluding *all* the parameters pertaining to D. The result would have been that estimates and tests of the effects of A and B on C would have been the same as those obtained from the ABC table. However, the parameters describing the relationship between A and B would have differed under the two alternative procedures.

It should also be noted that the results obtained in models 3 and 4 of Table 5 are equivalent to results that would be obtained by collapsing over both variables B and C to create a 2 × 2 table for variables A and D. Specifically, the A.D parameter estimated in model 3 is the same as that obtained from the 2 × 2 table; also, the difference in chi-square between models 3 and 4 is the same as that for the test of independence in the 2 × 2 table.

Collapsing Selected Categories

These results also apply to the case in which some, but not all, of the categories of a variable are collapsed into a single category. In Table 1, for example, education (variable C) has three categories: grade school, high school, and college. Suppose we believed that, net of other variables, there was no difference—in attitudes toward jobs for blacks—between those persons with grade-school educations and those with high-school educations. Yet, we still believed that there was a difference in attitudes between those with college educations and those without. One approach would be to collapse the grade-school and high-school categories into a single category and, then, estimate models for this collapsed table. Models 5–8 in Table 6 do just that. In this table, C′ represents the collapsed, two-category version of variable C. Alternatively, one could constrain the grade-school and high-school parameters to be equal in models for the full, uncollapsed table. While this is difficult to do with ECTA, it is easily accomplished with GLIM and certain other programs.[5] In Table 6, models 1–4 impose such constraints on the parameters for the relationship between the independent variables and the dependent variable. (No such constraints were imposed on the parameters describing the relationships among independent variables, since these have no causal interpretation.)

A comparison of models 1–4 with the corresponding models 5–8 shows that these two methods are, again, essentially equivalent. The chi-squares and degrees of freedom for models 1–4 are higher than those for the corresponding models 5–8, but differences in chi-squares and degrees of freedom within the first group are identical to the corresponding differences within the second group. Although not shown here, the estimated effects of A, B, and C′ on variable D are also the same

Table 6. Results for Models which Collapse over Selected Categories of a Single Variable

Fitted Marginals	G^2	d.f.
Models for the ABCD Table:		
1. ABC, ABC′D	52.9	4
2. ABC, ABD, AC′D, BC′D	58.6	5
3. ABC, AD, BD, C′D	64.7	8
4. ABC, AD, BD	147.6	9
Models for the ABC′D Table:		
5. ABC′D	0	0
6. ABC′, ABD, AC′D, BC′D	5.7	1
7. ABC′, AD, BD, C′D	11.8	4
8. ABC′, AD, BD	94.7	5

---

[4] The generalized version of the collapsibility theorem says the following: First, divide the variables into three mutually exclusive groups; if group 1 is independent of group 2 (but not necessarily group 3), we can collapse over group 1 without affecting any of the parameters pertaining to group 2. In this example, group 1 is variable C, group 2 is D, and group 3 includes A and B.

[5] Inquiries about GLIM should be addressed to Numerical Algorithms Group, Ltd.; NAG Central Office; 7 Banbury Road; Oxford OX2 6NN, U.K.

under either method. As in the preceding section, however, there is an important advantage to the method of imposing constraints on models for the full table; it provides an explicit test of the hypothesis that those constraints are true in the population. Thus, the chi-square for model 1 is a test statistic for the hypothesis that there is no difference—in attitudes about jobs for blacks—between those persons with grade-school and those with high-school educations. Clearly, that hypothesis can be rejected. Models 5–8, on the other hand, have nothing at all to say about that hypothesis.

REFERENCES

Bishop, Yvonne M. M., Stephen E. Fienberg, and Paul W. Holland
1975 Discrete Multivariate Analysis. Cambridge, Mass.: MIT Press.

Davis, James A.
1974 "Hierarchical models for significance tests in multivariate contingency tables." Pp. 189–231 in Herbert Costner (ed.), Sociological Methodology 1973–74. San Francisco: Jossey-Bass.

Fienberg, Stephen E.
1977 The Analysis of Cross-Classified Categorical Data. Cambridge, Mass.: MIT Press.

Gillepsie, Michael W.
1978 "The application of log-linear techniques to recursive models: comment on Reynolds." American Journal of Sociology 84:718–22.

Goodman, Leo A.
1971 "Partitioning of chi-square, analysis of marginal contingency tables, and estimation of expected frequencies in multidimensional contingency tables." Journal of the American Statistical Association 66:339–92.
1973 "The analysis of multidimensional tables when some variables are posterior to others: a modified path analysis approach." Biometrika 60:179–92.
1979 "A brief guide to the causal analysis of data from surveys." American Journal of Sociology 84:1078–96.

Holt, D.
1979 "Log-linear models for contingency table analysis: on the interpretation of parameters." Sociological Methods and Research 7:330–6.

Reynolds, H. T.
1977 "Some comments on the causal analysis of surveys with log-linear models." American Journal of Sociology 83:127–43.

Schwartz, M. A.
1967 Trends in White Attitudes toward Negroes. University of Chicago, NORC Report No. 119.

COMMENTS

KEYNESIAN GOVERNMENT INTERVENTION AND INCOME INEQUALITY

(COMMENT ON STACK, ASR, DECEMBER, 1978)*

In a recent paper, Steven Stack advances what he terms a "neo-Keynesian" model of income inequality, which makes the following predictions (Stack, 1978:883):

> [1.] the greater the direct government involvement in the economy (DGI) the less the unemployment and the less the income inequality;
> [2.] the greater the DGI, the greater the economic growth and the less the degree of income inequality; and
> [3.] that government spending will have an effect on income inequality independent of the level of development.

To test the validity of this model, Stack gathers data for 32 countries and regresses a measure of income inequality on three variables: (a) the sum of expenditures of central government, social security, and public enterprises as a percentage of GNP, 1959; (b) GNP per capita, 1965; and (c) GNP per capita growth rates, 1950–65. Because all the coefficient estimates have the predicted sign, and because the overall equation has a $\bar{R}^2$ of .57, Stack concludes that his neo-Keynesian model is generally consistent with the data. Specifically, he argues that the first and third propositions are strongly supported, but he finds that there is much less support for the second prediction concerning the effects of the rate of economic growth.

These conclusions, I believe, are too hasty. Stack's analysis is flawed by a lack of familiarity with basic aspects of the argument at hand and with elementary principles of data collection and analysis. I shall begin by discussing briefly some of the more obvious (and less important) flaws and then will turn to the more critical problems I find within Stack's paper.

Statistical Problems

To begin, Stack's main set of regression estimates is reported in a confusing and inconsistent way. For example, in Table 2, Stack reports that economic growth has a beta coefficient of −.333, with a t-ratio of 0.22 and a significance level of .005; while the level of economic development has a coefficient of −.239, a t-ratio of 2.02, and a significance level of .05. These figures are, of course, mutually inconsistent and one assumes that the first t-ratio is incorrectly reported. But, even if it is, the figures in Table 2 do not support the statement in the text that, unlike the level of economic development, growth has no significant effect on inequality.

Stack's (1978:887) comparison of the performance of his results with those of others is similarly confused over the issue of sample size and degrees of freedom. While it is true that large R^2s occur when the number of variables approaches the number of cases, Stack's assertion that the same pattern holds with corrected R^2s (or $\bar{R}^2$s) is false. I do not want to belabor the obvious, but the correction to R^2 is introduced *precisely* to compensate for the loss of degrees of freedom that occurs with increases in the ratio of parameters to cases. Stack's (1978:887) rule of thumb that one should have no more than "one variable for every ten cases" underscores his confusion, since the number of degrees of freedom is *not* a simple linear function of the number of parameters in the model and the number of cases analyzed.

One other minor point is worth noting here. The statement, ". . . that the original association between growth and inequality is spurious and that *growth affects inequality indirectly through its association with DGI*" (Stack, 1978:886, italics added), is a non sequitur and reflects ignorance about what a spurious association really is. If the original association is spurious, then the second (italicized) part of his statement is false. This is quite clear from Stack's own Figure 1, which specifies DGI → Growth → Inequality. Obviously, if the association between growth and inequality is spurious, then growth does not *affect* inequality directly or indirectly.

However, even if Stack were to correct these problems, his analysis would remain critically flawed. In particular, there are serious problems with his selection of cases and use of data sources. I shall deal with these, in turn.

---

* Direct all communications to: Robert W. Jackman; Dept. of Political Science; Michigan State University; East Lansing, MI 48824.

I would like to thank Mary R. Jackman, Brian D. Silver, and Jeffrey D. Straussman for their advice and comments.

Selection of Cases: Keynesianism in East Europe?

Stack places great emphasis on the idea that his is a test of a neo-Keynesian argument. As most readers will be aware, Keynes's argument concerns, inter alia, the effects of government spending and manipulation of the money supply *within the context of capitalist or market economies* (see, e.g., Klein, 1947; the essays in Lekachman, 1964; and Stein, 1969:156–65). In particular, if it is possible not to overstate the obvious, the Keynesian perspective addresses government intervention in market economies, as opposed to the central planning that is typical of the Soviet and East European economies. The distinction here—between Keynesian economic manipulation and central planning—involves fundamental, qualitative differences in both the nature *and* form of government intervention, so that the planners of East Europe and of the Soviet Union are not simply Keynesians writ large. Indeed, the distinction is also implicit in Stack's (1978:882–3) rendering of the argument. For example, after contrasting the United States and Sweden, he writes that

> the impact of government spending on unemployment and income inequality should be felt not only through direct job creation programs in government, but also through the *multiplier effect* or the multiple reinvestment of money obtained from higher levels of consumption in job creative ventures in the private sector. (Stack, 1978:883, italics in original)

Reasonable people would, therefore, expect the analysis to focus on Western and (perhaps) on some Third World countries.

This impression is not contradicted elsewhere in the text of Stack's (1978:884) paper, where we are told, "The present study utilizes Paukert's [1973] data bank," a data series that refers to Western and Third World countries. Only by reading footnote 9 does one become aware of the fact that eight socialist countries are also included in the analysis. Indeed, apart from the list of countries reported in the Appendix, this footnote is the only place in which Stack alludes to the presence of these socialist countries in his study.

An obvious question that this raises is how Stack's neo-Keynesian model performs if we focus on the kinds of economies that concern Keynesians?—i.e., if we exclude from consideration the eight socialist countries? Clearly, the empirical validity of Stack's hypotheses should not depend on the inclusion of centrally planned economies in the analysis.

To pursue this question, I have reestimated Stack's model for those Western and Third World countries that are covered by Paukert (1973:114–5) and for which data on the independent variables are available. The data are taken from the sources used by Stack. Thus, the measure of direct government intervention (DGI) is the sum of expenditures of central government, social security, and public enterprises as a fraction of GNP, and comes from Russett, et al. (1964:65–6). Similarly, figures for both GNP per capita (GNP/cap) and GNP growth rates (EG) are from Taylor and Hudson (1972:314–21). There are 24 Western and Third World countries for which data on income inequality and these three independent variables are available (all 24 of these countries are included in Stack's analysis).

Table 1 reports four sets of estimates. The first row contains metric parameter estimates for all three independent variables for all 24 countries. Of this set of estimates, only that for GNP/cap is more than twice its standard error (t = 2.31), while that for DGI is *smaller* than its standard error (t = .60) and is of the *wrong sign* (remember that, according to Stack, DGI is

Table 1. Regressions of Gini Index of Income Inequality on Government Spending, GNP Per Capita, and GNP Growth Rates**

N	Constant	DGI	GNP/cap	EG	R^2	$\bar{R}^2$	F
24	.496*	.138 −2	−.487 −4*	−.137 −1	.247	.134	2.18
	(.055)	(.230 −2)	(.211 −4)	(.131 −1)			
24	.478*	.217 −3	−.459 −4*		.206	.130	2.72
	(.053)	(.202 −2)	(.210 −4)				
22***	.500*	.238 −2	−.624 −4*	−.155 −1	.347	.239	3.19
	(.053)	(.228 −2)	(.215 −4)	(.127 −1)			
22***	.479*	.107 −2	−.586 −4*		.294	.219	3.95
	(.051)	(.204 −2)	(.216 −4)				

* Starred coefficients are at least twice their standard errors.

** Main table entries are parameter estimates, and entries below them in parentheses are their standard errors of estimate. Scientific notation is used to avoid endless strings of zeros.

*** Excluding Italy and Burma, for reasons given in the text.

supposed to *reduce* inequality). However, since there is a positive correlation between EG and DGI (r = .476), it is possible that the effects of DGI are being masked in some way by the inclusion of EG in the model (besides, the estimate for EG is barely larger than its standard error). To check this, the second set of estimates in Table 1 examines only the effects of DGI and GNP/cap. This modified set of estimates produces the same result: the coefficient for DGI remains statistically insignificant and of the wrong sign. In short, both of these sets of estimates provide little support for Stack's neo-Keynesian argument.

There is a problem, however, with these estimates. Two of the 24 countries have strange income inequality data. According to Paukert (1973:125), the figure for Burma constitutes a particularly rough approximation and is therefore of "rather doubtful value."[1] In addition, the Italian figure refers to 1948, a date that precedes that of the independent variables by a huge margin. To ensure that the inclusion of these countries is not affecting the analysis, the third and fourth rows of Table 1 report estimates corresponding to those in the first half of the table for the remaining 22 countries. These revised estimates suggest that the inclusion of Burma and Italy serves only to introduce noise into the analysis. Excluding Burma and Italy raises the $\bar{R}^2$s and increases the F-ratios to levels where they are significant at the .05 level. But the estimated effects of DGI in the third and fourth rows of Table 1 remain statistically insignificant and of the wrong sign.

Another possible objection to the foregoing analysis is that my use of Gini ratios is potentially misleading, because these ratios are insensitive to cases where the Lorenz curves intersect (see, e.g., Atkinson, 1975:29–49; Sawyer, 1976), while they are more sensitive to differences around the middle of the income distribution than they are to differences around the tails of the distribution (see, e.g., Champerowne, 1974). These considerations suggest that we might profitably examine more detailed data on the share of income received by successive population quintiles, ranging from the most- to the least-wealthy. Accordingly, Table 2 reports the estimates for four separate regressions on the three independent variables for the 22 countries analyzed in the bottom two rows of Table 1. The dependent variable in the first equation is the share of income received by the most-wealthy 20% of the population, while that in the fourth equation is the share received by the poorest 40%. Three of the four equations in Table 2 have F-ratios that are significant beyond the .05 level, while the equation for the share of income received by the poorest 40% in the bottom row does not meet this condition. In addition, the first three sets of estimates have higher $\bar{R}^2$s than did the corresponding estimates using Gini ratios. Apart from this, however, these figures simply reinforce the conclusions drawn earlier.[2] In no case is an estimate for DGI in Table 2 more than twice its standard error and, again, in three of the four cases, the estimate for DGI is of the wrong sign.

Table 2. Detailed Regressions of Income Inequality on Government Spending, GNP Per Capita, and GNP Growth Rates (N = 22)**

Dependent Variable	Constant	DGI	GNP/cap	EG	R^2	$\bar{R}^2$	F
Top 20%	58.651*	.936 −1	−.622 −2*	−.979	.419	.322	4.32
Q1	(4.723)	(.202)	(.190 −2)	(1.124)			
Q2	16.724*	.121	.169 −2*	−.277	.423	.327	4.40
	(1.860)	(.794 −1)	(.749−3)	(.443)			
Q3	10.744*	−.240 −1	.252 −2*	.381	.511	.430	6.28
	(1.627)	(.695 −1)	(.655−3)	(.387)			
Bottom 40%	13.910*	−.201	.217 −2*	.894	.268	.146	2.19
Q4 + Q5	(2.463)	(.105)	(.991 −3)	.586			

* Starred coefficients are at least twice their standard errors.

**Main table entries are parameter estimates, and entries below them in parentheses are their standard errors of estimate. Scientific notation is used to avoid endless strings of zeros.

---

[1] I should point out that others besides Stack seem to have ignored this caveat of Paukert's. Along with Burma, Paukert (1973:125) lists six other countries for which he judges the inequality data to be of "doubtful value." As far as one can gather (since he provides no list of countries), Chase-Dunn (1975) used all seven of these observations, which constitute almost a quarter of his sample. Similarly, Rubinson (1976) used six of these seven cases.

[2] Note, again, that the same pattern is obtained when GNP growth rates (EG) are excluded from the model (as was done in the bottom row of Table 1).

To summarize, an analysis based on data restricted to market economies provides no support for the neo-Keynesian model advanced by Stack. In particular, DGI has no measurable effect on inequality in market economies. The results Stack reported must therefore be a function solely of the inclusion of eight socialist countries in his analysis which, in turn, means that the results he obtained do not bear on Keynesian arguments.

Data Sources

If Stack's selection of cases fits poorly with his argument, his use of data sources is positively cavalier. Consider his description of the source of his information on government spending. We are told, "Comparative data on government spending were collected as part of the cross-national data archive of Russett et al. (1964)." After a brief discussion of this variable, Stack (1978:885) then states, "Data were available on this measure for 32 countries for which income inequality data are also available." Reasonable people might infer from this description that the data are, in fact, available in the source cited. Unfortunately, any such inference is false. The Russett handbook does *not* include data on government spending for the eight socialist nations that constitute one fourth of the cases analyzed by Stack. Indeed, it does not contain such data for *any* socialist societies.

To understand why data for these countries are excluded from Russett's handbook, it is necessary only to go back to the principal original sources used by Russett and his colleagues, which are the various *Statistical Yearbooks* and the more detailed *Yearbooks of National Accounts Statistics* published by the United Nations. For most of the eight countries, no data are available for *circa* 1959, while those data that are available are simply not comparable to the data available for market economy countries. First, the Russett et al. (1964:65–6) figures refer to *central* government spending; those in the U.N. Yearbooks for socialist countries refer to *general* government spending, which includes local and provincial government spending (Yugoslavia is the only partial exception to this). This in itself makes the data for socialist countries not comparable to the data for other countries. Second, socialist countries do not report their national accounts according to the standard U.N. format that is used by other countries. As a result, the country tables for these societies in the *Yearbooks of National Account Statistics* are all prefaced with the following note (or a close variant of it):

> The concept governing the official estimates of [country name—USSR, etc.] is that of net material product which is defined as the total net value of goods and "productive" services, including turnover taxes, produced by the economy in the course of a year. Specifically, the estimates cover the following economic activities: agriculture and forestry, mining, manufacturing, construction, transport and communication, trade and catering. Economic activities not contributing directly to material production, such as public administration and defence, personal and professional services, and similar activities, are not included.
>
> *For these and other reasons relating to the treatment of particular items, the estimates in the table below are not comparable in concept and coverage with those of the United Nations system of national accounts.* (emphasis added)

These two problems obviously affect the numerator of the term used to measure government influence in the economy (central government expenditures, etc.). Note that the second problem also affects the denominator of the term, GNP. Since the figures on government spending are reported in national currencies, they have to be deflated by GNP expressed in national currency. However, the U.N. *Statistical Yearbooks* report the wealth of socialist countries in terms of net material product, not GNP, and, as I have just pointed out, the two are not comparable. While it is possible to compare the figures on net material product *within* the group of socialist countries, comparing these countries with others is much more difficult (see also the discussion on this point in Russett et al. [1964:150], and Taylor and Hudson [1972:289]).

If data on DGI for the socialist countries are inherently not comparable with those for other countries and, if they are not readily available for all eight socialist economies, how did Stack score these countries on this variable? It appears (Stack: personal communication) that he proceeded by assuming that DGI scores for the "command socialist countries were at the 99 percent level," while those for the "market socialist countries" (presumably Yugoslavia) were "in the 90 percent range" (compare this with the highest score for a Western country [France] of 40% [Russett et al., 1964:65]). In other words, the eight socialist countries were simply assumed to have extremely high DGI scores. While this may help account for the differences between Stack's results and those reported above, the assumption itself is patently unreasonable. Consider the Soviet Union, presumably a "command socialist economy." Recent evidence indicates that even the legal component of the "second" or market economy accounted for at least 22% of GNP in 1950 and at least 10% by 1968 (which suggests that for 1960 a figure of 15% is probably close). Also, even by 1968, estimates suggest that the legal second economy accounted for between 15–25% of household consumption (Grossman, 1977:35). Adding to this the illegal second economy (of unknown size) suggests clearly that Stack's guessed value of DGI for the Soviet Union is far too high (see Grossman, 1977; Katsenelinboigen, 1977). Nor is the second economy an issue only in the Soviet Union—evidence suggests that its legal component is considerably larger in other East European countries (Grossman, 1977:40). These considerations imply that Stack's procedure does not even produce reasonable guesses of the values of DGI for the socialist countries.

In short, Stack fails to identify the sources of his information on government intervention in the socialist economies, and is less than candid about the facts that this information is not available in a form that allows meaningful analysis and that his estimates are simple guesses. Even if the inclusion of socialist countries could be justified on theoretical grounds (and that would be a tall order), these problems with Stack's measure of DGI are crippling.

Similar problems exist with Stack's data on income inequality for the socialist countries. His complete statement on this issue is as follows: "In addition to the data in the Paukert data bank we were able to obtain comparable data on eight socialist nations from the meticulous work of Michal (1973), Szafnicki, (1971), the United Nations (1967), and Yanowitch (1969)" (Stack, 1978:884, fn. 9). Note, first, that—although different sources sometimes give different figures for the same country—Stack does not identify which country values come from which source, or the specific tables or pages that he used in these sources; and, second, that no serious effort is made to justify the comparability of these additional data. More distressing, perhaps, are the facts that a careful reading of these four sources does not satisfactorily resolve the first issue, and that such a reading undermines any claim that the data are comparable with each other, let alone with Paukert's data. Let me elaborate.

In none of the four sources can I find any information for Romania on the size distribution of income among individuals or families. Michal (1973:410–1) gives data for Czechoslovakia, Hungary, and Yugoslavia; Szafnicki (1971:39) has information on Poland; Yanowitch (1969:145–6) discusses the USSR; while the United Nations (1967:Tables 9.19 and 9.20) gives figures for East Germany, as well as data for Czechoslovakia, Hungary, and Poland that conflict with those given in the first two sources, along with partial data for Bulgaria (Table 8.20).

The figures that Michal gives are for the pre-tax size distribution of earners' income, not household or family income (cf., Paukert's [1973:113] emphasis on the latter). The consequences of this distinction are not trivial, as Kuznets (1963:3–8), Sawyer (1976:4–6), and others have stressed.[3] In addition, Michal's data for Hungary apply *only* to "state sector" employees, a group that by his own estimate constituted approximately 60% of the work force in 1960. This means that Michal's information for Hungary is not in a form that can be compared either to his data for the other two countries he considers or to Paukert's figures.

Similar problems arise when we examine Szafnicki's (1971) paper, which examines data from a 1967 sample survey carried out in the city of Lodz, *not* in Poland as a whole. While the specific biases that this introduces cannot be specified precisely, it is clear that Lodz is not representative of Poland. For one thing, it does not reflect rural and agricultural sectors, despite the fact that, in 1960, 47% of the Polish work force was engaged in agriculture (United Nations, 1967:Table 8.28). Another reason for doubting that Lodz accurately reflects Polish society comes from evidence indicating that, of eight socialist countries in East Europe in 1968, Poland had the second highest scores for interregional inequality in industrial employment (Koropeckyj, 1972:73. Poland scored second only to Hungary in this respect).

These considerations undermine Stack's assertion that the income inequality data for socialist countries are "comparable" (this lack of comparability doubtless accounts for Paukert's [1973] decision not to include data on these countries from the United Nations and the Yanowitch studies, even though he used the former study as a source of data on several western European countries). In addition, it is important to note that the sources cited by

---

[3] To be fair, while Paukert claims that the income units for his data are "families or households," this is not always the case. For example, for seven West European countries, his data are from United Nations (1967:Table 6.10), which used taxpayers' income, not family income, as the unit. To gain some sense of the impact of this distinction compare the data that Sawyer reports in his Table 3 with his data for Denmark and Finland (1976:23–4). This and related problems that Sawyer discusses imply that Stack's acceptance of even Paukert's data is insufficiently critical, although Stack is certainly not alone in this respect (cf., Chase-Dunn, 1975; Rubinson, 1976; Hewitt, 1977; and Rubinson and Quinlan, 1977).

Stack provide conflicting data. For example, Michal (1973:410–1) gives a Gini ratio of .226 for Hungary, while the United Nations (1967: Table 9.20) gives .27. Similarly, Szafnicki (1971:39) gives a Gini ratio for "total family income" in Lodz of .166, which is about half the United Nations estimate for Poland as a whole of .29. (I should emphasize that the differences between these conflicting estimates are not trivial. To place them in context, note that for the 22 Western and Third World countries examined in Table 2 above, Paukert's Gini index ranges from .30 to .61, with a mean of .43 and a standard deviation of .09.) One assumes that Stack took the lower figure where possible (since there would otherwise be no point in citing Szafnicki), so that socialist countries are scored as being much more egalitarian than others. Indeed, this helps account for the correlations reported by Stack. While DGI is not related to income inequality when the socialist countries are excluded, a large correlation is readily induced when those countries are included and assigned extraordinarily high DGI scores coupled with very low Gini ratios. A careful examination of the relevant scatterplots and residuals might have revealed to Stack just how artificial this correlation is.

Summary

Stack's claim that his analysis provides support for a neo-Keynesian model of income inequality is groundless. The most critical flaw in his paper is his inclusion of socialist countries, partly because comparable and meaningful data for these countries are not available for the relevant variables, and, partly, because his model falls flat when these countries are excluded from consideration. Most important, however, Keynesian arguments do not apply to socialist economies, because the form of government intervention in these economies is qualitatively distinct from the intervention found in other economies. For this reason, Stack's analysis is irrelevant to the hypotheses that motivated his inquiry.

ROBERT W. JACKMAN
Michigan State University

REFERENCES

Atkinson, A. B.
1975 The Economics of Inequality. Oxford: Clarendon Press.

Champerowne, David
1974 "A comparison of measures of inequality of income distribution." The Economic Journal 84:787–816.

Chase-Dunn, Christopher
1975 "The effects of international dependence on development and inequality: a cross-national study." American Sociological Review 40:720–38.

Grossman, Gregory
1977 "The 'second economy' of the USSR." Problems of Communism 26:25–40.

Hewitt, Christopher
1977 "The effect of political democracy and social democracy on equality in industrial societies: a cross-national comparison." American Sociological Review 42:450–64.

Katsenelinboigen, A.
1977 "Coloured markets in the Soviet Union." Soviet Studies 24:62–85.

Klein, Lawrence R.
1947 The Keynesian Revolution. New York: Macmillan.

Koropeckyj, I. S.
1972 "Equalization of regional development in socialist countries: an empirical study." Economic Development and Cultural Change 21:68–86.

Kuznets, Simon
1963 "Quantitative aspects of the economic growth of nations: VIII. Distribution of income by size." Economic Development and Cultural Change 11(part 2):1–80.

Lekachman, Robert (ed.)
1964 Keynes' General Theory: Reports of Three Decades. New York: St. Martin's Press.

Michal, Jan
1973 "Income distribution of earnings and household incomes in small socialist nations." Review of Income and Wealth 19:407–28.

Paukert, Felix
1973 "Income distribution at different levels of development: a survey of evidence." International Labour Review 108:97–105.

Rubinson, Richard
1976 "The world economy and the distribution of income within states: a cross-national study." American Sociological Review 41:638–59.

Rubinson, Richard, and Dan Quinlan
1977 "Democracy and social inequality: a reanalysis." American Sociological Review 42:611–22.

Russett, Bruce M., Hayward R. Alker, Jr., Karl W. Deutsch, and Harold D. Lasswell
1964 World Handbook of Political and Social Indicators. New Haven: Yale University Press.

Sawyer, Malcolm
1976 "Income distribution in OECD countries." OECD Economic Outlook: Occasional Studies (June): 3–36.

Stack, Steven
1978 "The effect of direct government involvement in the economy on the degree of income inequality: a cross-national study." American Sociological Review 43:880–8.

Stein, Herbert
1969 The Fiscal Revolution in America. Chicago: University of Chicago Press.

Szafnicki, Krzysztof
1971 "Distribution of wages and income among individuals and groups." Polish Sociological Bulletin 24:32–47.
Taylor, Charles L., and Michael C. Hudson
1972 World Handbook of Political and Social Indicators. 2nd ed. New Haven: Yale University Press.
United Nations
1967 Economic Survey of Europe in 1965, Part 2—Incomes in Postwar Europe: A Study of Policies, Growth and Distribution. Geneva: United Nations.
Yanowitch, Murray
1969 "The Soviet income revolution." Pp. 143–54 in Celia S. Heller (ed.), Structured Social Inequality. New York: Macmillan.

THE CASE OF THE *MISSING-VALUES* CARD, AND OTHER MYSTERIES: ANOTHER LOOK AT THE EFFECT OF GOVERNMENT SPENDING ON INCOME INEQUALITY

(COMMENT ON STACK, ASR, DECEMBER, 1978)*

The *ASR* recently has published a number of cross-national studies of income inequality (Jackman, 1974; Rubinson, 1976; Rubinson and Quinlan, 1977; Hewitt, 1977; Stack, 1978a; 1978b). This paper reanalyzes the data from one of these studies (Stack, 1978b) and shows that his conclusions are wrong because his analysis is fundamentally flawed; in fact, his conclusions stem from a series of errors. In the process, we touch base with some issues which macrolevel cross-national studies, in general—and cross-national studies of inequality, in particular—cannot afford to overlook.

The Missing Missing-Values *Card*

Cross-national variations in income inequality have been explained in terms of national differences in (1) economic development/industrialization (Kerr et al., 1964; Lenski, 1966; Cutright, 1967; Jackman, 1974); (2) political democracy/socialism (Lenski, 1966; Cutright, 1967; Hewitt, 1977; Stack, 1978a); and (3) position in the world economy (Rubinson, 1976; Bornschier et al., 1978). Stack (1978b) proposes a fourth contender, which he calls *Keynesian* (or, in some places, *neo-Keynesian).* His Keynesian approach asserts that the "degree of direct government involvement in the economy should reduce income inequality through such means as full employment and the facilitation of economic growth" (Stack, 1978b, abstract). For a cross-section of 32 nations Stack finds that government involvement has a significant negative effect on income inequality, with economic level and rate of economic growth controlled; he views this result as consistent with his hypothesis.

I retraced his study, from first datum to last beta coefficient, and found that his results are seriously in error. Let us begin at the beginning. Stack's "Keynesian model" has four variables: income inequality (dependent variable), government involvement (key independent variable), and economic level and rate of growth (control variables). Estimates of per capita GNP and rate of growth of per capita GNP (his measures of economic level and rate of economic growth, respectively) are readily available for almost all nations. Estimates of income inequality (measured by the Gini index) and direct government involvement (measured by level of government spending) are available for only a few countries. Hence, Stack chose as his sample those nations for which he could find estimates of both income inequality *and* government spending. Or, at least, I so interpret his discussion on p. 885:

> Comparative data on government spending were collected as part of the cross-national data archive of Russett et al. (1964). We will utilize their data on the sum of the expenditures of the central government, social security, and public enterprises as a percent of the GNP. . . . Data were available on this measure for 32 nations for which income inequality data were also available. Our study is based on these countries.

In point of fact, however, only 24 of Stack's 32 nations (see his Appendix [Stack, 1978b], which lists the 32 nations) are included in the estimates of government spending found in Russett et al. (1964: Table 17); the eight socialist nations in Stack's sample (Eastern European nations, and the USSR) have no accessible data on government spending.

My first hunch was that Stack found estimates on government spending in sources which he forgot to mention in the article. However, in consulting other data sources, I was not able to locate estimates of government

---

* Direct all communications to: Glenn Firebaugh; Department of Sociology; Vanderbilt University; Nashville, TN 37200.

Discussion with Michael Hughes, Anthony Oberschall, and Gregory Sampson were helpful in the development of this paper. The paper was written and accepted for publication in the spring of 1979; since then other cross-national studies of income inequality have been published. However, the more recent studies do not affect this critique.

Table 1. Zero-Order Correlation Coefficients for Stack's Model, I

Variable	Inequality	Government Involvement	Economic growth
Government Involvement	−.74[a], −.16[b]		
Economic growth	−.46, −.48	.61, .48	
Economic level	−.24, −.23	.01, .39	−.06, −.04

[a] Coefficient reported by Stack (1978b:Table 1).
[b] Coefficient I obtained using Stack's measures and data sources.

spending for any of the socialist nations except Yugoslavia (and then only for 1970–73; IBRD, 1976:388), even though I was able to locate 48 other nations, missed by Stack, with data on both government spending and income inequality. This led me to guess that the eight socialist nations in Stack's sample are indeed missing data on government spending, his statements to the contrary notwithstanding.

To test my hunch that Stack was not able to locate these data I used his measures, his data sources, and his sample in an attempt to duplicate his correlation matrix (see Appendix, here, for data). Table 1 gives the results. Note that I was successful in duplicating his correlations between inequality and economic growth, inequality and economic level, and economic growth and economic level (I am willing to assume that differences of the order .02 are due to rounding errors or differences in minor judgments that had to be made in data collection). Apparently, then, I was able to duplicate his data on economic growth, economic level, and inequality.

However, the correlations which include government involvement diverge sharply. Of particular interest is the correlation between government involvement and inequality, since Stack's conclusions hinge on this coefficient: in Stack's analysis 55% of the variance in inequality is associated with government involvement (r = −.74), while in my reanalysis only 3% of the variance in inequality is associated with government involvement (r = −.16).

What could cause such a large discrepancy? Assume for the moment that Stack was able to locate data on government involvement for the socialist nations. What level of government spending for these eight cases would we need, to change the correlation from −.16 to −.74? Obviously, it would need to be larger than the mean for the 24 nonsocialist nations, since the level of inequality is lower for the socialist nations (mean = .24 versus .43). Suppose that, in these eight nations, government spending reached 99.9% of the GNP: Would that be large enough to change the correlation from −.16 to −.74? To find out, I gave the socialist nations a value of 99.9% on government involvement and recomputed the correlations. The results are given in Table 2. The assumption that government spending reaches 99.9% of the GNP in socialist nations does indeed duplicate Stack's correlations (within .03 in all cases). Coincidence? I think not. Note that 99.9 is a value one would likely use for missing data. While *we* know that a value of 99.9 for government spending is ridiculous, a computer does not. In short, circumstantial evidence indicates that a missing-values card was missing in Stack's computer program.[1]

Does this mistake affect Stack's conclusions? Table 3 compares Stack's results with the results where the missing data card is

Table 2. Zero-Order Correlation Coefficients for Stack's Model, II

Variable	Inequality	Government Involvement	Economic growth
Government Involvement	−.74[a], −.75[b]		
Economic growth	−.46, −.48	.61, .64	
Economic level	−.24, −.23	.01, .01	−.06, −.04

[a] Coefficient reported by Stack (1978b:Table 1).
[b] Coefficient I obtained using Stack's measures and data sources, giving the eight socialist nations a value of 99.9% for government involvement.

---

[1] The alternative is to assume that, for the socialist nations, Stack *intended* to code government spending at or near 100 percent of the GNP—that is, Stack arrived at his government involvement measure for the socialist nations by equating government spending with GNP. To be sure, in these nations, the bulk of GNP flows from the public sector, but this is clearly not the type of government spending Keynes had in mind; a government spending/GNP ratio which is near 1.0 is hardly Keynesian, since Keynes did not advocate the abolition of the private sector. I will discuss this point further when I focus on Stack's measures; here, it suffices to note that operationalizing Keynesian theory in such a way that it makes Keynes a socialist is no less a mistake than forgetting the missing-values card.

Table 3. Determinants of Inequality, Stack's Model: Stack's Results vs. Corrected Results

Independent Variable	Stack's Results[a]			Corrected Data[b]		
	b[c]	β[d]	F	b[c]	β[d]	F
Government Involvement	−.003	−.72	23.4	.004	.29	1.6
Economic Level	−[e]	−.24	4.1	−.00005	−.37	3.2
Economic Growth	−.002	−.03[f]	.05	−.042	−.64	8.9
R^2 (adjusted)	.57			.25		

[a] From Stack (1978b:Table 2)

[b] Pairwise deletion of missing cases was used. The direction (though not the magnitude) of the effects is the same under listwise deletion: b is .001 for government involvement, −.00005 for economic level, and −.014 for economic growth.

[c] Metric regression coefficient.

[d] Standardized regression coefficient.

[e] Unknown; Stack reports −.000.

[f] Stack reports −.33, but that is apparently a typographical error.

firmly in place.[2] Note that correcting the data (1) reverses the sign of the effect of government involvement and (2) reverses the order of importance of the variables (note the β's and F's). The former is particularly devastating to Stack's paper, since its central message is that government involvement *reduces* inequality. In sum, Stack's finding that government involvement reduces inequality apparently is based entirely on an error in the analysis; when this error is corrected, his data do not support his hypothesis.

Are there other serious mistakes in this analysis? Yes; and they should be detailed, since other macrolevel cross-national studies can be subject to some of these same mistakes. Further, these other lapses, while less stunning than the mishandling of the missing data for the socialist nations, are no less devastating to Stack's conclusions. I have grouped these lapses into three categories: model specification, cross-sectionalism, and operationalization and measurement.

Model Specification

Alternative models. As noted at the outset, Stack's study is a recent addition to an emerging body of literature on cross-national variations in income inequality. Stack cites this literature but, for the most part, he does not build upon it. For example, Rubinson (1976) found that a nation's dependence on external markets, its economic penetration by foreign nations, and its state strength all are significantly related to its degree of income inequality. Stack cites Rubinson's study, but he fails to control for any of these factors. Stranger still, in an earlier study, Stack (1978a) himself concluded that the "most important determinant of the degree of inequality is . . . democratic performance" (p. 271) and, thus, "the internal political model should not be neglected" (p. 272), yet measures of the polity are notable for their absence in his later paper. If political democracy does indeed affect income inequality; then it should be included in models of income inequality; if Stack (1978a) is correct, then Stack (1978b) uses a misspecified model.

What happens to the effect of government involvement when these other variables are controlled? Tables 4 and 5 give the results (for the uncorrected and corrected data, respec-

Table 4. Effect of Direct Government Involvement (DGI) on Inequality, *Uncorrected* Data: Alternative Models

Variables Controlled	Effect of DGI[a]		
	b[b]	*s.e.(b)*[c]	*F*
1. Economic level & economic growth (Stack model)	−.003	.001	23.4
2. Stack model & political democracy	−.002	.002	.9
3. Stack model & political democracy & state strength	−.001	.002	.2
4. Stack model & political democracy & state strength & dependence on foreign markets	.001	.002	.7

[a] Uses Stack's metric on DGI.

[b] Metric regression coefficient.

[c] Standard error of regression coefficient.

---

[2] To avoid redundancy, I do not report my results where I omit the missing-values card; they parallel Stack's results, as one would expect from the similarity in the correlations (Table 2).

Table 5. Effect of Direct Government Involvement (DGI) on Inequality, *Corrected* Data: Alternative Models

Variables Controlled	Effect of DGI[a]		
	b[b]	*s.e.(b)*[c]	*F*
1. Economic level & economic growth (Stack model)	.004	.003	1.6
2. Stack model & political democracy	.001	.003	.2
3. Stack model & political democracy & state strength	.001	.003	.1
4. Stack model & political democracy & state strength & dependence on foreign markets	.0001	.003	.001

For [a], [b], and [c] see notes to Table 4.

tively) for three alternative models. Following Stack's (1978a) advice, I added a measure of political democracy to Stack's (1978b) Keynesian model. Democracy is a difficult construct to operationalize; no measure enjoys universal acceptance. I used a measure readily accessible to me, the "political participation" index of Olsen and Firebaugh (1975); this measure parallels Jackman's (1974) "democratic performance" index on three of its four indicators (for the fourth indicator it replaces his measure of voting turnout with a measure of the freedom for opposition groups to organize). With the addition of this index, government involvement does not even attain statistical significance in the *uncorrected* data (Table 4).

In the second alternative model, I included a measure of state strength as well as of political democracy. Rubinson (1976) used government revenue/GDP as his primary measure of state strength, but Stack (1978a:fn. 1) criticized this measure for being too restricted. In view of this criticism, I replaced government revenue/GDP with an index which combines indicators of legislative effectiveness, administrative modernity, government size, and public order (from Olsen and Firebaugh, 1975). In this model, as in the previous one, the effect of government involvement falls short of statistical significance.

Finally, I added a measure of dependence on external markets. Following Rubinson (1976) and Stack (1978a), I measured this by exports/GDP. Once again the effect of government involvement is small (it is even in the wrong direction) and falls far short of statistical significance. In sum, under these reasonable alternative specifications the large, statistically significant, effect of government involvement reported by Stack disappears.[3]

Causal direction. Consider Stack's causal model (Stack, 1978b, Figure 1): economic level (per capita GNP in 1965) affects income inequality (*circa* 1960) directly, and indirectly through government spending in 1959 and economic growth (rate of change of GNP per capita, 1950–1965); government spending affects inequality directly, and indirectly through economic growth; and economic growth affects inequality directly. We note immediately that some of the effects in this model operate backward in time: 1965 GNP causes 1950–1965 rate of growth of GNP, 1959 government spending, and 1960 income inequality; and 1959 government spending causes 1950–1965 rate of growth of GNP.

Worse, this model ignores some very likely reciprocal effects (see Rubinson and Quinlan, 1977). For example, there is an extensive body of literature in economics about the effect of income distribution on economic growth. Also, it seems likely that inequality could affect government spending (for example, during recessions government spending may increase in response to increases in unemployment and inequality).

Omission of key Keynesian variables. In his abstract, Stack asserts that the paper "tests the Keynesian notion that the degree of direct government involvement in the economy should reduce income inequality through such means as full employment and the facilitation of economic growth." Similar statements appear throughout the paper.[4] When we get to his model, however, we search in vain for a mea-

---

[3] Stack (1978b:fn. 13) criticizes Hewitt for having *too many* variables in his analysis: "Hewitt (1977) violates one statistical rule of thumb by having more than one variable for every ten cases in his sample." But such a rule was never intended to be a justification for misspecification; when the cases/variables ratio is too small, adding cases is much preferable to purging causal variables.

But could Stack have increased his sample size? Yes; Jain (1975) has more complete data on inequality than Stack's source, Paukert (1973), and *World Tables 1976* (IBRD, 1976:44–390) has more complete data on government spending than Russett et al. (1964). Merging these four sources, I counted 72 nations with data on both inequality and government spending. Stack (1978b:fn. 2) rejects the Jain data as unreliable. Yet, for the 27 nations in Stack's analysis which are also in Jain, inequality measures correlate .96. Apparently, if Jain's data are unreliable, then so are the data Stack used.

[4] For example, p. 883:

> In summation, we anticipate that the greater the direct government involvement in the economy

sure of employment. I can appreciate the fact that comparable, reliable employment data are unavailable for most nations; but a paper which claims to test the notion that government spending reduces inequality through its effect on employment should either include a measure of employment or modify its claims.

The omission of employment is a serious lapse. Without the proper intervening variables it is impossible to eliminate alternative explanations for an observed relationship between government spending and inequality. For example, it seems plausible that government spending could reduce inequality, not because that spending is "Keynesian" (that is, promotes employment) but, rather, because that spending is substantially financed by progressive taxation (which by definition reduces inequality).

Stack does include a measure of the other key intervening variable, rate of economic growth. But does economic growth mediate the effect of government spending on inequality? Let us forget for the moment all the flaws in the analysis and take Stack's findings at face value. Note that *in his own analysis* Stack finds that economic growth has no effect on inequality (see Table 3, here). Of the two key intervening variables in the Keynesian model, then, Stack omits one and finds that the other has no effect. Hence, it is hard to share Stack's (1978b:886) view that his "results indicate support for the (Keynesian) model."

In short, aside from the missing data problem, Stack estimates a model (1) which fails to control for demonstrated causal variables and changes dramatically when they are added; (2) which has causal effects working backward in time; (3) which ignores the likely possibility of reciprocal effects; and (4) which does not work for one of the key intervening variables in the theory and omits the other one. These shortcomings suffice for dismissal of Stack's results. This is not to say that the "Keynesian approach" to income inequality is wrong (perhaps with better data and better-executed research one could show that Keynesian policies do, indeed, reduce a nation's income inequality). But it is to say that Stack's demonstration is unconvincing and should be ignored.

Cross-sectionalism

Cross-sectionalism refers to the use of cross-sectional (particularly cross-national) data to make causal inferences (Irwin, 1977). In some cases causal inference from cross-national data is appropriate (Firebaugh, forthcoming). Unfortunately, the cross-national relationship between government spending and inequality does not appear to be such a case. Students of income inequality often are impressed by the stability of inequality over time for a given nation (e.g., Nicholson, 1970, and Rubinson, 1976:646). Income inequality in the U.S., for example, has remained relatively constant throughout this century (Turner and Starnes, 1976: chaps. 2 and 3), in spite of some rather remarkable changes in the U.S. economy during this period. One of these changes was a marked increase in government spending/GNP. Other nations exhibited similar patterns. Time-series data on government spending are available for 23 of the nations in Stack's sample. From 1950 to 1970, the government spending/GNP ratio increased for 13 of these nations, remained the same for two, and decreased for eight (IBRD, 1976:Series I). However, as Stack (1978b:883) notes, disaggregated measures of government spending are more meaningful, since certain types of government spending are more relevant to income inequality than others. Significantly, increases in Social Security spending (Wilensky, 1975: Table 2) and government nondefense consumption spending (IBRD, 1976:433) outstripped increases in GNP in industrial nations from 1950 to 1970, yet there was little if any change in income inequality in these nations during this period.

Lieberson and Hansen (1974) faced a similar antinomy in their study of language diversity and national development (measured by urbanization). In particular they found that language diversity and urbanization are negatively related for a cross-section of 109 nations. For a given nation over time, however, they found no correlation between language diversity and urbanization, since language diversity (like income inequality) is relatively constant in a nation over time. Their conclusion was that the negative cross-national relationship between language diversity and urbanization is spurious.[5] I would be inclined to a similar conclu-

---

(DGI) the less the unemployment and the less the income inequality. Also, the greater the DGI, the greater the economic growth, and the less the degree of income inequality.

[5] Their explanation for the negative cross-national relationship was as follows: a nation's language diversity depends largely on the way that nation was formed. For older nations, language was an important determinant of national boundaries; for newer nations, language has been a less important determinant. Also, economic development is related to age of nation; older nations tend to have a higher economic level. Newer nations, then, tend to be both more heterogeneous in language and less developed economically—hence, the cross-national relation-

sion about negative cross-national relationships between government spending and income inequality. Stack gives us no explanation of why we should believe his cross-sectional results when they are apparently inconsistent with trends over time; indeed, unlike Rubinson (1976), Stack does not even attempt to reconcile his cross-sectional results with the apparent time-series pattern.

Operationalization and Measurement

On the choice of measures and metrics. Different cross-national studies often employ different measures of the same underlying construct. For example, level of economic development has been variously measured by per capita GNP, per capita energy consumption, percent of labor force in the secondary or tertiary sectors, etc. How, then, does one decide which measure to use? Often, the criterion is correlation with the dependent variable; thus, Stack (1978b:884) chose per capita GNP as his measure of economic level because it had the highest correlation with income inequality. This is a poor practice.

First, correlated measurement error can inflate the observed correlations. Hence, where correlated measurement error is present (as in much cross-national research), the measure which correlates highest with the dependent variable is likely that measure whose measurement error correlates highest with the measurement error of the dependent variable. Stated differently, the measure that correlates highest with the dependent variable is likely the worst measure, not the best. Second, choosing on the basis of observed correlations seems to reduce the possibility of falsification: if I want to show a relationship between X and Y, then choosing a measure of X on the basis of its correlation with Y seems to be a handy way to guarantee success.[6]

In cross-national research, one finds not only different measures for the same construct but also the same measure for different constructs. For example, in their cross-national study of the determinants of fertility, Friedlander and Silver (1967:41) use radios per capita as a proxy for frequency of sexual intercourse (on the assumption that listening to the radio is a substitute); others have used radios to measure quite different constructs. Similarly, Stack uses government spending/GNP to measure Keynesian notions of government involvement in the economy; while Rubinson (1976) uses a similar measure, government revenue/GDP, to measure a very different construct, position in the world economy. I suspect that it is this measurement anarchy—the use of different measures for the same construct and the same measure for different constructs—which accounts in large part for the noncumulative nature of much macrolevel cross-national research in sociology.

Even where there is agreement on the measure, there may not be agreement on the metric. The problem in cross-national research is that many measures are actually ordinal, not interval. This raises the question of whether interval scale techniques, such as regression analysis, are appropriate (Kim, 1975, concludes that they are). An important related issue is that of interpreting nonlinearities in the observed relationships. In the absence of an established metric, how does one know whether observed nonlinearities reflect nonlinearities between the measures and their constructs or actual nonlinearities between the constructs? Stated differently, how do we know which nonlinearities are due to the nature of the measures and which have substantive significance?

This is not an idle question; for example, on the basis of his finding that the cross-national relationship between inequality and per capita energy consumption (*circa* 1960) is nonlinear, Jackman (1974) concludes that economic development reduces inequality to a greater extent at early stages of industrialization than at later stages. But how do we know that per capita energy consumption is a linear measure of economic development? In 1960, the gap between Belgium and Switzerland in per capita energy consumption was about the same as the gap between Switzerland and Nepal (Banks, 1971:Segment 8); but can we say that the difference between Belgium and Switzerland in economic development was about the same as that between Switzerland and Nepal? If not, then we may want to use as our measure of economic development some transformation of per capita energy consumption, such as its logarithm; and Jackman finds that the logarithm of per capita energy consumption is *linearly* related to inequality. In sum, substantive interpretations of nonlinear relationships between measures are tenuous unless one can first establish that the measures are linearly related to their underlying constructs.

---

ship between language diversity and economic development is negative, even though the two variables are unrelated over time.

[6] Since GNP, energy consumption, etc., correlate so highly, I doubt that Stack's results were affected by the measure of economic level he used. Nevertheless, I do object to his method of choosing this measure; and I felt that I should mention these objections, since this method is commonly used in cross-national research.

Inequality. Allison (1978:865) observed, "Although inequality has long been a topic of intense interest to sociologists, few have bothered to carefully specify what they mean by the term." To say that we are interested in income inequality begs a number of questions (Lampman, 1973). Are we interested in inequality merely at a given point in time—or inequality in lifetime earnings? (The former overstates the latter: Dich, 1970.) Are we interested in wage inequality or household inequality? (These are quite different; if households have different numbers of wage earners, equality across households implies inequality in wages, and conversely.) Should we adjust for size of household? If so, do we give children and adults equal weighting?

Why are we interested in cross-national variations in income inequality? In itself, inequality in *income* may not be as interesting as many sociologists think. Indeed, I suspect that what cross-national studies of income inequality are really interested in (though they usually do not state it explicitly) is inequality in standard of living, or economic inequality. However, the use of income inequality as a proxy for economic inequality can be questioned, since the relationship between income distribution and standard-of-living distribution varies substantially across nations. First, in some nations, employees (especially the best-paid employees) are paid large fringe benefits which are overlooked not only in the calculation of income tax but also in the calculation of income inequality. Second, income distributions do not take government services into account. Nations vary considerably in the extent to which governments provide or subsidize education, health care, transportation, housing, food, etc. Third, in some nations, purchasing power is determined by factors other than income. In eastern European nations, for example, certain consumer goods are chronically in short supply; waiting lists are made and, typically, political position is more important than income in determining one's position on the list.

In short, the relationship between income inequality and inequality in living standard is complex. It is also largely uncharted in sociology. Typically, we do not even know whether income inequality overstates or understates inequality in living standard; on the basis of the omission of government services from the income inequality measure, we would expect this measure to overstate economic inequality, while on the basis of its omission of salary payment-in-kind we would expect it to understate economic inequality. We can certainly assume that these factors vary across nations. And therein lies the potential danger in using income inequality as a proxy for economic inequality: to the (unknown) extent that income inequality understates (or overstates) economic inequality differently across nations, to that same extent the cross-national correlation between income inequality and economic inequality is attenuated.

Government spending and Keynes. Stack uses government spending/GNP as his "Keynesian" measure—that is, nations with a higher level of government spending/GNP are assumed to be more Keynesian than those with a lower level of government spending. A brief review of Keynesian theory will reveal that this is a misleading oversimplification.

Consider a nation's real GNP (total output), Y. For a given level of technology (T), and stock of fixed land and capital (K), total national output is determined by employment. Hence, for a particular nation at a particular point in time, there exists a unique level of maximum national output, Y_F, which can be achieved only at full employment.

Keynes departed from classical economic theory when he argued that there is nothing inherent in a market economy to guarantee that the actual output (Y) will correspond to the potential—full-employment output (Y_F). If Y is not necessarily Y_F, then what determines Y? Aggregate demand; that is, $Y = C + I + G$—where C is consumption (the total demand for all goods and services by private consumers), I is investment (the total demand for investment goods by private industry), and G is government (the demand for goods and services, both consumption and investment, by the government).[7] With C and I constant, then, an increase in government spending will increase Y. This suggests the Keynesian cure for unemployment and an underachieving economy: increase government spending enough to raise Y to Y_F.

In Keynesian theory, then, the proper level of government spending is that level which raises Y' to Y_F (where Y' is $C + I$). If the gap between Y' and Y_F is large, government spending should be high. If the gap between Y'

---

[7] In an open economy, exports and imports also enter the equation; in this brief exposition, however, there is no need to add this complication.

Some readers may wonder why, in detailing the weaknesses of Stack's operationalization of Keynesian theory, I have not pointed out that, for Keynes, government manipulation of the money supply is at least as important as government spending. This is a problem with Stack's analysis, to be sure, but it is a problem which he recognizes (Stack, 1978b:fn. 5); I have tried to limit this comment to major problems which Stack fails to mention.

and Y_F is small, however, Keynesian theory advises *against* high government spending; high government spending would be inflationary (McConnell, 1966:257–8). Government spending/GNP would be a suitable operationalization of Keynesian policies, then, only if we could assume that (Y_F-Y')/GNP is similar for all the nations in the sample. Clearly, such an assumption is false; the labor situation varies markedly from nation to nation in Stack's sample (compare, for example, India and West Germany). Hence we cannot equate Keynesian theory with high government spending, as Stack does in his operationalization of the theory.

Note, also, that Keynesian theory views government spending as a *supplement* to private consumption (C) and investment (I). The purpose of government spending is *not* to replace C + I but, rather, to raise C + I + G to the potential output, Y_F. Stack (pp. 882–3) seems to appreciate many of these points in his discussion of Keynesian theory. Surely, then, he did not *intend* to code his Keynesian measure, government spending (G)/GNP (G + I + C), as 1.0 (or nearly 1.0) for the socialist nations. Since government spending/GNP in 1959 was .39 for the United Kingdom and Sweden, and .21 for the U.S. (see Appendix, here), such a coding would imply that the socialist nations are 2½ times as Keynesian as the United Kingdom and Sweden and 5 times as Keynesian as the U.S. Surely, this is false; indeed, it seems wrong to call the socialist nations Keynesian, at all. The abolition of the private sector is not Keynesian; it is socialism.

Conclusion

No matter how you view them, Stack's results are wrong. His results depend entirely upon his coding government spending/GNP as 1.0 (or thereabouts) for the eight socialist countries in his sample. There are two interpretations for this coding. One is the assumption that it was accidental; the missing-values card was missing. The other explanation is that Stack intended this coding, thereby distorting Keynesian theory beyond recognition by implying that Keynes advocated the abolition of the private sector. Further, *even if we ignore this problem and take his data at face value,* his results do not hold under more complete model specifications. In particular, controlling for political democracy—a variable which Stack (1978a) himself insists should be controlled for—eliminated the effect of government spending on inequality. Finally, *even if we take his empirical findings as correct*—that is, even if we take his data at face value *and* assume that his model properly represents Keynesian theory—*then his conclusion is wrong;* Keynesian theory is not supported, since (1) his results show that economic growth does *not* mediate the effect of government spending on inequality and (2) he gives no evidence to eliminate the alternative hypothesis that government spending reduces inequality because it is related to redistributive taxation and not because it is related to employment.

Quantitative cross-national research is not easy; it must be carefully conceived and carefully executed. Unlike another critic of cross-national studies of income inequality (Bach, 1977), however, I do not hold that quantitative cross-national research is merely an exercise in futility. I have faith that quantitative cross-national research on inequality, properly done, can advance our understanding of this important subject.

Glenn Firebaugh
Vanderbilt University

REFERENCES

Allison, Paul D.
1978 "Measures of inequality." American Sociological Review 43:865–80.

Bach, Robert L.
1977 "Methods of analysis in the study of the world economy." American Sociological Review 42:811–4.

Banks, Arthur S.
1971 Cross-Polity Time-Series Data. Cambridge, Mass.: MIT Press.

Bornschier, Volker, Christopher Chase-Dunn, and Richard Rubinson
1978 "Cross-national evidence of the effects of foreign investment and aid on economic growth and inequality: a survey of findings and a reanalysis." American Journal of Sociology 84:651–83.

Cutright, Phillips
1967 "Inequality: a cross-national analysis." American Sociological Review 32:562–78.

Dich, Jorgen S.
1970 "On the possibility of measuring the distribution of personal income." Review of Income and Wealth 16:265–72.

Firebaugh, Glenn
Forthcoming "Cross-national versus historical regression models: conditions of equivalence in comparative analysis." Comparative Social Research, Vol. 3.

Friedlander, Stanley and Morris Silver
1967 "A quantitative study of the determinants of fertility behavior." Demography 4:30–89.

Hewitt, Christopher
1977 "The effect of political democracy and social democracy on equality in industrial societies: a cross-national comparison." American Sociological Review 42:450–64.

APPENDIX

Data for Stack's Keynesian Model

Nation	(1) Income Inequality[a] *circa* 1960	(2) (Govt. Spending)/ GNP, 1959	(3) Per Capita GNP, 1965, Dollars	(4) Ave. Annual Growth Rate in (3), 1950–1965
1. Argentina	.42	.170	770	1.3
2. Australia	.30	.180	2002	1.9
3. Bolivia	.53	.161	164	2.7
4. Brazil	.54	.137	267	2.1
5. Burma	.35[b]	.291	71	3.1
6. Bulgaria	.21[c]	—[d]	829	—
7. Czechoslovakia	.20[c]	—	1561	4.2
8. Denmark	.37	.214	2120	3.0
9. East Germany	.20[c]	—	1260	—
10. Finland	.46	.301	1749	4.1
11. France	.50	.400	1924	3.6
12. Greece	.38	.236	687	5.7
13. Hungary	.23[c]	—	1094	5.3
14. India	.33	.139	101	1.7
15. Iraq	.60	.325	231	1.9
16. Israel	.30	.382	1422	5.4
17. Italy	.40	.342	1104	4.7
18. Japan	.39	.345	861	7.8
19. Netherlands	.42	.370	1554	3.5
20. Norway	.35	.286	1890	3.2
21. Peru	.61	.131	367	2.9
22. Philippines	.48	.092	160	2.1
23. Poland	.17[c]	—	978	5.1
24. Rumania	.23[c]	—	778	8.4
25. South Africa	.58	.311	611	2.3
26. Sweden	.39	.391	2549	3.1
27. United Kingdom	.38	.388	1818	2.4
28. United States	.34	.210	3575	2.0
29. U.S.S.R.	.28[c]	—	1357	4.9
30. Venezuela	.42	.272	882	3.1
31. West Germany	.45	.306	1901	5.5
32. Yugoslavia	.26[c]	—	451	7.2

[a] Gini index. See text for data source for this and the other variables.

[b] "Of rather doubtful value" (Paukert, 1973:125).

[c] See Stack (1978b:fn. 9) for data source.

[d] Missing datum.

International Bank for Reconstruction and Development (IBRD)
1976 World Tables, 1976. Washington, D.C.: IBRD.

Irwin, Patrick H.
1977 "Cross-sectionalism: mismatching theory and model" (Comment on Rubinson, 1976). American Sociological Review 42:814–7.

Jackman, Robert W.
1974 "Political democracy and social equality: a comparative analysis." American Sociological Review 39:29–45.

Jain, Shail
1975 Size Distribution of Income: A Compilation of Data. Washington, D.C.: International Bank for Reconstruction and Development.

Kerr, Clark, J. T. Dunlop, Frederik Harbison and Charles Myers
1964 Industrialism and Industrial Man. New York: Oxford University Press.

Kim, Jae-On
1975 "Multivariate analysis of ordinal variables." American Journal of Sociology 81:261–98.

Lampman, Robert J.
1973 "Measured inequality of income: What does it mean and what can it tell us?" Annals of the American Academy of Political and Social Science 409:81–91.

Lenski, Gerhard
1966 Power and Privilege. New York: McGraw-Hill.

Lieberson, Stanley and Lynn K. Hansen
1974 "National development, mother tongue diversity, and the comparative study of nations." American Sociological Review 39:523–41.

McConnell, Campbell R.
1966 Economics: Principles, Problems, and Policies. New York: McGraw-Hill.

Nicholson, J. L.
1970 "Redistribution of income—notes on some problems and puzzles." Review of Income and Wealth 16:273–8.
Olsen, Marvin and Glenn Firebaugh
1975 "Transforming socioeconomic modernization into national political development." Paper presented at the annual meeting of the American Sociological Association, San Francisco.
Paukert, Felix
1973 "Income distribution of different levels of development: a survey of evidence." International Labour Review 108:97–125.
Rubinson, Richard
1976 "The world economy and the distribution of income within states: a cross-national study." American Sociological Review 41:638–59.
Rubinson, Richard and Dan Quinlan
1977 "Democracy and social inequality: a reanalysis." American Sociological Review 42:611–23.
Russett, Bruce M., Hayward R. Alker, Jr., Karl W. Deutsch, and Harold D. Lasswell
1964 World Handbook of Political and Social Indicators. New Haven: Yale University Press.
Stack, Steven
1978a "Internal political organization and the world economy of income inequality" (Comment on Rubinson, 1976). American Sociological Review 43:271–2.
1978b "The effect of direct government involvement in the economy on the degree of income inequality: a cross-national study." American Sociological Review 43:880–8.
Turner, Jonathan H. and Charles E. Starnes
1976 Inequality: Privilege and Poverty in America. Pacific Palisades, Calif.: Goodyear.
Wilensky, Harold L.
1975 The Welfare State and Equality. Berkeley: University of California Press.

DIRECT GOVERNMENT INVOLVEMENT IN THE ECONOMY: THEORETICAL AND EMPIRICAL EXTENSIONS

REPLY TO JACKMAN AND FIREBAUGH*

Jackman's main criticisms are (1) a Keynesian model of DGI is inappropriate for the socialist nations; (2) the estimates of DGI for the socialist nations are too high; (3) the income inequality data for the socialist nations underestimate their inequality levels and are not comparable to those of the capitalist nations; (4) if we restrict the sample to the capitalist nations, the model no longer fits the data.

Firebaugh reiterates many of Jackman's criticisms and adds several important ones, including the notion that the model is misspecified. These criticisms, on the surface, may seem to have serious and negative consequences for the DGI model. However, these criticisms tend to be premature and are sometimes marked by errors and fabrications. First, for example, while a strictly *Keynesian* model of DGI may be inappropriate for socialist nations, the umbrella of the general DGI argument certainly covers the socialist case.[1] Second, if we use lower, alternative estimates of DGI, we obtain the same results. Third, if we use the most comparable income inequality data, which maximize the degree of estimated socialist inequality, we still find that the model is correct. Fourth, if we draw a larger, more representative sample of capitalist nations, the DGI is again supported by the analysis. Finally, Firebaugh's criticisms have a number of problems associated with them—such as his alternative specifications, all of which include a measure of political participation that introduces a severe bias against nonwestern systems making the analysis meaningless.

This reply suggests that the comments of the critics are largely inconsequential to the basic validity of the DGI model.

SOCIALIST DGI THEORY

Jackman's long argument that Keynesianism is nonapplicable to eastern Europe is largely irrelevant to the *general* DGI model, as is Firebaugh's statement that it seems wrong to call the Communist bloc nations Keynesian at all. Both make the mistake of limiting DGI theory to Keynesianism. DGI mechanisms can include much more than simple government spending, especially in the case of command socialism. While my original article stressed Keynesian DGI mechanisms, it does not take much imagination to extend the argument to DGI mechanisms in socialism. For example, there is a sound consensus (in the field of comparative economics) that DGI is substantially higher in socialist nations (Leeman, 1977; Holesovsky, 1977). The market is brought under human control through the nationalization of the means of production and the centralization of economic power. This centralized command over economic resources increases the probability of solving the basic "Keynesian" problem of disequilibrium between savings, consumption, and investment. Consequences of such an economic equilibrium relevant to the study of income inequality include full employment and high economic growth rates.

---

* Direct all communications to: Steven Stack; Department of Sociology; Indiana University; Indianapolis, Indiana 46202.

[1] This point was made, although briefly, in footnote 9 of my original paper (Stack, 1978a). In addition, however, the notion that Keynesian analysis is irrelevant to socialist systems is an exaggeration. Jackman ignores the substantial relevant literature on the convergence thesis. The literature on this concept has included works by such western authors as Tinbergen, Galbraith, and Wiles—as well as by such socialist thinkers as Bregel, Dalin, and Vajda (Millar, 1972). To the extent that the systems are converging, we would anticipate that socialist backsliding towards market mechanisms might have some relevance for Keynesian economics.

I would like to take this opportunity to correct two errors in my original article. On page 880, line 7, this phrase should be inserted: political development theory (Lenski, 1966; Cutright, 1967; Peters, 1973; Hewitt, 1977; Rubinson and Quinlan, 1977). Also, in Table 2, under computed value of Student's t, the two asterisks next to the value −.221 for the variable EG should be removed.

First, there is the notion that, under conditions of centralized planning, unemployment should be low or nonexistent. In contrast to capitalism—where there is often an imbalance between savings and investment, since the relevant decisions are made by many private individuals—there is coordination of such decisions in socialism. Since investment is financed from state revenue, and since the level and distribution of investment is state-determined, there is an identity of savings decisions and investment decisions, not a separation of them (Dobb, 1960; Nuti, 1978).[2] In addition, consumption levels also are manipulated from the same command centers. As Bornstein (1978) points out, the level of aggregate demand is normally kept high through ambitious national planning. One consequence of this coordination of the Keynesian categories of savings, investment, and consumption is full employment. While there are fluctuations in economic activity resulting primarily from changes in growth rates of investment and output, these are handled in such a way as to preclude periodic recessions in which workers are laid off (Bajt, 1971). On the microlevel, when a worker is no longer needed because of such considerations as automation, the management is typically expected—or even legally obligated—to reassign him/her, with retraining if necessary, to another department, or to another firm in the same area (Bornstein, 1978). The low rate of unemployment in the command socialist nations, then, is one way in which socialist DGI is responsible for less income inequality.

Socialism eliminates one of the causes of low wages found in capitalism. In the literature on the causes of poverty and the dual economy (Wachtel, 1974), it is often argued that capitalism has no way of effectively transferring part of the high incomes of the advanced technological sector (e.g., heavy industry) of the economy to augment the low incomes being made in the nontechnological sector (e.g., services). Capitalism maintains inequality by its inability to disrupt the dual economy of high-paid workers in the technological sector and low-paid workers in the nontechnological sector. In contrast, in command socialism, given the centralization of economic authority, the gains from productivity in the technological sector can be redistributed. They are often used to promote the general wage level of all workers in the society, as well as to reduce prices and to expand the social consumption fund (Wilczynski, 1977:112). This redistribution lessens inequality.

There are additional links between socialist DGI mechanisms and reduced income stratification. As Parkin (1971) points out, wage differentials are set by national wages boards. This is in contrast to capitalism wherein many individual enterprises set incomes in accordance with a variety of forces. The centralized authority of the state replaces, to an extent, the supply and demand mechanism of the market in capitalism. Given the egalitarian nature of socialist wages ideology, this centralized power tends to be used to decrease wage differentials, in the long run. For example, in the Soviet Union in recent years, the ratio of the average income of the highest-skilled workers to lowest-skilled workers has decreased from 3.68 to 3.2 in ferrous metals, from 2.7 to 2.0 in the petroleum industry, and from 2.71 to 2.0 in the machine construction sector (Kirsch, 1972). Other national wage policies designed to reduce inequality include increasing the minimum wage faster than the mean wage level. For example, in East Germany between 1971–1973, the minimum monthly wage increased from 325 to 392 marks (or by 20.6%) while the average wage increased by only 10.6% (Bruckner, 1974). The most detailed study of such wage policies concluded that

---

[2] Liberal economic theory takes issue with this sharp distinction (Galbraith, 1967). To the extent that corporations finance their investments internally, and not through the big banks, Dobb's distinction is overdrawn. Galbraith's notion of the growing independence of the corporation from finance capital would support the convergence thesis of economic systems (Tinbergen, 1961. However, there is still imperfect coordination of savings and investment in the "new industrial state," in the sense of the lack of coordination of investment ventures *among* individual corporations.

Yanowitch (1969) may be correct when he spoke of socialist "income revolutions" (Kirsch, 1972:184).

Through eliminating private property, socialism largely abolishes the sources of several types of income that are received by a privileged few in capitalism: capital gains, dividends, and rent. Taking the eight socialist nations in this study as a whole, 92% of the farming land is in the socialist sector, and 95% of all national income is contributed by this sector (Wilczynski, 1977:22).[3] The nationalization of the means of production reduces inequality through eliminating incomes from property ownership.[4]

MEASURING SOCIALIST DGI

Jackman has several criticisms of the socialist DGI measures. He states that they are "guesses," that they are probably too high, and that (this, he implies) alternative data might yield different results. Firebaugh states that such high values are "surely false." Firebaugh attributes this "mistake" to the possibility that I left out a missing-values card in my SPSS program. This, incidentally, would have been hard for me to do since I did not even use SPSS. I used COSAP, instead. The small computer I had at my disposal would not handle SPSS. COSAP does not have a procedure for handling missing values.

Data from the United Nations (1978) were incorporated into the estimation procedure for the socialist nations. For example, for Czechoslovakia, only 1,873 million koruny of primary income came from the private sector out of 172,304 million koruny of net material product. To this amount one can add 6,138 million koruny from the personal agricultural plots of households. Private sector income totaled less than 4% of net material product. A DGI figure in the high nineties does not seem "crippling" (as Jackman states) or "ridiculous" (as Firebaugh contends). For Hungary, the total private sector income amounted to less than 2% of total net material product.

Jackman's central basis for criticizing my DGI figures (Firebaugh provides no hard evidence) are some estimates of DGI taken from a CIA study cited by Grossman (1977). These CIA data place the Soviet DGI level at 90%. Jackman implies that there is evidence of it being even higher in other eastern European nations. Grossman (1977:40), however, states only that the "scope" (types of private activities allowed) of private activity is probably wider in other socialist nations. In addition, Grossman gives no evidence supporting this remark.

Other sources of data tend to support my estimates rather than those of the CIA and Grossman. For example, Wilczynski (1977:68) gives the following estimates of socialist DGI: USSR, 96; Hungary, 97; Czechoslovakia, 95; Romania, 95; Yugoslavia, 76; and East Germany, 94. Do these alternative data yield different results? Table 1 gives the results of the regression analysis using Wilczynski's estimates of socialist DGI. The results are largely the same. DGI is still the most important determinant of inequality. GNP/Capita still exerts a significant effect. Economic growth still does not. The amount of variance explained by the model drops by only a few percentage points.

The results shown in Table 1 should be taken with some caution, as Jackman correctly points out that there are differences between socialist and capitalist techniques of national income accounting. Indeed, estimates of socialist GNP are usually 10 to 20% *less* than what they would be if using western methods of calculation. One way of checking the results of Table 1 is to collapse DGI to a dummy variable, wherein capitalist nations receive a score of zero and socialist nations receive a score of one. This categorization seems to be in order, since literally all scholars of comparative economics agree that there is far more DGI in socialist nations than in capitalist nations. The results of this regression are given in Table 2. Measuring DGI in this fashion does not change the basic results of my original article, either. Again, the results are almost exactly the same. In addition, even more of the variance is

---

[3] Jackman exaggerates the importance of the private sector in socialism. Most activity in the private sector is petty bourgeois, in form, wherein a person operates a small enterprise, such as handicrafts, as an owner-worker, and has no employees. However, the private hiring of labor, while exceptional, does exist. In Poland, for example, there is a total of 200,000 persons (or 2% of the labor force) working as employees in the private sector. However, this activity is carefully regulated and held in check through DGI by such mechanisms as heavy taxation and such rules as the requirement of employers to work alongside their employees (Wilczynski, 1977:99).

[4] Additional reasons as to why we would anticipate that socialism would reduce inequality are not as closely related to the DGI model. Examples include the notion that, because socialist elites are highly proletarianized, policy will favor the lower classes and less inequality; because people (the central wages board), not the market, are responsible for one's income, the state is more apt to maintain low inequality in anticipation of discontent among the masses if it did otherwise; and, because elites are obliged to adhere to egalitarian wage policy because, from the standpoint of traditional socialist ideology, this is expected (Parkin, 1971; Stack, 1976:152–80).

Table 1. Effects of DGI, GNP/Capita, and the Rate of Economic Growth on the Degree of Income Inequality (Using Wilczynski's Estimates of Socialist DGI) (N = 32)

Variable	Regression Coefficient	Standard Error of Coefficient	Computed Value of Student's T	Beta Coefficient
DGI	−0.003	0.001	−4.589**	−0.694
GNP/cap	−0.000	0.000	−1.800*	−0.219
EG	−0.004	0.010	−0.377	−0.057
Intercept	.549218			

$R^2 = .59.$
$\bar{R}^2 = .55.$
F = 13.5, 3/28 df.
** = statistically significant at the .005 level.
* = statistically significant at the .05 level.

explained. These findings lend support to those who hold that socialism is the main determinant of destratification (Parkin, 1971; Stack, 1976).

SOCIALIST INEQUALITY

Jackman holds that the data on income distribution in the socialist nations are not comparable to those for the capitalist nations. At one point, he asserts that the data I use overstate the degree of equality in the socialist countries. The opposite is probably more accurate. He makes no mention of the fact that all of these data will, simply by definition, make the socialist nations look comparatively inegalitarian when compared with similar data from the capitalist nations. The income data from the capitalist nations routinely omit major sources of property income such as capital gains. Data from the capitalist nations exaggerate the degree of equality. Since property income is virtually eliminated in socialism, if all *else* is equal, socialist nations will have substantially less inequality. As Lundberg (1969) points out, property-based income is one of the backbones of inequality in capitalist nations. For those with incomes of over one million a year, approximately 90% comes from property, not wages. The importance of property income in maintaining inequality can be measured to an extent by comparing the degree of inequality before a Communist take-over with that after the rise of Communists to power. For example, the degree of inequality in Yugoslavia, in 1938, was .352 compared with levels in the .250–.280 range in the 1960s (Vinsi, 1967; Michal, 1973).

I am puzzled over Jackman's error wherein he senses discrepancies between the U.N. data and Michal's (1973) data. The problem is that he selects inappropriate U.N. data (United Nations, 1967:Table 9.20). The relevant discussion on these data (United Nations, 1967, chap. 9:24–6) notes that they refer to the per capita distribution of income in Hungary and Poland. These are not the kinds of data we want for our investigation. This is why Jain (1974) used data from another part of the book (United Nations, 1967, chap. 8:71) for his calculations on measuring inequality. For Hungary, these estimates are .229 (1955), .202 (1958), and .208 (1964). These more appropriate U.N. data are, in fact, well in line with Michal's estimates of .226 (1960) and .240 (1964). If anything, the Michal data provide more conservative estimates of socialist equality. If Jackman had used the appropriate U.N. tables, he would have found the opposite of

Table 2. The Effects of DGI, GNP/Capita and the Rate of Economic Growth on the Degree of Income Inequality with DGI Measured as a Dummy Variable (N = 32)

Variable	Regression Coefficient	Standard Error of Coefficient	Computed Value of Student's T	Beta Coefficient
DGI	−0.195	0.036	−5.418**	−0.721
GNP/cap	−0.000	0.000	−2.786*	−0.312
EG	−0.006	0.009	−0.652	−0.087
Intercept	.501963			

See notes to Table 1.
$R^2 = .65.$
$\bar{R}^2 = .61.$
F = 17.4, 3/28 df.

what he asserts. I was certainly not out to select data to make the socialist countries appear less unequal than they are. For the same reasons, Jackman's comment on Poland is also an error.[5]

In the case of the data for Czechoslovakia, the U.N. (Table 9.19) provides a quartile ratio of 1.62 for 1959. I do not see what basis Jackman has for asserting that this is at odds with the Michal data, since Michal does not even provide the data necessary for such a comparison. Furthermore, the Ginis based on the U.N. data are between .188 and .200 (Jain, 1974). The Michal estimates are between .195 and .199. Again, Jackman's criticisms seem to be based on some misunderstanding of the data involved.

In an unnecessary fabrication, Jackman discusses the pitfalls of my supposed use of data based on a single city in Poland—Lodz—to estimate inequality in the country, as a whole. I simply did not use these data (Szafnicki, 1971:39). Instead, I used the more appropriate data for Poland as a whole (Szafnicki, 1971:43). The Gini for Lodz was .166 while the Gini for Poland was .234. Jackman appears to be guessing which data I used. His guesses are wrong ones.

With respect to the data for Hungary, Jackman implies that they are not representative of the entire income distribution since they apply to state sector employees. He provides no evidence for this assertion. However, available information (on the degree to which the degree of inequality in the state sector is a reliable approximation of overall inequality) indicates that the former is a good predictor of the latter. For example, McAuley's (1977) estimates of the total degree of inequality in the USSR are decile ratios (D9/D1) between 3.04 and 3.08. These can be compared with his decile ratio for the state sector alone which is 3.00. While there is a slight difference in these figures, I do not see it as "undermining" the results of the investigation! We might note that the corresponding decile ratio for the United States is approximately 14.0 (Kolko, 1970:14). Nevertheless, if we omit Hungary from the analysis—as I did in a separate analysis—the results support the model just as solidly as before.

Still, Jackman is correct in stating that there is some variability in the estimates of socialist inequality. One way of assessing whether or not this variation "undermines" our results is to use the estimates (excluding the noncomparable data on the per capita income distribution) that provide the highest degree of income inequality for the socialist nations for the 1959–1965 period. In this manner, we are probably exaggerating the degree of socialist inequality. For example, we will use the estimate of .244 for Bulgaria instead of the original approximation of .215.[6] As a further precaution, we will use Wilczynski's (1977) estimates of socialist DGI. The results of the regression analysis using the maximum Ginis for the socialist nations are given in Table 3. Even with these exaggerated estimates of socialist inequality, the results of the analysis are nearly the same. DGI is again the most important variable. The level of development is significant while the rate of growth is not. The model explains almost as much of the variance (53%) as in the original work (57%).

CAPITALIST DGI

The final criticism of Jackman is that if we restrict the DGI model to capitalist nations, DGI and inequality are not significantly related. However, he provides no rationale about why this should be the case. In particular, should we attempt to reformulate DGI theory?

---

[5] Again, these data are from the wrong table (United Nations, 1967, Table 9.20) which provides data on the per capita distribution. Jackman should inspect other data (United Nations, 1967, chap. 8: 71). The estimates of inequality for Poland, based on these data, are between .252 and .253 (Jain, 1974). The estimate from Michal's work is exactly the same: .253.

Firebaugh also makes numerous errors and/or guesses in his handling of income inequality data. For example, he uses inappropriate estimates for Poland and Romania consisting, respectively, of .17 and .23 (see his Appendix). His assertion that I apparently "overlooked" comparable data on some 48 nations is quite inaccurate. The data in the Jain source, as Jain cautions, are marked by many serious problems of comparability. While Firebaugh reports a correlation of .96 between the Ginis of approximately two dozen nations which are available in both the Paukert and Jain sources, this is not surprising and does not necessarily mean that the Jain data are reliable. For these two dozen nations, Paukert and Jain used many of the same data sources! As Jain (1974:1) states:

> In preparing this compilation we have been greatly reinforced in our reservations on the accuracy of such data. . . . The reader is therefore cautioned that the data reported in this paper are not in any sense presented as reliable or as best estimates.

This is why I restricted my capitalist sample to the data adjusted by Paukert (1973).

[6] The data for Bulgaria are taken from Jain's calculations which were based on the appropriate United Nations data (United Nations, 1967, chap. 8:71). In my original article, I neglected to give the source (Grindea, 1974) of the data on Romania.

Table 3. Effects of DGI, GNP/Capita and Economic Growth on Income Inequality (Using Maximum Ginis for the Socialist Nations (N = 32)

Variable	Regression Coefficient	Standard Error of Coefficient	Computed Value of Student's T	Beta Coefficient
DGI	−0.003	0.001	−4.425**	−0.679
GNP/cap	−0.000	0.000	−1.863*	−0.230
EG	−0.004	0.010	−0.398	−0.061
Intercept	.547687			

See notes to Table 1.
$R^2 = .58$.
$\bar{R}^2 = .53$.
F = 12.8, 3/28 df.

If so, how so? Is the DGI/full employment connection deficient? Or perhaps we should, as I indicated in my paper, focus on the type of government spending and not just on the quantity (Stack, 1978a:883). Jackman does not engage in any theoretical discussion of why the DGI model may not work for the capitalist sample. Instead, he seems interested only in using the results of his "quickie" empiricism to discredit and disprove the DGI model. In similar fashion, in his Table 1, Firebaugh notes a discrepancy in the correlations between DGI and inequality.[7]

The need for theoretical reformulation of DGI theory, however, may be premature. First, Jackman's own analysis fails even to mention a problem of near severe multicollinearity, which makes the results of his regression analysis quite tenuous. Jackman, as does Firebaugh, fails to report the zero-order correlation between capitalist DGI and the level of development (r = .43). According to the Klein test for severe multicollinearity, if rij > R, severe multicollinearity is present, and the results of regression analysis are meaningless (Dutta, 1975:150). Jackman's adjusted multiple correlation is .46, indicating the near presence of severe multicollinearity. His results are to be taken with great caution.

I recently came across some data which allow us to explore the capitalist DGI/inequality relationship for a much larger sample of 53 capitalist nations.[8] The zero-order correlation between DGI and inequality is significant and in the expected direction ($r = -.30$, $p < .025$). This was not the case in Jackman's relatively small sample of 22 nations. The findings for the larger sample support the DGI model. Unfortunately, we were unable to engage in a meaningful regression analysis of the effect of capitalist DGI on inequality because there was a severe problem of multicollinearity between level of economic development and DGI. For this reason, great caution should be exercised in further investigations on the subject. We suggest that DGI and development be analyzed separately.

MODEL SPECIFICATION

Firebaugh holds that a major problem in my work is that the model is misspecified—that is, if one adds a measure of political democracy to the analysis, the DGI/inequality relationship vanishes whether we use my data or his "corrected" data which omit all of the socialist nations from all calculations involving DGI. He sees my omission of an index of politics as "strange" since in my work on inequality in *capitalist* nations I have stressed the need for such an index (Stack, 1978c). There are two devastating problems with Firebaugh's naive forcing of his western measure of democracy into the analysis.

First, being a western measure it discriminates against one-party political systems. Second, even if it did not discriminate against such systems, there is such a severe problem of multicollinearity between it and DGI that the results of the regression analysis are meaningless. Firebaugh's index uses a measure of party competition, based on the number of parties, in which the higher the number of parties, the higher the competition and democracy. Because all of the socialist nations have only one party, it is then assumed that they are, therefore, low in democracy. Problems associated with this index include the notion that there can be competing factions within a single party. Firebaugh's index of democracy also contains an index of electoral irregularity based on Mackenzie's notions of what constitutes "regular" (e.g. honest, competent administration to run the elections) elections (Taylor and Hudson, 1972:23–5). As Taylor and Hudson (1972:25) point out, this index has been under attack for some time. Critics hold that the Mackenzie criteria are not relevant to nonwestern systems and that the coding is uncritically biased against the socialist nations. It seems quite irresponsible of Firebaugh not to mention these debates.

---

[7] Likewise, by omitting all socialist nations from all regression calculations involving DGI and income inequality (the SPSS pairwise deletion of missing-data procedure), Firebaugh finds no evidence in his Table 3 for the DGI theory as it applies to capitalist nations.

[8] The data on DGI are taken from World Bank (1976:440–6). They refer to total government expenditures as a percent of gross domestic product. The data on income inequality are taken from Paukert (1973).

The second problem with the Firebaugh index is that the socialist nations all have very low scores on it—just as they all have very high scores on the DGI index. Statistically, the two scales measure the same thing, multicollinearity is present, and the results of all three of Firebaugh's new "specifications" are meaningless. I did not include an index of political democracy because I could not find one that avoided the problems of bias against the communist nations and the problem of multicollinearity. The class composition of parliament and/or elites—such as the politiburos and cabinets—avoids the first problem; but, because communist political bodies are far more representative of the lower class than are capitalist bodies, the DGI and class-composition variables are marked by the problem of severe multicollinearity (Stack, 1976).

Firebaugh then proposes a second respecification, including not only his invalid measure of politics but also an invalid measure of state strength in world economy. In a rather sloppy fashion, he borrows, from his ASA conference paper, his index of political performance, using it to measure state strength in world economy (Olsen and Firebaugh, 1975:8). His index of political performance has essentially nothing to do with a state's power in world economy. His index measures four dimensions of political performance: the degree of civil violence; legislative effectiveness, which has to do with the extent to which the legislature's power is independent from that of the executive branch; the degree of modernity in the administrative branch; and government size, which is measured as government expenditures per capita (Olsen and Firebaugh, 1975:8). As I have pointed out elsewhere, in a work Firebaugh claims to have read, state strength in world economy needs to be measured in terms of tariff and trade policies—or in terms of other relevant indices, such as the rules and regulations controlling foreign capital in a nation (Stack, 1978c:271). Far from satisfying this criticism, Firebaugh creates a useless index of state strength. Firebaugh's index fails to distinguish between two very different variables: state strength in world economy, and political performance.[9]

CONCLUSION

Jackman's criticisms are largely restricted to the problem of measurement errors. If we address this problem where it, in fact, exists—by substituting the most comparable and appropriate data—the DGI model is still supported. Jackman's one theoretical criticism (that a Keynesian model is inappropriate for socialist economies) is based on a misunderstanding of DGI theory which can draw on a number of perspectives besides Keynesianism. Relevant DGI mechanisms in socialism, for example, include the coordination of savings and investment decisions, the redistribution of income gains from the technological sector to the nontechnological sector, and the manipulation of wage rates by national wages boards. All of these DGI mechanisms are lacking in capitalism. All are associated with the abolition of private corporate property and "ridiculous" levels of government spending in the high nineties. One must remember that government spending is just *one* example of a DGI mechanism. Jackman's critique has serious problems associated with it, and one wonders if my own critical review of Jackman's work may have motivated his hasty criticisms (Stack, 1978b).

---

[9] Firebaugh raises a number of other criticisms which have numerous problems. While some of the effects of the model work backward in time if we substitute available data for alternate years, such as that for GNP, we find the same results. In cross-national research, the inavailability of data often leads to this problem. Indeed, Firebaugh's own alternative specifications have this problem (e.g., he uses an index of electoral irregularity which is *circa* 1965 and which comes after the *circa* 1960 Ginis). Other problems are germane to much research. These problems include a lack of investigation of reciprocal effects and a truly systematic measure of economic inequality. With respect to the latter problem, income researchers might feel better about their supposedly "primitive" attempts to measure the concept of inequality if they read Firebaugh's own work which uses passenger cars per capita to measure income distribution (Olsen and Firebaugh, 1975:9)! Firebaugh's criticism that I omit the major Keynesian variable of unemployment from my discussion of DGI theory suggests to me that he fell asleep while reading page 883 of my original article. I find Firebaugh's cursory view of 1950–1970 trends in inequality and DGI unconvincing. His largely unsubstantiated premise that income inequality is a constant of sorts which defies changes in societal organization does not fit well with much available data. For example, data from the Japanese ministry of finance (personal communication) indicate that inequality fell from .346 in 1955 to .281 in 1972. Jain's data for Finland indicate an increase in inequality from .395 in 1952 to .454 in 1962. In addition, the delineation of a simple bivariate relationship, over time, should be supplemented with control variables to unmask possible spurious and suppressed relationship. One of Firebaugh's concluding remarks—that I give no evidence that redistribution occurs because of taxation, not because of full employment policy—is quite a blunder, since any income inequality researcher knows that all of the relevant data refer to the pretax distribution of incomes. Hence, the variation in inequality in the data is necessarily due to factors outside the tax policies of the nations involved.

Firebaugh's claims about missing-value cards are simply erroneous rumors. His "corrected" data which he presents in his tables are to be viewed with great caution because they leave all the socialist nations out of all the critical DGI calculations. In a similar fashion, his alternative specifications are meaningless—given such considerations as their use of biased measures of democracy, which are inappropriate for a sample containing socialist nations. Firebaugh's fabrications include his paragraphs on how I failed to deal with the key Keynesian variable of unemployment. Many of his other criticisms, such as "cross-sectionalism"—failure to test the model with unavailable time-series data—and his long tangents on needed refinements in measuring economic inequality (even though the needed data are not available) might be viewed as impossible dreaming.

In general, I found both critics to be preoccupied with attempts to discredit the new DGI approach. Their resort to such minor means as fabrications, guesses, mundane empirical problems, inappropriate data analyses, and countertheoretical arguments serves only to reveal their ignorance of how DGI theory can be applied to socialist economic systems.

Steven Stack
Indiana University

REFERENCES

Bajt, A.
1971 "Investment cycles in European socialist economies." Journal of Economic Literature 9:53–63.

Bornstein, Morris
1978 "Unemployment in capitalist regulated market economies and socialist centrally planned economies." American Economic Review 68:38–43.

Bruckner, Alfred
1974 Introducing the GDR. Dresden: Volkerfreundschaft.

Dobb, M. H.
1960 An Essay on Economic Growth and Planning. London: Routledge.

Dutta, M.
1975 Econometric Methods. Cincinnati: South Western.

Galbraith, J. K.
1967 The New Industrial State. New York: Signet.

Grindea, Daniel
1974 "Theoretical and practical aspects of the distribution of national income in Romania." Kansantaloudelliner Aikakauskirja 70:333–5.

Grossman, Gregory
1977 "The 'Second Economy' of the USSR." Problems of Communism 26:25–40.

Holesovsky, Vaclav
1977 Economic Systems: Analysis and Comparison. New York: McGraw-Hill.

Jain, Shail
1974 Size Distribution of Income: Compilation of Data. Washington: International Bank for Reconstruction and Development.

Kirsch, Leonard
1972 Soviet Wages. Cambridge: MIT Press.

Kolko, Gabriel
1970 Wealth and Power in America, New York: Praeger.

Leeman, Wayne
1977 Centralized and Decentralized Economic Systems. Chicago: Rand McNally.

Lundberg, Ferdinand
1969 The Rich and Super-Rich. New York: Bantam.

McAuley, A.
1977 "The distribution of earnings and income in the Soviet Union." Soviet Studies 29:214–37.

Michal, Jan
1973 "Income distribution of earnings and household incomes in small socialist nations." Review of Income and Wealth 19:407–28.

Millar, J. R.
1972 "On the theory and measurement of economic convergence." Quarterly Review of Economics and Business 12:87–97.

Nuti, D. M.
1978 "Investment, interest, and degree of centralization in Maurice Dobb's theory of the socialist economy." Cambridge Journal of Economics 2:191–202.

Olsen, Marvin E. and Glenn Firebaugh
1975 "Transforming socioeconomic modernization into national political development." Paper read at the annual meeting of the American Sociological Association, San Francisco.

Parkin, Frank
1971 Class Inequality and the Political Order. New York: Praeger.

Paukert, Felix
1973 "Income distribution at different levels of development: a survey of evidence." International Labor Review 108:97–125.

Stack, Steven
1976 Inequality in Industrial Society: Income Distribution in Capitalist and Socialist Na-

tions. Ph.D. dissertation, Department of Sociology, University of Connecticut.
1978a "The effects of direct government involvement in the economy on the degree of income inequality: a cross-national study." American Sociological Review 43:880–8.
1978b "Review of Jackman's Politics and Social Equality: A Comparative Analysis." Contemporary Sociology 7:220–2.
1978c "Internal political organization and the world economy of income inequality (Comment on Rubinson, 1976)." American Sociological Review 43:271–2.
Szafnicki, Krzysztof
1971 "Distribution of wages and income among individuals and groups." Polish Sociological Bulletin 2:32–47.
Taylor, Charles and Michael Hudson
1972 World Handbook of Political and Social Indicators. New Haven: Yale University Press.
Tinbergen, J.
1961 "Do Communist and free economies show a converging pattern?" Soviet Studies 11:333–41.
United Nations
1967 Economic Survey of Europe in 1965, Part II: Incomes in Postwar Europe: A Study of Politics, Growth and Distribution, Geneva.
1978 Yearbook of National Accounts Statistics, Vol. 1. Individual Country Data. New York: United Nations.
Vinsi, Ivo
1967 "The distribution of national income by social classes in 1938." Review of Income and Wealth 13:259–81.
Wachtel, Howard
1974 "Looking at poverty from radical, conservative, and liberal perspectives." Pp. 180–90 in Pamela Roby (ed.), The Poverty Establishment. Englewood Cliffs: Prentice-Hall.
Wilczynski, J.
1977 The Economics of Socialism. London: George Allen and Unwin.
World Bank
1976 World Tables, 1976. Washington, D.C.: International Bank for Reconstruction and Development.
Yanowitch, Murray
1969 "The Soviet income revolution." Pp. 143–54 in Celia Heller (ed.), Structured Social Inequality. New York: Macmillan.

SOCIOBIOLOGY AND PARADIGMS IN EVOLUTIONARY THEORY

(COMMENT ON QUADAGNO, ASR, FEBRUARY, 1979*

While Dr. Quadagno's (1979:100–8) paper on evolutionary theory and sociobiology is a useful contribution to the growing literature on the subject, we find several of her major arguments to be either in error or somewhat misleading. Similarly, the conclusion drawn from her analysis seems to be severe and not justified by the arguments presented in her paper. First, we will examine alleged errors in the logical structure of sociobiological theory.

Tautology and Evolutionary Thought

Quadagno maintains (p. 104) that sociobiologists introduced tautological reasoning into the evolutionary model. This is a somewhat surprising charge coming from someone apparently knowledgeable in the history of Darwinian thought. It is not clear how sociobiology utilizes any tautology not present in Darwin's original formulation. Quadagno mentions (p. 102) that Darwin adopted Spencer's term, "survival of the fittest," in later editions of the *Origin of Species*. It is interesting that Quadagno considers the sociobiological concept, "maximization of inclusive fitness," (Alexander, 1975; Barash, 1977; Wilson, 1975) to be tautological, and yet does not find "survival of the fittest" to be equally so.

A tautology can be derived from Darwin's and Spencer's phrase: survival is the criterion of fitness and fitness results in survival (Hull, 1974). If, in turn, Darwin's notion is tautological, the implication is that not only sociobiology can be rejected as logically inadequate, but all of Darwinian evolution as well.

The charge of tautology has been leveled at Darwinian theory, even very recently (Gould, 1976). If fitness in Darwin's sense is defined in terms of survival, the theory is reduced to a tautology. However, assertions of Darwinian fitness must always be made in reference to an organism or trait in the context of a changing environment (Hull, 1974:67; Scriven, 1971:215). Gould (1976:26–30) argues that survival is an expression of a superior a priori trait and that it is the improved trait that is the independent criterion of fitness in Darwin's sense, not survival per se (cf. Dobzhansky, 1955). As Mayr (1976:13) notes, the possession of certain genetic characteristics determines evolutionary success, in that an individual with those characteristics is more likely to reproduce than those without them. Thus, "the process of natural selection can be thought of as the comparison of variant structures in terms

---

*Direct all communications to: Susan Bolton Bolin, Dept. of Zoology; or Robert Bolin; Dept. of Sociology; North Dakota State University; Fargo, North Dakota 58105.

We would like to thank J. Frank Cassel and an anonymous *ASR* reviewer for reviewing earlier drafts of this comment.

of their survival value and choosing the best" (McCleery, 1978:382).

But if Darwin's formulation is not truly tautological, what of sociobiology? In order to assess whether sociobiology rests on a tautology, some clarification of "fitness" is necessary. Darwin defined fitness as being well adapted and any trait that helped an organism to survive increased its fitness (Mayr, 1976:74). However, sociobiologists and population geneticists do not use fitness in this very broad sense.

Mathematical geneticists Wright, Haldane, and Fisher redefined fitness to mean the contribution an individual makes to the gene pool (Mayr, 1976:316). According to Mayr (1976:74), "[f]itness under the new definition can be due either to the superior fitness in the Darwinian sense or to reproductive advantage that does not add to the adaptedness of the species." Adaptiveness, therefore, should not be equated with fitness in the genetic sense. An adaptation is any genetically determined characteristic that increases an organism's ability to cope with its environment (Ricklefs, 1973). Fitness, on the other hand, is ". . . a value free algebraic parameter that measures the contribution made by hereditary determinants to the differential reproductive rate of organisms" (Stent, 1978:213). Adaptiveness and fitness are conceptually linked, but distinct—a point not always observed in the literature.

In a correct sociobiological sense, fitness refers only to an individual's contribution to the gene pool, directly and through kin. The statement that organisms tend to act to maximize their fitness means only that they "seek" representation in the gene pool through reproduction, altruism, etc. Behaviors that promote this representation—i.e., that increase fitness—are selected for by environmental pressures over those that do not. In this matter sociobiology is orthodox Darwinian theory (Ruse, 1978:358). An individual can be well adapted but unfit if it fails to reproduce.

If sociobiology were truly tautological, presumably it would be unfalsifiable. Nevertheless, critics (e.g., Sahlins, 1976) have, simultaneously, accused sociobiology of being tautological and attempted to falsify it (Ruse, 1979). It is possible to make behavioral predictions based on sociobiological tenets and to empirically evaluate those predictions. Then, if found false, and if acceptable alternative explanations cannot be developed, those predictions can be rejected as false (Krebs and Davies, 1978). This would not be the case, however, if sociobiology were tautological.

The upshot of this is that neither Darwin's formulation nor sociobiology's is tautological. If there were a tautology present, it has been in the theory since Darwin's time, and not introduced by sociobiologists as asserted by Quadagno.

Maximization of Genetic Fitness[1]

The treatment of the maximization of genetic fitness in Quadagno's (1979:106ff.) paper is also problematic. While Quadagno treats fitness as denoting an *outcome*, fitness, in fact, refers only to a *process*, or *tendency*. Indeed, her quote from Alexander (p. 104) refers to a continuing process, *not* an outcome. Barash (1977:33) notes that "all fitnesses are relative not only to other traits but also to the environment in question." Quadagno's paper seems to misunderstand this point. Sociobiologists do not argue that species ever achieve maximal fitness in an ultimate sense but, rather, only that natural selection is a directional influence towards increasing genetic fitness.

In this regard, Quadagno's (1979:104) paper exhibits conceptual confusion. She introduces the term, "adaptive fitness," a term not found in sociobiological literature. Additionally, she fails to use fitness in the sense of genetic contributions to the gene pool. While her introduction of this term may serve her argument, it obscures the distinction between adaptiveness and fitness. She then attempts to equate the principle of the maximization of genetic fitness with "maximal adaptiveness" (p. 104), further confounding adaptiveness and fitness. Her term implies an ideal phenotypic adaptation, an implication not found when fitness is used in its sociobiological sense. Sociobiologists, and evolutionists in general, don't normally look at the overall fitness of an organism but, rather, look at the fitness of particular traits of an organism in a given environment (Hull, 1974).

In attempting to demonstrate how sociobiologists violate Darwin's emphasis on minimum reproductive advantage with their "preoccupation with maximization" (p. 103), Quadagno misinterprets the use of the concept. She cites Williams (1966:132) in this regard, who writes, "[t]he reproduction of every individual is designed to maximize the number of its successful offspring." The key word in that phrase is "successful"—i.e., offspring who themselves live to reproduce. Maximization refers to an optimum. Organisms do not necessarily attempt to produce a maximum number of offspring, nor does sociobiology imply that they do. An organism maximizes its genetic fitness by producing an optimum number, with optimality dictated by local environmental circumstances. An organism can reduce its genetic fitness by producing too few offspring *or* too many, depending on a host of ecological variables. This is central to the process of natural selection. By "maximizing" its genetic fitness, an organism gains an advantage over its conspecifics who may fail to produce that optimum number; but that advantage may be only a slight one. Therefore, we do not consider sociobiology's "preoccupation" with maximization to violate Darwin's position.

---

[1] Sociobiologists use the concepts terms, "inclusive fitness" and "genetic fitness," neither of which is used systematically in Quadagno's paper. Genetic fitness refers to the "contribution to the next generation of one genotype in a population relative to the contribution of other genotypes" (Wilson, 1975:585). Inclusive fitness is defined as the "sum of an individual's own fitness plus all its influence on fitness in its relatives other than direct descendants . . ." (Wilson, 1975:586). Neither concept makes any implication of the ultimate adaptiveness of a phenotype as suggested by Quadagno.

It is interesting that Quadagno (1979:102) does not see a maximizing principle in the phrase, "survival of the fittest." Not only does it argue that the *fit* survive, it also clearly states that the fit*test* survive. In fact, in either Darwin's or sociobiology's formulation, fitness is relative. To maximize its fitness, an organism has to gain only some genetic advantage over conspecifics.

Adaptation and Nonadaptation

Quadagno (1979:104) argues that sociobiologists ignore the complexities of the mechanisms of evolution and concentrate on adaptation via natural selection. She asserts (p. 104) that "[n]o single mechanism accounts for all evolutionary change . . ." and goes on to note that many differences between organisms can be " . . . attributed to *nonadaptive* differentiation" (emphasis hers). Quadagno identifies (p. 104) some other mechanisms of differentiation such as genetic drift, migration between gene pools, and recombination. It should be noted that anything that causes genotypic—and, hence, phenotypic—variation is, in turn, acted on by natural selection, including the random factors listed by Quadagno. Therefore, it is not clear how sociobiology's interest in adaptive factors in evolutionary change distorts either evolution or "the actual state of affairs in the real world" (Quadagno, 1979:105). And, while sociobiologists are most concerned with behavioral adaptations of organisms, it would appear that they have not ignored other factors in evolution (e.g., Wilson, 1975:21–4, 64, 66).

It should not be surprising that the main concern of sociobiologists is the adaptations. As Ruse (1979:115) points out, evolutionists today more readily assume adaptive advantage for traits than they did twenty years ago. Darwin did not develop his particular formulations by assuming that the traits he observed in various organisms were *nonadaptive*.[2] While the explanation of possible nonadaptive traits has become a point of interest for some evolutionists, in the development of Darwin's theory the concern was clearly with the adaptive significance of traits. It would seem premature to accuse an emerging theory of ignoring one factor or another, when it has only begun to enter its period of "normal science" (Kuhn, 1970).

One other point in Quadagno's (1979:105) discussion of this requires some clarification. She writes, "[f]actors such as nutrition, toxins, accidents and learning experiences introduce phenotypic variance that goes beyond the control of the genotype, often reducing the adaptiveness of the organism." While she implies that such factors are nonadaptive, it would appear that accidents, nutrition, and toxins are simply part of the selection pressures that organisms face. In other words, these are components of natural selection as they act differentially on variations in individuals. It is true that such factors may reduce phenotypic adaptability (Quadagno, 1979:105) and, ultimately, genetic fitness; but there is no reason why the converse cannot be equally true—i.e., that such factors affect, positively, the fitness of some genotypes. Quadagno's major problem in this section stems from her attempt to take a concept referring to gene frequencies—i.e., fitness—and to improperly extend it to refer to a "maximally adapted" phenotype, an extension not found in sociobiological literature.

Altruism

The problem of altruistic behavior, as discussed by Quadagno (1979:103), is in need of elaboration. Why some organisms exhibit altruistic behavior has been one of the major problems of evolutionary theory and it is unquestionably a problem of interest to sociobiologists.[3] One of the explanations offered in sociobiology, the theory of kin selection, is rejected by Quadagno in the course of her rejection of the notion of reciprocal altruism. We would first raise the question: how *does* evolutionary theory explain altruistic behavior in nonhuman animals—if not by kin selection or a related concept? An early reference to the principle of kin selection appears in Darwin's *Origin of Species* (Wilson, 1975:117), indicating that kin selection is not a particularly new concept. The kin selection hypothesis has been successful in explaining altruism in a number of social species and is a plausible theory being offered in regard to certain forms of animal behavior. The sociobiological concept of parental manipulation (Alexander, 1975) also has been found useful in explaining certain forms of animal behavior, although it is not considered by Quadagno.

---

[2] Quadagno is in error when she states (p. 103) that Darwin studied fossils on the Galapagos Islands. He studied variations among living species of finches on the various islands of the archipelago (Barlow, 1933:438).

[3] We are in disagreement with Wilson (1975:3) that altruism is the central problem of sociobiology. His own work would indicate that it is but one of many points of interest to sociobiology.

The idea of reciprocal altruism (Trivers, 1971) is problematic and we are in agreement with Quadagno as to its lack of utility in sociobiology. We find it of little use because, conceptually, it does not seem to be distinct from more general notions of sociality. The examples of reciprocal altruism given by Wilson[4] (1975:120) may be taken as a case in point. Quadagno seems to suggest (p. 107) that, by rejecting reciprocal altruism as an explanation, ipso facto all sociobiological explanations of altruism can also be rejected. Trivers' concept is scarcely a central part of the sociobiological explanation of altruistic behavior—so, by its refutation, the power of kin selection and parental manipulation hypotheses are not weakened. However, we neither suggest that kin selection or parental manipulation are suitable for explaining all forms of altruistic behavior nor have the space to discuss the complexities of their application to human groups.

Evolutionary Principles

In discussing evolutionary principles, Quadagno (1979:107) characterizes sociobiology as suggesting that organisms are always progressing toward an optimum phenotype. This interpretation is at variance with Wilson's position, even on the page that Quadagno cites from him to support her contention (cf. Wilson, 1975:24). Wilson discusses "an equilibrium case," in which organisms have reached a steady state with regard to a given trait, even though theoretically they could evolve a better-adapted phenotype. Evolution of the trait does not continue toward some ideal value but, rather, it is halted in an intermediate state, due to opposing selection factors operating on secondary characteristics of that trait.

When sociobiologists do discuss optimums, intrinsic in the notion is the principle of evolutionary compromise (Wilson, 1975:131–2). In this case, an "optimum phenotype" is that which exists when various selection pressures strike a balance and evolution reaches a stabilizing state (Wilson, 1975:131). An optimum state is a compromise and a temporary condition. It should not be confused with an ideal or "maximally adapted" phenotype, as Quadagno (1979:105) implies. Optimality in sociobiology simply refers to a "temporarily stabilized" state (Wilson, 1975:132) and is the result of the play of various selection pressures from a changing environment.

A further point in Quadagno's (1979:105) paper involves an issue that can be by no means resolved in this short essay. That is the problem of the applicability of sociobiology to the study of human social behavior. Quadagno consistently characterizes nonhuman social behavior as "simple" and human social behavior as "complex." To describe nonhuman social behavior as necessarily simple is, at best, anthropocentric. Some of sociobiology's empirical "successes" have been in the analysis of very complex insect societies (Wilson, 1975) as well as in the analysis of relatively large primate troops (e.g., Hrdy, 1977). There are levels of human social organization that are less complex than such nonhuman social aggregates.

The real issue in a human sociobiology—one not directly addressed by Quadagno—is the degree of genotypic involvement in human behavior and the plasticity, or the lack of it, in those behaviors. One cannot simply assume that there is no genotypic involvement in human behavior. A necessary step in the development of a human sociobiology will have to be the determination of the role of genotype in human behavior, and in which behaviors genotype plays a part. Most of what sociobiology has proposed in this area has been, of necessity, speculative (e.g., Barash, 1977; Daly and Wilson, 1978; Wilson, 1975; 1978a). However, some recent studies are beginning to provide at least some suggestive preliminary evidence of the role of genotype in some human behaviors (Freedman, 1978; 1979;[5]

---

[4] Wilson (1975:120) does cite several examples of behavior that may be considered as examples of reciprocal altruism, in contradiction to Quadagno's assertion. However, the examples cited do not seem to be distinct enough to be considered as anything but examples of general sociality. Packer (1977) does offer a clear empirical case of primate behavior best described as reciprocal altruism.

[5] Freedman (1979) summarizes 50 empirical studies applying sociobiological principles to the study of humans.

Loehlin and Nichols, 1976; R. Wilson, 1978). At the same time, we recognize that some sociobiologists have been overzealous and incautious in their "biologicizing" of human social behavior and have been justifiably criticized.

Summary and Conclusions

We have argued that there is no inherent tautology in sociobiological reasoning and that it is logically consistent with Darwin's version of natural selection. The principle of the maximization of genetic fitness does not violate Darwin's formulation but, rather, simply extends it to behavior. In this instance, Quadagno (1979:104ff.) has reinterpreted the concept and has apparently confused a process (maximization of genetic fitness) with an ideal end state (maximal phenotypic adaptiveness). Maximizing genetic fitness refers only to achieving a certain gene representation in the gene pool—via successful offspring and siblings not producing a maximum number of offspring, irrespective of their survival—unless the latter is the evolved strategy that insures gene representation as it does in some species. Reproductive strategies are species specific.

We have attempted to show that sociobiologists are aware of random and nonadaptive factors in evolution. However, as with most evolutionary theory, sociobiology is more concerned with how traits are adaptive and to criticize it for that is to ignore the basic premise of Darwinian evolution.

We have suggested that Quadagno's treatment of the problem of altruism is, in some ways, in error. She correctly criticizes the notion of reciprocal altruism but attempts to discredit other sociobiological explanations of altruism by association. The kin selection hypothesis as an evolutionary explanation of altruism has been found to have analytic utility for nonhuman animals.

Given these suggested weaknesses in her presentation, we find Quadagno's (1979:108) claim that "sociobiology is not applicable to the study of human social behavior" to be severe and unwarranted, particularly at this early stage in the development of sociobiology. We are in agreement with Quadagno's assertion that sociology is in no immediate danger of being replaced by sociobiology. As Wilson (1978b:xii) notes, "[c]ontemporary general sociobiology might at best explain a tiny fraction of human social behavior in a novel fashion." We would argue that, although sociobiology as a paradigm of behavior is in its infancy, it has provided an interesting, if controversial, set of postulations regarding human social behavior. The outright rejection by Quadagno of sociobiology, at least in its relevance to humans, is very similar to the "scientific" response to Darwin's *Origin of Species* by his contemporaries (Hull, 1978). Hull notes that Darwin's theory faced many of the same methodological and theoretical critiques as sociobiology.

It appears that some sociologists are attempting to hold sociobiology to standards of scientific rigor that few of their own theories would survive. As Hull (1978:145) writes, "[t]he trouble with the methodological objections raised against sociobiology is that the standards from which they flow are applicable only to theories as finished products and they are being used to assess a newly emerging theory." It is ironic that a discipline that uses its youth as a defense of the inadequacy of some of its theories would so readily attack a paradigm that has barely settled on its major principles or methods.

In its current state of development, sociobiology requires further conceptual and methodological elaboration and clarification. That some of sociobiology's concepts may be obscure should not be surprising.[6] As Hull notes (1978:145), ". . . this state of affairs is common in science" and, as such, does not justify a call for the rejection of sociobiology.

We contend that the charges brought by Quadagno (1979:106ff.) against sociobiology are little different from the charges that have been, or could be, brought against Darwin's theory. We have also tried to demonstrate that, in general, they are unwarranted. Paradigmatically, sociobiology does not represent any radical discontinuity with Darwinian principles. Rather, it extends them, in combination with population genetics, to the study of social behavior. Calls for the rejection of a theory are commonplace in the history of science. Darwin was subjected to them and has been vindicated (Hull, 1978; Mayr, 1976). Quadagno's conclusion of the inapplicability of sociobiology for human behavior suggests the premature closure of sociology and is not likely to serve any scientific purpose. Hypotheses such as those offered by sociobiology must be entertained, often before we have ways of testing those hypotheses or before we know if they can be tested (cf. Kaplan, 1964). While careful evaluation of sociobiology must continue in sociology, calls for its rejection are not justifiable at this stage in sociobiology's development.

---

[6] Ruse (1973:49) indicates it is not unusual, in evolutionary theory, to find instances of "suggestions, hypotheses, extremely weak inductive inferences and frequently outright guesses." In the development of a science, particularly one just emerging such as sociobiology, there is a need for open-ended concepts, concepts open to alteration as new data become available (Skolimowski, 1974). Critiques such as Quadagno's are useful in pointing out areas in need of clarification.

ROBERT BOLIN
SUSAN BOLTON BOLIN
North Dakota State University

REFERENCES

Alexander, R.
1975 "The search for a general theory of behavior." Behavioral Science 20:77–98.
Barash, D.
1977 Sociobiology and Behavior. New York: Elsevier.
Barlow, N. (ed.)
1933 Charles Darwin's Diary of the Voyage of H.M.S. Beagle. Cambridge, England: University Press.
Daly, M. and M. Wilson
1978 Sex, Evolution and Behavior. North Scituate, Mass.: Duxbury.
Dobzhansky, T.
1955 Evolution, Genetics and Man. New York: Wiley.
Freedman, D. G.
1978 "Ethnic differences in babies." Human Nature 2:36–43.
1979 Human Sociobiology. New York: Free Press.
Gould, S. F.
1976 "Darwin's untimely burial." Natural History 85:24–30.
Hrdy, S. B.
1977 The Langurs of Abu. Cambridge, Mass.: Harvard University Press.
Hull, D. L.
1974 The Philosophy of Biological Science. Englewood Cliffs, New Jersey: Prentice Hall.
1978 "Scientific bandwagon or traveling medicine show." Pp. 136–63 in M. Gregory, A. Silvers, D. Sutch (eds.), Sociobiology and Human Nature. San Francisco: Jossey-Bass.
Kaplan, A.
1964 The Conduct of Inquiry. Scranton, Penn.: Chandler.
Krebs, J. R. and N. B. Davies
1978 Behavioural Ecology: An Evolutionary Approach. Sunderland, Mass.: Sinauer.
Kuhn, T.
1970 The Structure of Scientific Revolutions. Chicago: University of Chicago Press.
Loehlin, J. and R. Nichols
1976 Heredity, Environment and Personality. Austin: University of Texas Press.
Mayr, E.
1976 Evolution and the Diversity of Life. Cambridge, Mass.: Belknap.
McCleery, R.
1978 "Optimal behaviour sequences and decision making." Pp. 377–410 in J. R. Krebs and N. B. Davies (eds.) Behavioural Ecology: An Evolutionary Approach. Sunderland, Mass.: Sinauer.
Packer, C.
1977 "Reciprocal altruism in *Papio anubis*." Nature 265:441–3.
Quadagno, J. S.
1979 "Paradigms in evolutionary theory: the sociobiological model of natural selection." American Sociological Review 44:100–9.
Ricklefs, R.
1973 Ecology. Newton, Mass.: Chiron.
Ruse, M.
1973 The Philosophy of Biology. London: Hutchinson.
1978 "Sociobiology: a philosophical analysis." Pp. 335–75 in A. Caplan (ed.), The Sociobiology Debate. New York: Harper and Row.
1979 Sociobiology: Sense or Nonsense? Boston: D. Reidel.
Sahlins, M.
1976 The Use and Abuse of Biology. Ann Arbor: University of Michigan Press.
Scriven, M.
1971 "Explanations and predictions in evolutionary theory." Pp. 213–27 in R. Munson (ed.), Man and Nature. New York: Dell.
Skolimowski, H.
1974 "Problems of rationality in biology." Pp. 205–33 in F. J. Ayala and T. Dobzhansky (eds.), Studies in Philosophy of Biology. Berkeley: University of California Press.
Stent, G.
1978 Paradoxes of Progress. San Francisco: Freeman.
Trivers, R.
1971 "The evolution of reciprocal altruism." Quarterly Review of Biology 46:35–6.
Williams, G. C.
1966 Adaptation and Natural Selection: A Critique of Some Current Evolutionary Thought. Princeton: Princeton University Press.
Wilson, E. O.
1975 Sociobiology: The New Synthesis. Cambridge, Mass.: Belknap.
1978a On Human Nature. Cambridge, Mass.: Harvard University Press.
1978b "Foreword." Pp. xi–xiv in A. Caplan (ed.), The Sociobiology Debate. New York: Harper and Row.
Wilson, R.
1978 "Synchronies in mental development: an epigenetic perspective." Science 202:939–47.

REPLY TO BOLIN AND BOLIN*

In commenting on my critique of sociobiology, Bolin and Bolin propose a series of arguments, some perhaps valid, some based on a misunderstanding of points made, and a few containing clear misrepresentations of my position. Since the central theoretical problem of sociobiology has been to explain altruism, I will begin with this issue.[1]

---

* Direct all communications to: Jill S. Quadagno; Department of Sociology; The University of Kansas; Lawrence, KS 66044.

As I stated in my original essay, kin selection and reciprocal altruism provide mutually exclusive and contradictory hypotheses to explain the same phenomenon. The explanation of reciprocal altruism by selection has proven difficult for sociobiologists, and Bolin and Bolin (with some vacillation on their part) join me in rejecting it. Bolin and Bolin state that I also reject the theory of kin selection. Since they do not refer to a specific page in my paper, it is impossible to determine where they find my rejection of this concept. In fact, no such statement appears.

The fact that I do not reject kin selection does not mean that it is not problematic and still a source of debate, even among "known" sociobiologists. For instance, Dawkins (1976:101–2) disputes Wilson's (1975) presentation of kin selection as a special case of group selection, arguing instead that it is a special consequence of gene selection. He also chastises Wilson for excluding offspring in his definition of kin selection, for offspring have the same genetic relationship to an individual as siblings ($r = ½$). Their exclusion ignores Hamilton's (1964) argument that the distinction between kin and nonkin is not hard and fast but a matter of mathematical probability.

Parental manipulation, a theory which I ignored in my original essay because it was not directly relevant, has been posed as an alternative evolutionary cause of altruistic behavior, independent of straightforward kin selection (Alexander, 1975). It has been accepted by some and rejected by others. For instance, Dawkins (1976:148) states, "Alexander's conclusion that there is a built-in advantage on the parent's side in the conflict of interests is not correct."[2] I will not attempt to summarize the debate here but will leave the topic open as a possible source of explanation of some forms of altruistic behavior in nonhuman animals.

If we then accept kin selection and parental manipulation as potentially valid explanations of altruistic behavior toward relatives, we are still left with the original dilemma of how altruism toward nonkin (which, by definition, reduces "personal fitness") could possibly evolve by means of natural selection. Bolin and Bolin do not deal with this problem at all. More important is the fact that my paper was quite specifically a critique of the application of sociobiology to complex human social behavior. I do not disagree with Bolin and Bolin that kin selection has "analytic utility for nonhuman animals." However, I would question its extension to humans and, apparently, Bolin and Bolin would also.

This brings me to the second issue, the presumed ability of sociobiologists to identify the biological basis of complex human social behavior. On this topic, Bolin and Bolin misrepresent my position. By using complex (as opposed to simple), my allusion was to the array of human social behaviors—referred to by Wilson and other sociobiologists—which can purportedly be explained by sociobiological theory. Among these, to name a few, are deceit, parent/child conflict, the self-centeredness of young children, the peer-group orientation of adolescents, religion, and the altruism of the living saint, Mother Theresa (Barash, 1977; Trivers, 1972; 1974; Wilson, 1975; 1978). I did not mean to imply, as Bolin and Bolin suggest, that nonhuman animal behavior was not complex but, rather, that sociobiology is incapable of calculating the genetic basis of complex human behavior. This does not signify that there is no genotypic involvement in human behavior, only that there is no way of measuring it.

Third, there is the question of whether sociobiological reasoning is tautological. The answer depends partly on the definition of two terms, "adaptation" and "fitness." Interestingly, in defining these concepts, Bolin and Bolin do not quote sources identified as mainstream sociobiologists. Thus, they are able to keep the terms analytically distinct. In the definition they use, an adaptation is "any genetically determined characteristic that increases an organism's ability to cope with its environment," whereas fitness refers to "the contribution made by hereditary determinants to the differential reproductive rate of organisms." In contrast, the sociobiological usage confounds the two terms. For instance, Barash (1977:33) states that an adaptation is "any evolved characteristic of an organism that increases its fitness." Similarly, Wilson (1975:21) defines a trait as nonadaptive "if it reduces the fitness of individuals that consistently manifest it under environmental circumstances that are usual for the species." In both cases, the term "adaptation" is defined in terms of fitness. Bolin and Bolin state that I "obscure the distinction between adaptiveness and fitness." It is obvious that the blame is misplaced.

---

[1] Bolin and Bolin do not agree with Wilson's statement that the explanation of altruistic behavior is the central *theoretical* problem of sociobiology. Regardless of their personal opinion, there is no doubt that this unresolvable dilemma provided a major impetus in the historical development of sociobiology as a discipline (Quadagno, 1979:103).

[2] For a more detailed and comprehensible explanation of the theory as well as its criticisms, see Dawkins (1976:145–9).

So far, then, we can cull two statements from the sociobiological literature:

> (1) Any behavior pattern that increases fitness is adaptive; and
> (2) any behavior pattern that reduces fitness is nonadaptive.

These statements are not, in themselves, tautological. However, there is a third assumption—or, really, set of assumptions—made by sociobiologists, which make it possible to explain *any* given human behavior as the end product of natural selection. These assumptions are that existing traits of species are adaptive (Barash, 1977:33) and indicative of fitness (Wilson, 1975:67). Thus, Barash states, "A fundamental assumption of sociobiology is that behavior patterns are adaptive." In a similar vein, Daly and Wilson (1978:24) conclude that "species typical behavior is usually fit behavior." An example will illustrate the misuse to which these principles can be put.

In pursuing his case for the genetic basis of altruistic behavior, Trivers begins his explanation with the assumption that altruism between friends is an evolved characteristic. He states:

> The tendency to like others, not necessarily closely related, to form friendships and to act altruistically toward friends and toward those one likes will be selected for as the immediate emotional rewards motivating altruistic behavior and the formation of altruistic partnerships. (Trivers, 1978:213).

As evidence to support his hypothesis, Trivers cites research findings which demonstrated that altruistic behavior was correlated with friendship. Thus, he notes that "Sawyer (1966) has shown that all groups in experimental situations tested showed more altruistic behavior toward friends than toward neutral individuals. Likewise, Friedrichs (1960) has shown that attractiveness as a friend was most highly correlated among undergraduates with altruistic behavior" (Trivers, 1978:213). A few pages later, Trivers cites research which reaches the exact opposite conclusion. In this case, children were shown to behave more altruistically to a stranger than to a friend. Trivers (1978:219) states:

> Particularly toward strangers, humans may initially act more altruistically than toward friends. Wright (1942) has shown, for example, that third grade children are more likely to give a more valuable toy to a stranger than a friend.

This discrepancy, which might provide some persons cause for contemplation, poses no problem for Trivers. He is equally able to conclude that this behavior, too, is biologically based, the result of the forces of natural selection. As he explains:

> Selection will favor a mechanism for establishing reciprocal relationships. Since humans respond to acts of altruism with feelings of friendship that lead to reciprocity, one such mechanism might be the performing of altruistic acts toward strangers, in order to induce friendship. In short, do unto others as you would have them do unto you. (Trivers, 1978:219)

Once it is assumed that existing behaviors are either fit or adaptive or both, any human behavior can be explained genetically. Without using the apparently offensive term "adaptive fitness" which I admittedly (but inadvertently) coined, I am able to make the exact same argument as that made in my original essay. In the sociobiological explanation of human social behavior, the potential for tautological reasoning always exists.

Bolin and Bolin also argue that I treat fitness as denoting an ideal outcome, whereas sociobiologists view it as a process. There are two flaws in their argument. First, the definition of fitness which they accept as valid (Stent, 1978:213) refers to neither a process nor an outcome. Rather, it is simply a measure of reproductive success. Bolin and Bolin use the term "fitness" as both a kind of measure and a process, because they are coupling the standard definition of fitness with its sociobiological usage. They seem to forget that it is not the maximization of genetic fitness that is the process in evolutionary theory, but natural selection which in turn produces fitness.

Second, *I* do not equate the maximization of genetic fitness with an ideal end state (maximal phenotypic adaptiveness). This, unfortunately, is what sociobiologists sometimes do. As I stated, "Even though they [sociobiologists] are aware that evolution need not produce maximal solutions, they tend to forget or simply ignore the fact that this is the case" (Quadagno, 1979:105). The following quotation from Wilson (1975:156) illustrates this occasional tendency:

> When exploratory behavior leads one or a few animals to a breakthrough enhancing survival and reproduction, the capacity for that kind of exploratory behavior and the imitation of the successful act are favored by natural selection. The enabling portions of the anatomy, particularly the brain, will then be perfected by evolution.

Finally, Bolin and Bolin also state that I do not understand that fitness is relative to a given environment. I can only quote again from my paper, "the evolutionary process can only be defined and comprehended relative to a given set of environmental and demographic circumstances (Quadagno, 1979:105).

In my conclusion, I suggested that sociobiology is not applicable to the study of human social behavior. My statement was extreme as a matter of necessity. Few sociologists, including myself, would deny that there are biological limitations imposed on the range of possible social systems which can develop. However, the wildly speculative claims of some sociobiologists have only served to illustrate the dangers present when those claiming to be armed with the tools of "science" enter the realm of philosophy. The acceptance or rejection of sociobiology as a suitable approach to the study of human behavior ultimately lies in the future, a future which can be determined, in part, by how those who identify themselves as sociobiologists proceed. Sociobiologists, themselves—not their critics—have been their own worst enemy.

Jill S. Quadagno
The University of Kansas

REFERENCES

Alexander, R.
1975 "The search for a general theory of behavior." Behavioral Science 20:77–98.
Barash, D.
1977 Sociobiology and Behavior. New York: Elsevier.
Daly, M. and M. Wilson
1978 Sex, Evolution and Behavior. North Scituate, Mass.: Duxbury.
Dawkins, R.
1976 The Selfish Gene. Oxford: Oxford University Press.
Friedrichs, R. W.
1960 "Alter versus ego: an exploratory assessment of altruism." American Sociological Review 25:496–508.
Hamilton, W. D.
1964 "The genetics theory of social behavior, I and II." Journal of Theoretical Biology 7:1–52.
Quadagno, J. S.
1979 "Paradigms in evolutionary theory: the sociobiological model of natural selection." American Sociological Review 44:100–9.
Sawyer, R.
1966 "The altruism scale: a measure of cooperative, individualistic, and competitive interpersonal orientation." American Journal of Sociology 71:407–16.
Stent, G.
1978 Paradoxes of Progress. San Francisco: Freeman.
Trivers, R. L.
1972 "Parental investment and sexual selection." Quarterly Review of Biology 46:35–57.
1974 "Parent-offspring conflict." American Zoologist 14:249–64.
1978 "The evolution of reciprocal altruism." Pp. 189–226 in T. H. Clutton-Brock and P. H. Harvey (eds.), Readings in Sociology. Reading: W. H. Freeman.
Wilson, E. O.
1975 Sociobiology: The New Synthesis. Cambridge, Mass.: Belknap.
1978 On Human Nature. Cambridge, Mass.: Harvard University Press.
Wright, B.
1942 "Altruism in children and the perceived conduct of others." Journal of Abnormal Social Psychology 37:218–37.

Weinberg and COLIN J. WILLIAMS are currently studying the determinants of black/white differences in sexual patterns. Williams is Associate Professor at Indiana University-Purdue University at Indianapolis, and a Senior Research Sociologist, Institute for Sex Research, Indiana University. His book, *American Sexual Standards*, written with Albert D. Klassen and Eugene E. Levitt, is forthcoming.

■ MARK ABRAHAMSON (Sudden Wealth, Gratification, and Attainment) is Professor and Department Head, Department of Sociology, University of Connecticut. To be published by Prentice-Hall in 1981 is his forthcoming book regarding classical theory and contemporary research. His continuing research is on patterns of dominance within and between metropolitan areas in the United States.

■ WILLIAM P. BRIDGES (Industry Marginality and Female Employment) is Assistant Professor, Department of Sociology, University of Illinois at Chicago. His current research involves developing a sociological conception of industry structure and identifying its relationships with the division of labor and the differentiation of the labor force.

■ FRANÇOIS NIELSEN (The Flemish Movement in Belgium after World War II) is Assistant Professor, Department of Sociology, University of Chicago. In addition to further research on the linguistic conflict in Belgium, he is interested in the mathematical and statistical aspects of dynamic models in sociology. He is preparing a survey of the high school student population of Hispanic origin, in connection with the "High School and Beyond" project at the National Opinion Research Center in Chicago.

■ DELBERT S. ELLIOTT (Reconciling Differences in Estimates of Delinquency) is Director of the Behavioral Research Institute as well as Professor, Department of Sociology, University of Colorado. He is the principal investigator of two studies: (1) "The Dynamics of Delinquent Behavior . . ." (funded by the Center for Studies in Crime and Delinquency, NIMH), which involves a national youth panel initially interviewed in 1977, with follow-up interviews annually through 1981; and (2) "The Dynamics of Delinquency and Drug Use," funded by the National Institute of Juvenile Justice and Delinquency Prevention, LEAA. His associate, and his co-author in this issue of ASR, is SUZANNE S. AGETON, a Senior Research Associate at the Behavioral Research Institute. She is the principal investigator on a study of sexual assault among adolescents, focusing on the question of vulnerability to such assaults as well as on the short- and long-term consequences of sexual assault. Her central involvement presently is as co-principal investigator on a national longitudinal study of delinquent behavior.

■ LOUISE SHELLEY (The Geography of Soviet Criminality), an Assistant Professor in the School of Justice, American University, Washington, D.C., is doing further research on the impact of development on crime, Soviet crime and criminology, crime in socialist countries, and corrections. She has edited *Readings in Comparative Criminology*, due for September, 1980, publication by Southern Illinois University Press which is also to publish her nearly completed book on development and crime in comparative perspective.

■ PAUL D. ALLISON (Analyzing Collapsed Contingency Tables) is Assistant Professor, Department of Sociology, Cornell University. With J. Scott Long, he is studying the effects of departmental prestige and resources on the research productivity of academic scientists. He is also developing new methods for analyzing event history data. His *Processes of Stratification in Science* is forthcoming from Arno Press.

■ ROBERT W. JACKMAN (Comment on Stack, ASR, December, 1978) is Associate Professor of Political Science, Michigan State University, East Lansing, Michigan. His current research involves: (1) Coups d'etat and related forms of political violence in Third World countries; and (2) the nature of social class identification in the United States.

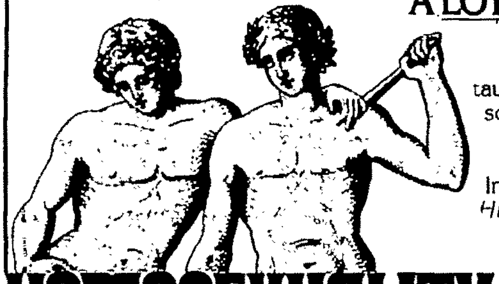
■ GLENN FIREBAUGH (Comment on Stack, ASR, December, 1978) is Assistant Professor, Department of Sociology, Vanderbilt University, Nashville. He is continuing his study of the linkages between agricultural change and population change in the Third World.

■ STEVEN STACK (Reply to Jackman and Firebaugh) is Assistant Professor, Department of Sociology, Indiana University. Currently he is working on cross-national studies of suicide. He also is investigating the relationship between unemployment rates and crime rates.

■ ROBERT BOLIN (Comment on Quadagno, ASR, February, 1979), Assistant Professor, Department of Sociology, North Dakota State University, is analyzing data on elderly victims of the Wichita Falls, Texas, tornado of 1979. His concern is to examine coping strategies and patterns of aid utilization among elderly families in environmental stress situations. SUSAN BOLTON BOLIN, his co-author, is a Ph.D. candidate, Department of Zoology, North Dakota State University, and is currently engaged in behavioral ecology research on flock behavior patterns and determinants of depredation among certain species of birds.

■ JILL S. QUADAGNO (Reply to Bolin and Bolin) is Assistant Professor of Sociology at the University of Kansas. She is doing an investigation of the social and economic conditions concerning poverty among the rural aged (in collaboration with Scott G. McNall). She has been in Cambridge, England, for several months, working with the Cambridge Group for the History of Population and Social Structure.

THE ANCIENT GREEKS DIDN'T GIVE
HOMOSEXUALITY A SECOND THOUGHT.
A LOT HAS CHANGED SINCE THEN.



Throughout history, homosexuals have been sent to prison, put in asylums, drugged, taunted, scorned, even executed. However, not all societies have been hostile toward homosexuals. The Greeks, for example, believed that homosexuality was a normal part of nature's plan. In his ground-breaking book, *Homosexuality: A History,* Vern L. Bullough outlines the forces that have led to our present-day attitudes toward homosexuality. He shows how social attitudes have a history, and how that history can tell us something about what we believe today. Comprehensive, far-reaching, and fair-minded, this book traces homosexual behavior from ancient Greece to the present.

HOMOSEXUALITY
A HISTORY
FROM ANCIENT GREECE TO GAY LIBERATION

By Vern L. Bullough, Ph.D.

Meridian F516 $4.95

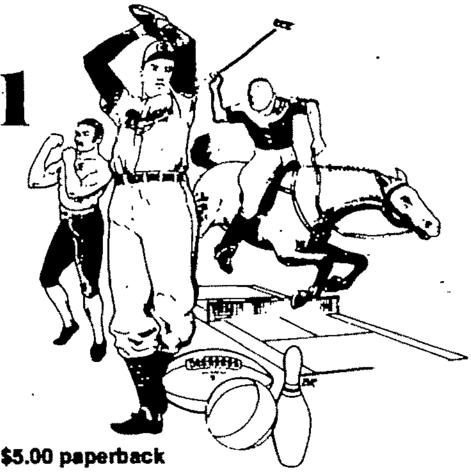
NEW AMERICAN
LIBRARY EDUCATION
1633 Broadway, New York 10019

NAL

Now in paperback—". . . the most intelligent book yet published in English on modern sport." —*Journal of American History*

From Ritual to Record

The Nature of Modern Sports
ALLEN GUTTMANN



A deftly researched and highly entertaining profile of the nature of modern sports, *From Ritual to Record* is the most thorough look yet at the way our sports have developed to fill their special role in the contemporary world.

"Here is a study of popular culture as imaginatively serious as the best humanistic studies in literature and philosophy. And, special blessing, its style is graceful, demotic, witty."
—Patrick D. Hazard, *Annals*
"It is a pioneering work in a field remarkably little touched . . . required reading."
—Julian Symons, *Times Literary Supplement*

$5.00 paperback

To order send check or money order to Dept. JN at the address below, including $1.30 per order for postage and handling.

COLUMBIA UNIVERSITY PRESS
136 South Broadway, Irvington, New York 10533

CAMBRIDGE

Portraits of "The Whiteman"

Linguistic play and cultural symbols among the Western Apache

Keith H. Basso

with a foreword by Dell Hymes

"Ethnographers are not the only ones who do ethnography and *Portraits of 'the Whiteman'* presents the most subtle evidence that this is so for North American Indians. It is absolutely at the cutting edge of developments in mutual understanding, to say nothing of the fact that it is superb anthropology."—*Alfonso Ortiz, University of New Mexico*

"Basso provides a rare insight into Western Apache perceptions of Anglo-Americans that should be required reading for anybody interested in native Americans."—*Library Journal*

Hardcover $14.95 Paper $4.95

Liberalism and Sociology

L. T. Hobhouse and Political Argument in England, 1880–1914

Stefan Collini

An historical account, based on extensive original research, of the relationship between Liberal political theory and the development of British sociology in the late 19th and early 20th centuries, especially as it was revealed in the work of Hobhouse.

$24.95

Contrasting Communities

English Villagers in the Sixteenth and Seventeenth Centuries

Margaret Spufford

Now in Paperback $11.95

Urbanization and Urban Growth in the Caribbean

An Essay on Social Change in Dependent Societies

Malcolm Cross

With examples drawn from Cuba, Haiti, Puerto Rico, and the Commonwealth Caribbean, Cross examines the nature and impact of unplanned urban growth, taking into account the influence of past European economic and strategic needs and the area's proximity to the United States. *Urbanization in Developing Countries*

Hardcover $19.95 Paper $5.95

A Comparative Approach to Policy Analysis

Health Care Policy in Four Nations

Howard M. Leichter

"This book is excellent both as an introduction to comparative policy analysis and for its specific examination of health care. . . . The historical perspective and the attempt to account for the causes of variation in policy makes this book quite different from those that are simply surveys of health care in other countries. Recommended."—*Library Journal*

Hardcover $21.50 Paper $7.50

Families in Former Times

Kinship, Household and Sexuality in Early Modern France

Jean-Louis Flandrin

Richard Southern, Translator

Hardcover $32.50 Paper $8.95

Cambridge University Press

32 East 57th Street. New York. N.Y. 10022

It doesn't have to be hard to be good sociology.

Reece McGee
Purdue University

Sociology:
AN INTRODUCTION,
SECOND EDITION

In direct response to user and reviewer comments, *Sociology: An Introduction, Second Edition,* more comprehensive than any other introductory sociology text on the market, has been updated and completely refined, and retains all the popular features of the successful first edition.

What you wanted refined:

- Eight new chapters by eight new authors providing better clarity, focus, and theoretical integration.
- Research methods chapter now appears as the last chapter of the book emphasizing its optional nature.
- Three chapters for the first edition covering social organization have been tightened and re-written by two new authors to focus on social interaction and social organization.

What you wanted retained:

Sociology: An Introduction, Second Edition still features the comprehensive, theoretically informed approach which caused the first edition to be adopted by more than 150 schools in its first year. The second edition also retains the popular running glossary, and biographical sketches of major theoreticians.

A Complete Ancillary Package to make teaching easier:

- An Instructor's Manual with film guide by Joseph Rogers, New Mexico State University.
- Study Guide by Joseph Rogers, New Mexico State University, with chapter summaries, outlines, key terms, concepts, phrases, names, self-tests, and suggestions for further study.
- Computerized test bank of 1,000 items to save instructor's time.

ISBN 0-03-043891-8
1980, cloth, 672 pages

Introduction to Sociology

SOCIOLOGY FULL CIRCLE:
Contemporary Readings on Society, Third Edition
William Feigelman
Nassau Community College
ISBN: 0-03-053186-1, Available January, 1980, 420 pages, paper

20th CENTURY SOCIAL THOUGHT
Third Edition
R.P. Cuzzort
University of Colorado
E.W. King
University of Denver
ISBN: 0-03-052521-7, Available March, 1980, 416 pages, paper

Social Problems

SOCIAL PROBLEMS
Third Edition
Frank R. Scarpitti
University of Delaware
ISBN: 0-03-046736-5, Available January, 1980, 768 pages, cloth

Marriage and Family

FAMILY SYSTEMS IN AMERICA
Third Edition
Ira L. Reiss
University of Minnesota, Minneapolis
ISBN: 0-03-047246-6, Available January, 1980, 544 pages, cloth

Human Sexuality

FUNDAMENTALS OF HUMAN SEXUALITY
Third Edition
Herant A. Katchadourian
Stanford University School of Medicine
Donald T. Lunde
Stanford University
ISBN: 0-03-042941-2, Available March, 1980, 628 pages, paper

Gerontology

AGING AND SOCIETY:
An Introduction to Social Gerontology
John B. Williamson
Boston College
Linda Evans
(Ph.D. Boston College)
Ann Munley
Marywood College
ISBN: 0-03-040321-9, Available January, 1980, 496 pages, cloth

Gerontology

GROWING OLD: The Social Problems of Aging
Elizabeth S. Johnson
John B. Williamson
Boston College
ISBN: 0-03-040316-2, Available January, 1980, 224 pages, paper

Sociology of Education

EDUCATION AND SOCIETY
Joseph A. Scimecca
George Mason University
ISBN: 0-03-038466-4, Available December, 1979, 322 pages, cloth

Complex Organizations

SOCIOLOGY OF COMPLEX ORGANIZATIONS:
Readings, Third Edition
Edward W. Lehman
Amitai Etzioni
New York University
Center for Policy Research
ISBN: 0-03-047461-2, Available February, 1980, 576 pages, cloth

Social Change

SOCIAL CHANGE
Steven Vago
St. Louis University
ISBN: 0-03-0407710-0, Available January, 1980, 480 pages, cloth

Introduction to Criminal Justice

CRIMINAL JUSTICE:
An Overview, Third Edition
Alexander B. Smith
(Ph.D. New York University)
Harriet Pollack
John Jay College
ISBN: 0-03-046586-9, Available December, 1979, 264 pages, paper

Police Science/ Criminal Justice

POLICE AND POLICING:
An Introduction
Richard J. Lundman
The Ohio State University
ISBN: 0-03-042551-4, Available January, 1980, 256 pages, paper

Law of Arrest

THE LAW OF ARREST, SEARCH AND SEIZURE
Third Edition
J. Shane Creamer
(J.D. Temple University)
ISBN: 0-03-054896-9, Available January, 1980, 448 pages, paper

For complimentary copies, please send course title and approximate enrollment to: James I. Ryder
HOLT, RINEHART AND WINSTON
383 MADISON AVE
NEW YORK, NY 10017

HOUGHTON MIFFLIN UPDATE:

New for 1980

Understanding Human Sexuality

Joann S. DeLora
San Diego State University
Carol A. B. Warren
University of Southern California
Carol Rinkleib Ellison
Clinical Psychologist
About 432 pages • paper • Study Guide • Instructor's Manual • Forty 35mm slides • Just published

A concise, yet comprehensive overview of human sexuality from three perspectives — social, psychological, and medical-physiological — that encourages introspection and self-understanding. DeLora, Warren, and Ellison present a balanced, developmental picture of sexuality without becoming highly technical or theoretical.

Special extra: A *free* Study Guide comes shrinkwrapped with each *new* copy of the text.

Sociology of Education: An Introduction Second Edition

Sarane S. Boocock, Rutgers, The State University of New Jersey
About 448 pages • Just published

Can schools produce a high-level of cognitive learning without losing sight of other objectives? Can schools be instruments of social progress and reform? In the Second Edition, Boocock addresses these and other fundamental questions within a framework that helps students understand the relation of social factors and learning.

Sociology of Aging

Diana K. Harris
William E. Cole
both of University of Tennessee
About 480 pages • Instructor's Manual • Early 1980

Harris and Cole integrate aging within the framework of sociological principles. With comprehensive treatment, they examine social institutions (including education and religion), cultural values, socialization, the minority aged, problems of the elderly, and more. Reflecting the latest research, the text exposes many fallacies and "myth-conceptions" about the aged and aging.

Strangers to These Shores: Race and Ethnic Relations in the United States

Vincent N. Parrillo
William Paterson College
About 448 pages • Instructor's Manual • Early 1980

Parrillo's wide-ranging study of many groups helps students to gain insight into the patterns of majority-minority relations. He includes groups not usually covered, such as Vietnamese, Koreans, Arabs, Gypsies, Armenians, Greeks, and Poles. First-hand accounts by immigrants sharpen theoretical perspectives.

Issues in Feminism: A First Course in Women's Studies

Sheila Ruth
Southern Illinois University
About 704 pages • paper • Early 1980

From a feminist point of view, Ruth's text/reader presents a unified introduction to the diverse concepts and disciplines that make up Women's Studies. *Issues in Feminism* combines explication and analysis with abundant selections from sociology, anthropology, economics, law, literature, philosophy, and political science.

For adoption consideration, request examination copies from your regional Houghton Mifflin office.

Houghton Mifflin

Dallas, Texas 75234
Geneva, Illinois 60134
Hopewell, New Jersey 08525
Palo Alto, California 94304

HOUGHTON MIFFLIN

UPDATE:

**Sociological Footprints:
Introductory Readings in Sociology**
Leonard Cargan and
Jeanne Hazen Ballantine
both of Wright State University
430 pages • paper
Instructor's Manual • 1979

Cargan and Ballantine's reader balances classical articles with applications of current research to bring theory to life for students. Articles were pretested on over 1,000 students to ensure significance, readability, and usefulness.

The Criminology of Deviant Women
Freda Adler, Rutgers,
The State University of New Jersey
Rita James Simon
University of Illinois, Urbana
425 pages • paper • 1979

Using a historical, cross-cultural, encyclopedic approach, Adler and Simon unite scholarly writings with research on female offenders, presenting all schools of thought.

**Race and Ethnic Relations
Fourth Edition**
Brewton Berry, Emeritus
The Ohio State University
Henry Tischler
Framingham State College
433 pages • Test Manual • 1978

Medical Sociology
John A. Denton
Eastern Kentucky University
361 pages • 1978

Deviant Behavior
Alex Thio, Ohio University
416 pages • Instructor's Manual • 1978

**Sociology and American
Social Issues**
Leonard Gordon
Arizona State University
548 pages • Study Guide by
Patricia A. Harvey
Instructor's Manual • 1978

**The History and Nature of
Sociological Theory**
Daniel W. Rossides, Bowdoin College
567 pages • 1978

**The Family, Society, and the
Individual, Fourth Edition**
William M. Kephart
University of Pennsylvania
546 pages • Instructor's Manual • 1977

**Statistical Reasoning in Sociology
Third Edition**
John H. Mueller
Karl F. Schuessler, Indiana University
Herbert L. Costner
University of Washington
544 pages • Solutions Manual • 1977

**The American Class System:
An Introduction to Social
Stratification**
Daniel W. Rossides, Bowdoin College
489 pages • 1976

For adoption consideration, request examination copies from your regional Houghton Mifflin office.

Houghton Mifflin

Dallas, Texas 75234 • Geneva, Illinois 60134
Hopewell, New Jersey 08525
Palo Alto, California 94304

Great Scott!

The Student Sociologist's Handbook Third Edition

Pauline Bart,
University of Illinois-Chicago Circle
Linda Frankel, Harvard University
Available April 1980, approx. 288 pages, softbound

Aging and Old Age

Sheila C. McKenzie,
Western Kentucky University
Available February 1980, approx. 320 pages, illustrated, softbound, with Instructor's Manual

Exploring Marriage and the Family

David Knox, East Carolina University
January 1979, 580 pages, illustrated, hardbound, with Instructor's Manual

Essential Sociology

Robert L. Ellis, Amarillo College
Marcia J. Lipetz,
Northwestern University
December 1978, 432 pages, illustrated, softbound, with Instructor's Manual

Sociology: Focus on Society

Lucile Duberman,
Rutgers University, Newark
Clayton A. Hartjen,
Rutgers University, Newark
February 1979, 601 pages, illustrated, hardbound, with Instructor's Manual and Study Guide

Social Problems: A Critical Approach

Kenneth J. Neubeck,
The University of Connecticut
December 1978, 460 pages, illustrated, hardbound, with Instructor's Manual

Complex Organizations: A Critical Essay Second Edition

Charles Perrow, State University of New York at Stony Brook
November 1978, 270 pages, softbound

For further information write
Jennifer Toms, Department SA
1900 East Lake Avenue
Glenview, Illinois 60025

SF
Scott, Foresman and Company

ROUTLEDGE &
KEGAN PAUL **9 Park Street, Boston, Mass. 02108**

GRAMSCI AND MARXIST THEORY
EDITED BY CHANTAL MOUFFE *Visiting Lecturer,*
City University, London

This selection of essays by leading scholars provides an excellent and timely introduction to the work of Antonio Gramsci, focusing in particular on his advances in the study of the superstructures, and his relation to Marx and Lenin and his influence on Eurocommunism.

$24.50, *paperback* $12.95

ESSENTIAL INTERACTIONISM ***On the Intelligibility of Prejudice***
BARRY GLASSNER *Assistant Professor of Sociology,*
Syracuse University, New York

This book considers the purported instability of social science concepts by presenting a critique of causal and symbolic interactionist sociologies, and proposes in their stead a form of essentialism for sociology.

$21.00

PROBLEMS OF A SOCIOLOGY OF KNOWLEDGE
MAX SCHELER *Translated by Manfred S. Frings*

This work, which first appeared in 1924 and has never been published in an English translation, marks the first major investigation of its kind into the sociology of knowledge. In it Scheler challenges the social theories of Auguste Comte and Karl Marx, and expounds his belief that the sociology of knowledge is a major part of the sociology of culture and the foundation for all more specialized studies in the sociology and philosophy of religion, education, language and science.

International Library of Sociology $25.00

SOCIOLOGY OF YOUTH CULTURE AND YOUTH SUBCULTURES
MIKE BRAKE *Associate Professor of Social Work,*
Carleton University, Ottawa

Dr. Brake argues that subcultures arise as responses to collectively experienced problems resulting from defects and contradictions in the social structure. He discusses ethnic minorities and the problems facing them, femininity versus the *machisimo* identity, the emancipation of girls from the cult of romance and marriage, and the development of a new form of femininity.

$25.00, *paperback* $11.50

POLITICAL MANIPULATION AND ADMINISTRATIVE POWER

A Comparative Study

EVA ETZIONI-HALEVY ***Lecturer in Sociology, Australian National University***

Dr. Etzioni-Halevy examines the means by which elites in Western-style democracies have established and maintained themselves in power. She describes how they have manipulated the public by methods which run counter to the spirit and the letter of democracy without overtly disrupting the system. *International Library of Sociology* $22.50

VIOLENT CRIMINAL ACTS AND ACTORS

A Symbolic Interactionist Study

LONNIE H. ATHENS ***Professor of Sociology, Wayne State***

From the Foreword by Herbert Blumer: 'Dr Athens's study of the violent action of criminals is to the best of my knowledge the only such study that has been made from the standpoint of symbolic interactionism. It is truly a pioneering effort and a rewarding effort. Without any previous study to chart the way, he has had to carve out his own scheme of analysis ... it opens the door to a much-needed form of study in the grand task that confronts criminology.'

International Library of Sociology $11.50

INEQUALITY, CRIME AND PUBLIC POLICY

JOHN BRAITHWAITE ***Research Criminologist, Australian Institute of Criminology***

This study attempts to integrate and interpret the vast corpus of existing research evidence on social class, slums and crime. In so doing it provides the most comprehensive and systematic review available of the empirical evidence to support or refute the classic theories of Engels, Bonger, Merton, Cloward and Ohlin, Cohen, Miller, Shaw and McKay, as well as many more recent formulations. $32.50

OFFICIAL DISCOURSE *On Discourse Analysis, Government Publications, Ideology and the State*

FRANK BURTON ***Lecturer in Sociology, The City University, London;*** **and PAT CARLEN** ***Lecturer in Criminology, University of Keele***

This book is an unofficial report of theoretical investigations into a specific state practice — the publication of reports of official inquiries into law, order and justice issues.

International Library of Sociology $20.00

F.E. PEACOCK PUBLISHERS, INC.
is pleased to announce for publication in
the Spring

SOCIAL RESEARCH: Guides to a Decision-Making Process
By **Susan Gustavus Philliber,** Columbia University; **Mary R. Schwab,** University of Cincinnati; and **G. Sam Sloss,** Indiana University Southeast

This is an introductory text for a course in social research. It addresses the necessary steps in planning, conducting, evaluating, and presenting social research data. It covers the nature of knowledge and its application to social research; sources of research problems and influencing factors in their selections; understanding measurement issues; types of research design and factors to weigh in their use; techniques for collecting research data; understanding and utilizing data analysis; and ways to organize, summarize and present research findings.
February 1980 c. 200 pages c. $5.95 paper

THEORIES OF DEVIANCE, Second Edition
Edited by **Stuart H. Traub** and **Craig B. Little,** SUNY-Cortland

Theories of Deviance (revised edition) is a collection of seminal theoretical statements from diverse sociological perspectives. The flow of the book follows the development of the sociology of deviance from 19th century functionalism to contemporary explanations emphasizing political-economy. The book's aim is to provide students with an opportunity to recognize cross-fertilization, compatibility and contrast among the theorists whose work has been chosen. A brief introductory essay at the beginning of the book and introductions by the editors preceding each chapter help guide the students' reading. A *unique* feature of this edition is the inclusion of readings which analyze and critique each of the approaches to explaining deviance.
February 1980 c. 350 pages c. $8.95 paper

F. E. PEACOCK PUBLISHERS, INC. ITASCA, IL 60143

NEW TEXT WITH A SOLID "SOCIAL FACTS" APPROACH GIVES YOUR STUDENTS *TODAY'S* SOCIAL REALITIES

SOCIOLOGY: An Introduction

Michael S. Bassis, University of Rhode Island; **Richard J. Gelles,** University of Rhode Island; and **Ann Levine,** Professional Writer

This important new text gives in-depth coverage of basic topics for the introductory course. At the same time, its "social facts" orientation shows students, as in no other text, the *value* of the science and methodology of sociology in challenging myths and supporting reality. It does this through the innovative and fascinating device of first presenting commonly held *assumptions* (e.g., most only children are socially and intellectually backward; most murder victims are killed by strangers), then giving the often startling social *facts* as revealed by research findings (only children tend to have *higher* I.Q.'s and *stronger* self-concepts than those with siblings; most murder victims *know* their killers). These facts, in turn, are used as unforgettable illustrations of how social forces operate in all areas of human life.

SOCIOLOGY: *An Introduction* also features:

- sixteen-chapter division appropriate for a one-quarter as well as a full-semester course;
- real-life current examples and applications throughout, from comparisons between Vietnamese and American culture to the Jonestown tragedy as collective behavior;
- material on social change integrated into each chapter;
- thematic series of boxes—cross-cultural, subcultural, and historical—in each chapter;
- time lines placing each chapter's material in historical context;
- full graphics program of photos, charts, and graphs;
- Instructor's Manual and Student Study Guide.

Random House/February 1980/544 pages hardbound/Order Codes: 32259, 32509 (I.M.), 32510 (S.G.)

For more information, write:

Random House
College Review Desk, 400 Hahn Road, Westminster, MD 21157

WADSWORTH PUBLISHING COMPANY:
You Know Us by Our Authors

A textbook is only as good as its author. At Wadsworth we work with the best authors we can find, and these texts verify that we find the **best.**

New in 1980!

Sociology: An Introduction, Second Edition
Earl R. Babbie

One of the most readable and teachable texts of the 1970s takes a fresh and informative look at sociology in a New Second Edition. A broader research base with a 25% increase in references, a reorganization that explores Deviance after Socialization and distinguishes clearly between Organizational and Institutional Agreement, and new chapters on such topics as Groups, Formal Organizations, and Urban Society all add up to an insightful perspective for students.

There's **more**—boxes capsulize recent sociological studies, short stories by such noted authors as Kurt Vonnegut, Jr. dramatize sociological concepts, and a new design with numerous photographs and illustrations highlights key material. The list goes on and on, and we think you'll appreciate the new look this text gives to the 1980s. Teacher's Manual. Student Study Guide. Test Items.

Sociology: Concepts and Characteristics, Fourth Edition
Judson R. Landis

The best-selling **brief** introductory sociology text is now in a New Fourth Edition with provocative new readings, coverage of contemporary topics of concern, full page photographs, and a complete updating throughout. Teacher's Manual.

The Individual, Marriage, and the Family, Fourth Edition
Lloyd Saxton

Extensive updated research, a new art and photo presentation, and the same objective **facts** that made Saxton a household word. Teacher's Manual. Study Guide.

Social Psychology as Social Process
Donald E. Allen, Rebecca F. Guy, and Charles K. Edgley

The most current social psychology text that recognizes the contributions of sociologists to the field. An introduction from a symbolic interactionist perspective. Instructor's Manual.

Crime and Deviance in America: A Critical Approach
Sheila Balkan, Ronald J. Berger, and Janet Schmidt

Coverage of standard criminology topics within a pure, critical approach to the study of crime.

Social Research for Policy Decisions
Kurt Finsterbusch and Annabelle Bender Motz

Analyzes three major social research contributions to the policy decision process—social problem analysis, social impact assessment, and program evaluation.

For more information on these or other texts, please write to:
Wadsworth Publishing Company
A Division of Wadsworth, Inc.
Box ASR-F80
10 Davis Drive
Belmont, California 94002

New Viewpoints on Sociology, Psychology, and Social Work

INTRODUCING SOCIAL PSYCHOLOGY

EDWARD E. SAMPSON. Who Are You? Organized around this basic question, *Introducing Social Psychology* provides a unique and effective introduction to the major topics, theories, and research findings in social psychology. Six major areas are covered: culture and history; family; sex, sex roles, and sexism; attitudes, beliefs, and values; memberships, associations, and roles; personality and personal style. Here is a truly social view of the study of human behavior. Bibliography. Index.
February 1980, 288 pp.

Hardcover ISBN: 531-05413-6	$15.00
Paperback ISBN: 531-05627-9	$ 8.95

PSYCHOTHERAPY: Current Perspectives

THOMAS J. COTTLE and PHILLIP WHITTEN, Editors. The major issues, purposes, and forms of psychotherapy, plus some of the underlying philosophical principles, are clearly presented. Also discussed are various types of psychotherapy—from classical psychoanalytic to the more modern forms of group psychotherapy, sex therapy, bioenergetics, and family therapy. Finally, the book examines psychotherapy's own findings about human behavior. 305 pp.

Hardcover ISBN: 531-05411-X	$12.95
Paperback ISBN: 531-05625-2	$ 6.95

THE WORK OF SOCIAL WORK

ALLAN R. MENDELSOHN. Based on the first-hand experiences of a practitioner, educator, and consultant in the field, this study presents a no-nonsense, realistic picture of social work operations—their value base, knowledge base, and skill base.

What is social work? How does the actual practice differ from theoretical discussions of the occupation? This introductory textbook examines these important questions and will be of tremendous value to undergraduate and graduate students preparing to enter this profession. Index.
February 1980, 192 pp.

Hardcover ISBN: 531-05412-8	$12.50
Paperback ISBN: 531-05626-0	$ 6.95

For more information or to order
books, write to: Dept. NS
New Viewpoints
A Division of
Franklin Watts, Inc.
730 Fifth Avenue
New York, New York 10019

HELP: A Working Guide to Self-Help Groups

ALAN GARTNER and FRANK RIESSMAN. Now you can find them: self-help groups for every major disease, stage of life, and type of social/economic/marital status. Complete with: people to contact, phone numbers, addresses, purposes; and a section on how professionals can help these groups function effectively. Photographs. (A Vision Book)
January 1980, 192 pp.

Paperback ISBN: 531-06751-3	$9.95

STEPFAMILIES: A Cooperative Responsibility

FREDRICK CAPALDI and BARBARA McRAE. "The approach is positive, and the book is recommended for stepparents and single parents considering remarriage... a readable, nontechnical manual that emphasizes communication and an orderly solution to problems."—*Library Journal.*
"... A real gift of the book is its ability to place otherwise tension-ridden topics squarely out where they can be examined and discussed."—*Continuum.* (A Vision Book)
Available now, 154 pp.

Hardcover ISBN: 531-06373-9	$10.95
Paperback ISBN: 531-06750-5	$ 6.95

Recently published:

CRIMINAL JUSTICE: Issues and Ironies
2nd Edition
By Abraham S. Blumberg
432 pp.

Hardcover ISBN: 531-05407-1	$15.00
Paperback ISBN: 531-05618-X	$ 9.95

WOMANHOOD IN AMERICA: From Colonial Times to the Present
2nd Edition
By Mary P. Ryan
304 pp.

Paperback ISBN: 531-05624-4	$6.95

CITIES IN TRANSITION: Social Changes and Institutional Responses in Urban Development
By Peter R. Gluck and Richard J. Meister
256 pp.

Hardcover ISBN: 531-05409-8	$15.00
Paperback ISBN: 531-05623-6	$ 6.95

GOOD NEWS from Little, Brown

NEW for Social Problems

SOCIAL PROBLEMS
by Burton Wright and John P. Weiss. cloth/c. 610 pages/$15.95 tentative/ with Study Guide and Instructor's Manual

This new introductory text is based on the premise that in order to deal with social problems, we must first understand the social forces that cause and perpetuate them. The authors explore 15 social problems in 13 chapters and provide a unique and useful **integrating framework** for study. Each chapter includes an overview, an introduction, a historical overview, a conceptual orientation, the objective dimensions of the problem, attempts at dealing with the problem, and prospects for future solutions. SOCIAL PROBLEMS presents an orderly, objective approach to some knotty problems. Be sure to examine it soon.

NEW from John Irwin

PRISONS IN TURMOIL
by John Irwin. paper/c. 320 pages/$6.95 tentative

This fascinating account of how prisoners cope with prison life was written by one of this country's foremost authorities on prison. As a scholar and a former inmate, Irwin is uniquely able to translate his own experiences into an authoritative analysis of social change in the prison world. He explores the social structures that emerge in prisons, and analyzes them in their social and historical contexts. Controversial, provocative, and perceptive, PRISONS IN TURMOIL is the **ideal supplement** for a variety of courses.

All of these new titles are now available for examination.

NEWLY Revised for Marriage and Family

FAMILY IN TRANSITION: Rethinking Sexuality, Child Rearing, and Family Organization
Third Edition, by Arlene Skolnick and Jerome H. Skolnick. paper/c. 640 pages/$9.95 tentative/with Text Bank

In its new edition, the **most popular reader** for marriage and family courses continues to examine changes that have occurred in the family over the last decade, focusing on sexuality and sex roles, childhood and parenthood, the nature of the family, and social change. More than 70% of the material is new to this edition, including articles on love, coupling, divorce, divorce law, and remarriage.

NEW for the Sociology of Aging

SOCIAL GERONTOLOGY: An Introduction to the Dynamics of Aging
by David L. Decker. cloth/ c. 400 pages/$14.95 tentative/with Test Bank

Using a social and historical approach, this text explores the biological, economic, political, and psychological aspects of aging. A detailed analysis of the demographic shifts that have occurred within the last century provides a basis for examining the growing significance of aging as a social phenomenon. Social problems of the aged such as crime and victimization, medicare abuse, alcoholism, and suicide are treated in a separate chapter.

NEW for the Sociology of Health

THE SOCIOLOGY OF HEALTH: Principles, Professions, and Issues
by Fredric D. Wolinsky. cloth/c. 480 pages/$15.95 tentative

This comprehensive and readable examination of health-related behavior focuses on major principles, professions, and issues within the expanding field of medical sociology. In addition to discussing the social organization of health care, the author provides an in-depth analysis of the relationship between social factors and health.

All prices are subject to change without notice.

COLLEGE DIVISION
LITTLE, BROWN AND COMPANY
34 Beacon Street, Boston, Massachusetts 02106

Announcing Two Volumes in the STUDIES IN POPULATION Series
Series Editor: H. H. WINSBOROUGH

Measuring Underemployment

Demographic Indicators for the United States

By CLIFFORD C. CLOGG

CHAPTER HEADINGS: Introduction. Underemployment in the United States, 1969–1973. Information in the Measures of Invisible Underemployment. The Effect of Changing Demographic Composition on the Crude Indicators of Underemployment. Latent Class Structure in the Distribution of Labor Market Rewards. The Dependence of Labor Force Status on Age, Time-period, and Cohort. Trending and Forecasting Underemployment: Log-Linear Models and Population Projection. Underemployment in the Life History of Synthetic Cohorts. Some New Directions for Mathematical Demography. Conclusion. Appendices. References. Index.

Conclusions appear at the end of each chapter.

1979, 288 pp., $21.50 ISBN: 0-12-176560-1

Residential Consumption, Economic Opportunity, and Race

By FRANKLIN D. WILSON

CHAPTER HEADINGS: Overview: Residential Consumption, Economic Opportunities, and Race. DETERMINANTS OF THE DEMAND FOR RESIDENTIAL SERVICES. Methodology: Demand for Residential Services: The Demand for Housing Services. The Demand for Neighborhood Services. Residential Location: Central City versus Suburbia. Journey to Work. Race and Residential Behavior. Implications of Findings for Determinants of the Demand for Residential Services. INTRAMETROPOLITAN LOCATION AND ECONOMIC WELL-BEING. The Spatial Structure of Economic Opportunity in Metropolitan Areas. Wage Differentials: Race, Residence, and Workplace Analyses of Wage Differentials. Alternatives for the Improvement of Black Residential Consumption and Employment. References. Subject Index.

1979, 240 pp., $17.00 ISBN: 0-12-757980-X

Welfare of the Poor

By MARY BRYNA SANGER

CHAPTER HEADINGS: WELFARE CASELOAD DYNAMICS. Caseload Variations: The Conceptual Issues and Methodological Techniques. Variations Within the Welfare Population. EMPLOYMENT. Measuring Employability. Factors Affecting Work and Welfare Patterns. FAMILY COMPOSITION. Introduction. Determinants of Marital Instability. The Effects of the Welfare System on Remarriage. The Effects of the Welfare System on Illegitimacy. The Effects of the Welfare System on Fertility. HEALTH. Introduction. Health Definitions. Nutrition: A Causal Factor. Utilization of Health-Care Services Among the Poor. EDUCATION. Introduction. Socioeconomic Determinants. Inequality of School Resources. Institutional Determinants. CONCLUSION. References.

Conclusions appear at the end of each chapter.

1979, 192 pp., $15.00 ISBN: 0-12-618650-2

Class Structure and Income Determination

By ERIK OLIN WRIGHT

A Volume in the INSTITUTE FOR RESEARCH ON POVERTY MONOGRAPH Series

CONTENTS: THEORETICAL PERSPECTIVE. What Is Class? Classes in Advanced Capitalist Societies. Theoretical Perspectives on Income Inequality. Class and Income: Hypotheses. THE EMPIRICAL INVESTIGATION OF CLASS MEDIATIONS OF THE INCOME DETERMINATION PROCESS. Class and Occupation. Class and Income. The Managerial Hierarchy. Race and Class. Class and Sex. Conclusions. Appendices. References. Index.

1979, 288 pp., $21.00 ISBN: 0-12-764950-6

Surveys by Telephone:

A NATIONAL COMPARISON WITH PERSONAL INTERVIEWS

BY ROBERT M. GROVES and ROBERT L. KAHN

A Volume in the QUANTITATIVE STUDIES IN SOCIAL RELATIONS Series

CHAPTER HEADINGS: Measuring Populations in Person and on The Telephone. Sample Designs for Personal and Telephone Interview Surveys. Administration of the Surveys. Response Differences between the Two Modes of Data Collection. Method Effects Associated with Specific Types of Measures. Measurable Sources of Error in the Survey Data. Sampling and Data Collection Costs for the Surveys. Summary and Speculation. Appendices. References. Index.

Summaries are included at the end of each chapter.

1979, 384 pp., $21.50 ISBN: 0-12-304650-5

Two titles in the LAW, STATE AND SOCIETY Series

Reading Ideologies

An Investigation Into the Marxist Theory of Ideology and Law

By COLIN SUMNER

CONTENTS: The CONCEPT OF IDEOLOGY. Introduction. Ideology and Social Practice. METHODS OF READING IDEOLOGY. Content Analysis: Speculation Disguised as Science and Art. The Analysis of Form: Structuralism. The Semiology of Roland Barthes. Neo-structuralism: The Marxist Avant-garde. READING IDEOLOGIES IN THE LAW: AN HISTORICAL MATERIALIST METHOD. An Historical Materialist Method of Reading Ideologies. The Ideological Composition of Law. Summary and Final Reflections. Index.

1979, 328 pp., $25.50 (Cloth)
ISBN: 0-12-676650-9

328 pp., $11.50 (Paper) ISBN: 0-12-676652-5

Marx and Engels on Law

By MAUREEN CAIN and ALAN HUNT

CONTENTS: YOUNG MARX, YOUNG ENGELS. Extracts: Marx, the Student. The Trial of Hegelianism. On Social Morality and the Early Engels. The Experience of Law. LAW IN HISTORICAL MATERIALISM. Extract. LAW AND ECONOMIC RELATIONS. Extracts: Law and the Transition to Capitalism. Law and Bourgeois Economic Interests. Law and Property Relations. Regulation of Labour. IDEOLOGY. Extracts: The Form of Ideology. The Effectivity of Ideology. The Content of Ideology. Legal Thought. Lawyers. THE STATE, LAW AND CRIME. Extracts: The State and Law. Law Repression. Crime and Criminality. Punishment. LAW AND POLITICS. Extracts: The Struggle for the Constitution. The Uses of Law. Sources. Index.

1979, 308 pp., $20.25 (Cloth)
ISBN: 0-12-154850-3

$10.00 (Paper) ISBN: 0-12-154852-X

Continuation Orders authorize us to ship and bill each volume in a series, or "Advances" type publication automatically, immediately upon publication. This order will remain in effect until cancelled. Specify the volume number or title with which your order is to begin.

Send payment with order and save postage and handling charge.
Orders under $15.00 directed to New York must be prepaid.
Prices are subject to change without notice.

Academic Press, Inc.
A Subsidiary of Harcourt Brace Jovanovich, Publishers
111 FIFTH AVENUE, NEW YORK, N.Y. 10003
24-28 OVAL ROAD, LONDON NW1 7DX

CHARLES C THOMAS • PUBLISHER

GAMBLING TODAY

Edited by **David Lester,** *Richard Stockton State College, Pomona, New Jersey. (8 Contributors)* Gambling and gambling behavior are herein analyzed for their economic, sociological, criminological, psychological, and psychiatric ramifications.

The first three chapters of this timely book explore the potential impact of an increase in legal, as well as illegal, gambling. Specific topics include the social impact of the opening of the first casino in Atlantic City, the economic impact of casinos for communities interested in the tax revenues generated by legalized gambling, and the problems that legal and illegal gambling activities present for law enforcement officers.

The next three chapters present sociological motives for gambling and pertinent psychiatric and psychological theories. Among the questions addressed are *Why do people gamble? Is gambling disturbed or healthy behavior?* and *Can gambling be predicted on the basis of the payoff arranged by those who control the gambling establishments?*

Remaining chapters delineate treatment techniques for compulsive gamblers; the philosophy, experiences, and results of Gamblers Anonymous; and a cross-cultural analysis of gambling in Great Britain. *'79, 176 pp., 4 tables, $12.75*

OLD AND GROWING: Conversations, Letters, Observations, and Reflections on Growing Old *by* **Florence E. Vickery,** *College of Marin, Kentfield, California. Foreword by* **Jack Ossofsky.** Physical, psychological, and social aspects of the aging process are herein explained in everyday language and highlighted by glimpses into the lives of real people. Topics include retirement, changing roles in marriage and family, sexuality, loss of a spouse, bereavement and remarriage, loneliness, reengagement in new relationships, maintenance of health and well-being, and dealing creatively with death. *'78, 176 pp., 9 il., $13.00*

AN ECONOMIC ANALYSIS OF CRIME: Selected Readings *edited by* **Lawrence J. Kaplan** *and* **Dennis Kessler,** *both of City Univ. of New York, New York City. (25 Contributors)* This text discusses the activities and operations of organized crime, its economic implications, and its impact on the American consumer. Among the specific crimes examined are gambling, loansharking, narcotics, labor union corruption, crimes infiltrating legitimate business, pornography, and prostitution. *'76, 432 pp., 4 il., 9 tables, cloth-$22.25, paper-$16.50*

THE SEXUAL VICTIMOLOGY OF YOUTH *edited by* **LeRoy G. Schultz,** *West Virginia Univ., Morgantown, West Virginia. (21 Contributors)* The first section of this book considers the legal control of the sexual abuse of children. A short history of efforts in this field are presented along with discussions of the sexual victimization and molestation of children. Following sections cover medical and psychological diagnosis and treatment, incest, the victim and the justice system, the child sex industry, and sexual emancipation. The last of these constitutes a critical evaluation of age-of-consent and statutory rape laws. *'80, 456 pp., 1 il., 6 tables, $17.50*

THE EFFECTIVENESS OF SOCIAL CASEWORK *by* **Joel Fischer,** *Univ. of Hawaii, Honolulu. Foreword by* **H. J. Eysenck.** This work covers one of the most important topics in social work today—effectiveness of practice. The author, in the first part of this text, summarizes and analyzes all of the controlled research on casework effectiveness. In the following part, eleven leading social work theorists, researchers, educators, and practitioners respond to and critique the research analysis and discussion of Part I. *'76, 360 pp., 15 il., 7 tables, $25.00*

SOCIAL PSYCHOLOGICAL PERSPECTIVES ON AND RECREATION *edited by* **Seppo E. Iso-Ahola,** *Univ. of Iowa, Iowa City. (18 Contributors)* Contributors to this book review theoretical and empirical research and propose new directions for the leisure/recreation field. Practical implications of research findings receive special attention. The first of four sections deals with basic aspects, including theory and methodology. Succeeding sections cover the process of leisure socialization; the interdependent influences of social cognition, interaction, and leisure satisfaction; and the interdisciplinary nature of leisure studies. *'80, 448 pp., 6 il., 10 tables, $19.75*

PSYCHOTHERAPY FOR WOMEN: Treatment Toward Equality *edited by* **Edna I. Rawlings,** *Univ. of Cincinnati, Cincinnati, Ohio, and* **Dianne K. Carter,** *Univ. of Iowa, Iowa City. (22 Contributors)* A reflection of the feminist philosophy, this book provides alternatives to what the authors consider to be inadequate psychotherapeutic treatment of women today. They examine the feminist ideology and its relationship to such topics as assertion training, careers, therapy for lesbians, and social action. *'77, 500 pp. (6 3/4 x 9 3/4), 10 il., 16 tables, $22.25*

THE EVOLUTION AND CHEMISTRY OF AGGRESSION *by* **Delbert D. Thiessen,** *Univ. of Texas, Austin. Foreword by* **I. Newton Kugelmass.** The author of this book reviews the phylogenetic history of aggression and emphasizes its adaptive significance. Central to the book is the theme that human aggression is partially determined by genetic factors mediated through biochemical and morphological attributes. The effects of hormones and sex steroids on aggressive patterns are examined, as are brain biochemistry, the ways that pharmacological substances affect aggression, and the significance of chemosignals in directing aggression. *'76, 232 pp., 38 il., 27 tables, $23.75*

FAMILY VIOLENCE *by* **George Thorman,** *St. Edward's Univ., Austin, Texas.* Basic information is herein presented on the causes, treatment, and prevention of family violence. Initially, the author constructs a theoretical framework within which domestic violence can be understood. The nature and extent of child abuse and wife beating are reviewed, and statistical data and conclusions that can be drawn from them are given. Then, in separate segments on abused children and battered spouses, he focuses on aggressor and victim characteristics, including social/cultural factors and psychological profiles. A consideration of methods of treatment and prevention concludes the text. *'80, 208 pp., 1 table, $15.50*

DRUGS AND THE ELDERLY: Social and Pharmacological Issues *edited by* **David M. Petersen, Frank J. Whittington** *and* **Barbara P. Payne,** *all of Georgia State Univ., Atlanta. Foreword by* **Peter G. Bourne.** *(18 Contributors)* This text addresses such issues as drug side effects, drug interactions, self-medication, and community and institutional pharmacy. A final section emphasizes the responsibilities of the clinic and community with regard to drugs and the elderly and explores such related topics as physician and nurse responsibilities for educating the elderly about drugs, strategies for drug education, and social policy concerns and suggestions. *'79, 280 pp., 5 il., 9 tables, $26.25*

CONTEMPORARY SOCIAL GERONTOLOGY: Significant Developments in the Field of Aging *edited by* **Bill D. Bell,** *Univ. of Nebraska, Omaha. Foreword by* **Erdman Palmore.** *(61 Contributors)* The entire spectrum of gerontological concerns is herein covered, including the demography and ecology of age; theories of aging; economics, housing, and health; work, retirement, and leisure; and attitudes toward age and aging. Contributors also discuss adjustment and life satisfaction and the institutionalized aged. *'76, 480 pp. (6 3/4 x 9 3/4), 11 il., 99 tables, $36.25*

Orders with remittance sent, on approval, postpaid

301-327 East Lawrence Avenue • Springfield • Illinois • 62717

NEW FROM HARPER & ROW

In this long-awaited volume, noted British sociologist Tom Bottomore presents a wide-ranging examination of political sociology, or political science, from the viewpoints of competing schools of social theory. He confronts the different paradigms with each other and shows them in the context of an historical process that embraces not only the progress of the science itself, but also the transformation of its external environment as a result of economic, political, and cultural changes.

hardcover, $10.00
paperback CN751, $3.95

Political Sociology
Tom Bottomore

POWER IT'S FORMS, BASES, AND USES
DENNIS H. WRONG

Dennis Wrong offers a penetrating analysis of the elusive concept of power in social theory. He discusses a typology of diverse forms of power —authority, force, manipulation, etc.—then shows how the various forms blend and interact in real life. Exploring the concept of power, he shows how it can be used to examine the central issues of political sociology. In an important final chapter, he deals with power as a goal, and examines the roots in human nature of striving for power as a motive.

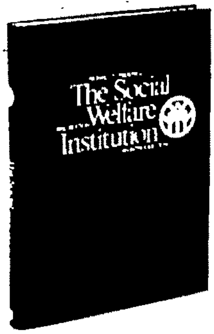
paperback CN702, $6.95

At your bookstore or from

Harper & Row
10 E. 53d St., New York 10022

New Perspectives on Sociological Issues. . .

The Social Welfare Institution: An Introduction Third Edition



Ronald C. Federico
University of Cincinnati
1980/Casebound/416 pages

A basic introduction to social welfare institutions, this text combines both theoretical and practical issues. It retains the outstanding features of previous editions and adds new material such as a chapter on the helping process, a chapter on social work practice methods, a new appendix, and a new Instructor's Guide.

Delinquency: A Sociological View

Gary F. Jensen, University of Arizona
Dean G. Rojek, University of Georgia
1980/Casebound/352 pages

This text presents a systematic sociological analysis of juvenile delinquency with careful attention given to treatment and prevention. It examines the origins of adolescent misbehavior; appraises society's reaction to the problem; and discusses the success of the current system of juvenile justice in dealing with the problem.

Criminology: Perspectives on Crime and Criminality

Peter Wickman, State University of New York, Potsdam;
Phillip Whitten, and *Robert Levey*
1980/Casebound/640 pages

Written for the beginning student in criminology, the text covers major perspectives, issues, and the diverse concerns and debates that characterize contemporary criminology. The balanced "humanistic" view of criminology that is critical as well as current is designed to stimulate the student's systematic study of crime, criminality and criminal behavior.

For details or sample copies, call us toll free: 800-225-1388.
In Massachusetts, call collect: 617-862-6650, ext. 1344.

125 Spring Street **Lexington, Massachusetts 02173**

From D. C. Heath!

SINCE 1478

Oxford University Press

Theoretical Criminology
Second Edition

GEORGE B. VOLD. Revised by **THOMAS J. BERNARD,** Guilford College. The standard work in its field now appears in a thoroughly revised second edition, expanded to include significant new developments in all major areas of criminological thinking. Retaining the style and historical framework of the first edition, Bernard incorporates the latest research in such disciplines as genetics, neurophysiology and learning theory in an intensive account of the major criminological theorists. New chapters discuss the anomie tradition, the ecology of crime, deviance and social reaction, and contemporary conflict and radical criminology. Annotated bibliographies follow each chapter.

November 1979 448 pp.; figures $14.95

Crime Control Strategies
An Introduction to the Study of Crime

HAROLD E. PEPINSKY, Indiana University. How can a knowledge of criminology be used to change the rates of crime and criminality, and what would the consequences of such change be? Pepinsky provides the first comprehensive introduction to the practical questions of crime control. Engagingly written, the text is designed to develop the reader's ability to critically analyze the issues of crime control in the context of society at large.

January 1980 384 pp. paper $9.95

Police Behavior
A Sociological Perspective

Edited by **RICHARD J. LUNDMAN,** Ohio State University. Research on police behavior has increased dramatically during the past fifteen years. This course-oriented anthology of eighteen articles samples recent studies on police decision-making, police-citizen interaction, police discretion, brutality and corruption. In his introductory commentaries on the readings, Lundman emphasizes the environmental and organizational roots of police conduct and misconduct.

January 1980 320 pp. paper $4.95

Women, Crime, and Justice

Edited by **SUSAN K. DATESMAN,** Arizona State University, and **FRANK R. SCARPITTI,** University of Delaware. The study of female crime has become one of the most popular topics in contemporary criminology. The fourteen articles collected here focus on the etiology of female crime, patterns of female criminal behavior, and the treatment of women in the criminal justice system. In addition, the editors have written a substantial introduction analyzing crime trends and the nature of female criminality, and a comprehensive concluding essay on the relation between women's crime and the women's movement.

January 1980 384 pp.; 19 tables paper $5.95

Suburban Youth in Cultural Crisis

RALPH W. LARKIN, Rutgers University. "A rare find—one that combines ethnographic sophistication with macro-social awareness. His book is not only relevant to education but to any student of socialization, social psychology or of American society." —Peter Freund, Montclair State College. "The best and most solid study of the tensions underlying the apparent but spurious resignation of American young people in the face of social and political insult."—Edgar Z. Friedenberg, Dalhousie University.

1979 272 pp. cloth $10.95 paper $4.95

Introductory Sociology

Order and Change in Society

Third Edition

GERALD R. LESLIE, University of Florida; **RICHARD F. LARSON,** Clemson University; and **BENJAMIN L. GORMAN,** University of Florida. *Introductory Sociology* is designed for students in the general survey course, majors and nonmajors alike. Stressing the interrelations of social life, social processes, and social institutions, the text thoroughly integrates major theoretical models and research methods. All the major substantive areas of contemporary sociology are covered. Clearly written in an engaging style, the text leads the student from basic terms and concepts to the analysis of complex social entities. Outlines at the beginning of each chapter, and summaries, lists of important terms, annotated readings, questions, and projects at the end of each chapter assist the student in assimilating the fundamental notions of the discipline. Lavishly illustrated and handsomely designed, broad-ranging yet closely reasoned, *Introductory Sociology* presents sound sociological principles in a manner both comprehensible and relevant.

January 1980 608 pp.; 200 photographs, 50 figures cloth $17.95

Complimentary Instructor's Manual

Socguide

An Aid to Learning Sociology through Projects, Puzzles and Problems to Accompany Leslie, Larson and Gorman: *Introductory Sociology*, Third Edition

MARGARET-MARY FRANZ, California State University. This student workbook provides readings, writing and research assignments, short answer questions, quizzes with answers, and brief essays focussing on basic concepts, terms and theories in each chapter of *Introductory Sociology*.

January 1980 160 pp.; 22 figures paper $4.95

A Primer on Ethnomethodology

KENNETH C.W. LEITER, Southern Methodist University. Ethnomethodology is an attempt to get beneath social behavior to find out how people understand the patterns of daily life. Drawing on the latest research, the *Primer* is a lucid description of the ethnomethodological perspective. Through the detailed analysis of numerous examples, Leiter clarifies the central concepts and concerns of ethnomethodology and demonstrates its relevance to contemporary sociology.

January 1980 272 pp. cloth $14.95 paper $4.95

The Environment

From Surplus to Scarcity

ALLAN SCHNAIBERG, Northwestern University. Schnaiberg provides a social scientific perspective on the institutional roots of the environmental crisis. Rejecting popular explanations of environmental deterioration, he concentrates on the prevailing influence of the unequal distribution of income, consumption, and economic and political power. An analysis of the generation and distribution of social surplus unifies this discussion of environmental problems, their roots in industrial production, and the obstacles to movements for environmental reform.

January 1980 464 pp. cloth $14.95 paper $9.95

Prices and publication dates are subject to change.

Oxford University Press **200 Madison Avenue, New York, N.Y. 10016**

LEADING TEXTS IN SOCIOLOGY YEAR AFTER YEAR

1 2

NEW!

MARRIAGE, FAMILY, AND INTIMATE RELATIONSHIPS

Gary S. Belkin,
Long Island University
Norman Goodman,
State University of
New York at Stony Brook

A contemporary examination of the rich variety of marriage and family styles in American culture. The authors go beyond the typical whys and wherefores of "dating and mating" to consider the various social and psychological forces that shape the behavior of individuals in relationships. Their emphasis on communication and counseling as the means to the resolution of conflicts sets this book apart from others in the field. Its practical perspective allows students to apply the concepts and ideas to their own lives. Instructor's Manual.

1980 c. 448 pages Cloth

7 8 9

NEW!

THE SOCIOLOGICAL METHOD:

An Introduction to the Science of Sociology, Third Edition

Stephen Cole,
State University of
New York at Stony Brook

This major revision guides students in introductory courses toward a conceptual understanding of the empirical foundations of social science. Drawing on his own extensive experience as both an instructor and as a productive social researcher, the author discusses the logic that underlies the *doing* of sociology and clearly illustrates the principles of the sociological enterprise.

1980 c. 180 pages Paper

14 15 16

SOCIAL PROBLEMS:

A Policy Perspective, Third Edition

Howard E. Freeman,
University of
California at Los Angeles
Wyatt C. Jones,
Brandeis University
Lynne G. Zucker,
University of
California at Los Angeles

A clear, interesting, and eminently readable Introduction to the study of social problems and social policy. Content has been updated to cover topics of current interest, including recent efforts directed at social action and change. Numerous pedagogical features and dramatic, issues-oriented photographs promote learning and retention. Instructor's Manual.

1979 579 pages Cloth

21 22 23

1980

3 **4** **5**

NEW!
THE FAMILY:
A Sociological Interpretation, Third Edition
Bert N. Adams,
University of Wisconsin at Madison

This classic among theoretical family sociology texts has been thoroughly revised and updated to include citations from the most recent research in the field. Retaining the concise nature and highly readable style of the previous editions, the book features new chapters on a variety of current concerns, expanded discussions and new sections on numerous other topics. Instructor's Manual.
1980 c. 512 pages Cloth

10 **11** **12**

NEW!
INTRODUCTION TO SOCIOLOGY,
Fourth Edition
Jerry D. Rose,
State University College, Fredonia, New York

Redesigned and updated throughout, the new fourth edition is more topically comprehensive and concisely written than ever before. All major areas of the introductory course are covered in detail. Heavily illustrated with an outstanding selection of photographs, the book features new historical material, extensive citations of recent research, and expanded discussions of a variety of topics.
1980 c. 432 pages Paper

17 **18** **19**

THE SOCIOLOGICAL ORIENTATION:
An Introduction to Sociology, Second Edition
Stephen Cole,
State University of New York at Stony Brook

An in-depth treatment of the discipline's essential concerns in a flexible core text. Carefully chosen topics illustrate the sociological perspective at work. Updated and consistently readable throughout, the book is unique in its emphasis on sociological methods and the case study approach. Instructor's Manual.
1979 366 pages Paper

25 **26**

RAND McNALLY
COLLEGE PUBLISHING COMPANY
Box 7600/Chicago, Illinois 60680

TRADITION and *INNOVATION* for 1980

Focus on Society: An Introduction to Sociology, Second Edition

Norman W. Storer, Bernard M. Baruch College, City University of New York

Comprehensive and penetrating examination of the basic structures and dynamics of sociology. Clear writing and a carefully-developed presentation of a modified structural/functional perspective provide unity and coherence. New in this edition are: a chapter on socialization and changing sex roles, "focus" sections on key concepts and ideas, conceptual diagrams, end-of-chapter glossaries, and appendices on the empirical research in the field. Accompanied by an Instructor's Manual with tests.

(07337) 544 pp. paper *Available Now*

Introduction to Social Problems

Leonard Glick and Daniel E. Hebding, both of Community College of Philadelphia

The aim of this well-researched text-reader is to examine the nature and causes of, the interrelationships, and possible solutions to contemporary social problems. Emphasizes the sociological approach to the study of social problems, but also draws on evidence and theory from other fields. Excellent pedagogy includes lists of key terms at chapter-end, margin notes, chapter summaries. Comprehensive Instructor's Manual.

(02600) 544 pp. paper *Available Now*

Gerontology: A Behavioral Science Approach

Richard C. Crandall, Lake Superior State College

Comprehensive and thoroughly documented examination of issues from a variety of perspectives. Includes chapters on the chronological development of gerontology as a science and on the aged in preliterate society. Instructor's Manual with tests.

(01252) c. 496 pp. *Available Now*

The Social Reality of Death: Death in Contemporary America

Kathleen C. Charmaz, California State University at Sonoma

Important new book uses a distinctively sociological approach to integrate the interdisciplinary materials in the field of death and dying.

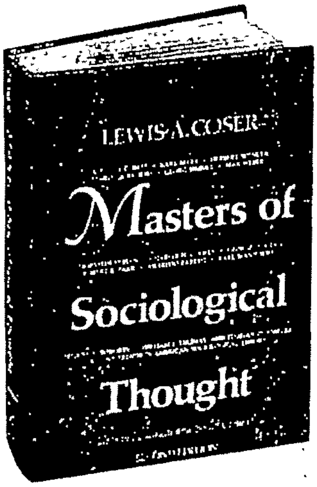
(01033) 352 pp. paper *Available Now*

For complimentary copy consideration, write to Abigail Winslow at Addison-Wesley Publishing.
Be sure to mention course title, enrollment, and author of current text.

Social Science & Humanities Division
Addison-Wesley Publishing Company
Reading, Massachusetts 01867

H J publishes th classics

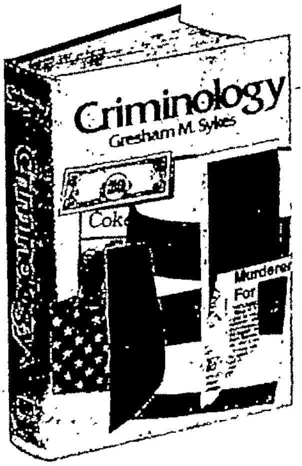
...the classic textbook on the evolution of sociological thought



Masters of Sociological Thought
Ideas in Historical and Social Context
SECOND EDITION
LEWIS A. COSER,
State University of New York, Stony Brook

A comprehensive presentation of sociological theory through in-depth study of the outstanding sociological theorists from the early 19th century to the present. "A superlative presentation, scholarly yet enjoyable to read."—Eric P. Godfrey, Ripon College

...the classic textbook on criminology



Criminology
GRESHAM M. SYKES, University of Virginia

A balanced introduction to criminology that provides broad coverage of the nature and causes of crime, the content of the criminal law, the workings of the criminal justice system, and society's various and changing reactions to crime and the criminal. "This is the most useful, balanced, and current text in criminology."—Roy Lotz, Washington State University

HARCOURT BRACE JOVANOVICH, INC.
New York • San Diego • Chicago • San Francisco • Atlanta

Central Problems in Social Theory

Action, Structure, and Contradiction in Social Analysis

Anthony Giddens

In this new and brilliantly original book of essays, Giddens discusses three main theoretical traditions in social science which cut across the division between Marxist and non-Marxist sociology: interpretive sociology, functionalism, and structuralism.

"One of the most creative among the younger generation of critical social theorists, Giddens stands alone in his concern for the classical tradition of sociology; but he also makes brilliant use of the latest philosophical and theoretical work of several contemporary schools and disciplines. A very important book for all of social science." —*Jeffrey C. Alexander*

$20.00 cloth, $8.95 paper

Poverty in the United Kingdom

A Survey of Household Resources and Standards of Living

Peter Townsend

The most detailed and comprehensive study of poverty ever undertaken for any society. It develops new measures, concepts, and methods which will be applicable to other settings and of direct comparative significance. It challenges many currently held ideas on the subject and will be an indispensable resource for policy makers as well as scholars.

$37.50 cloth, $15.95 paper

Catharsis in Healing, Ritual, and Drama

Thomas J. Scheff

What is catharsis? What are its effects and when does it occur? In this thoughtful and provocative study Scheff defines catharsis as a therapeutic process that discharges repressed emotions and is signalled by certain kinds of laughing, crying, and analogous responses reflecting discharge of fear and anger.

$12.95

The Fruited Plain

The Story of American Agriculture

Walter Ebeling

The drama and romance of American agriculture. Ebeling covers it all—from environmental problems to the effects of modern technology, from foreign land speculation to the back-to-the-land movement—in this exciting overview of agriculture in the U.S.A. He details the history and present status of agriculture for each of the seven geographic regions of the U.S.—from the Eastern seaboard to the Pacific Coast.

$22.50, illustrated

New in Paperback—

The Myth of Marginality

Urban Poverty and Politics in Rio de Janeiro

Janice E. Perlman

$4.95

At bookstores

University of California Press

Berkeley 94720

NOW AVAILABLE FOR THE CLASSROOM

THE SYMLOG CASE STUDY KIT

With Instructions for a Group Self Study

ROBERT F. BALES

The SYMLOG Kit provides a new and practical method for identifying group problems and solving them. This booklet clearly illustrates how a small group can observe and record its normal activities, and it contains all the forms necessary to undertake a group self study.

ISBN 0-02-910310-0 *$4.95*

WOMEN AND THE FUTURE

Changing Sex Roles in Modern America

JANET ZOLLINGER GIELE

"An interesting and useful effort to pull together much of the sociological literature on changing sex roles, to provide an interpretive framework in which these changes can be analyzed and understood, and to relate sex role changes to other important changes in modern industrial society."

—Anne Firor Scott, Duke University

ISBN 0-02-911690-2 *$6.95*

MARRIAGE AND FAMILY IN A CHANGING SOCIETY

Edited by JAMES M. HENSLIN

Unusually wide-ranging and contemporary in approach, this fresh and humane collection leads the student to a realistic understanding of marriage and the family today.

ISBN 0-02-914580-5 *$9.95*

THE MILITARY IN AMERICA

From the Colonial Era to the Present

Edited by PETER KARSTEN

Interweaving sociological and historical perspectives, the more than 40 articles in this book span 200 years of military history from colonial times to Vietnam and beyond. Critical issues affecting the military institution's image and role in contemporary society are examined in detail.

ISBN 0-02-916740-X *$9.95*

MANY SISTERS

Women in Cross-Cultural Perspective

Edited by CAROLYN J. MATTHIASSON

Written by and about women, this book movingly portrays the relationship of women to their society through 14 cross-cultural studies, revealing *what women do* and *how women feel.*

ISBN 0-02-920320-1 *$8.95*

READINGS IN MEDICAL SOCIOLOGY

Edited by DAVID MECHANIC

This rich assortment of 29 provocative readings introduces major issues and alternative perspectives on health, health care, and social policy.

ISBN 0-02-920700-2 *$9.95*

SOCIAL POLICY AND SOCIAL WELFARE

Structure and Applications

THOMAS M. MEENAGHAN &
ROBERT O. WASHINGTON

The authors create a general framework for analyzing social welfare policy, and demonstrate how this approach can be applied in specific areas of social service: social security, public welfare, community mental health, and programs for the elderly.

ISBN 0-02-920750-9 *$11.95*

THE SOCIOLOGY OF RACE RELATIONS

Reflection and Reform

Edited by THOMAS F. PETTIGREW

These readings chronicle the triumphs and failures of race relations in our nation, unfolding the tensions and struggles of racial conflict through the analyses of sociologists writing since the late nineteenth century.

ISBN 0-02-925110-9 *$9.95*

DETECTIVE WORK

A Study of Criminal Investigations

WILLIAM B. SANDERS

The first participant observation study of plain clothes detectives.
"The best writing about criminal investigation this reviewer has read ... a major contribution to the literature." —*Journal of Criminal Justice.*

ISBN 0-02-927590-3 *$6.95*

To order an examination copy, *indicate the course you are teaching and send your request to:*

THE FREE PRESS *A Division of Macmillan Publishing Co., Inc.,*
200D BROWN STREET, RIVERSIDE, N.J. 08370

CHICAGO
Since 1891, Publishers of Scholarly Books and Journals

The Calling of Sociology

and Other Essays on the Pursuit of Learning
Edward Shils

This third volume of the Selected Papers of Edward Shils brings together ten essays which deal with the history of American and European sociology as an intellectual undertaking and as a means to the attainment of practical ends. Professor Shils's main themes are the influence of ethical and practical intentions on scholarly study in the social sciences, the autonomy of the intellectual tradition of sociology, and the significance of the institutiona organization of sociological teaching and research.

Cloth 512 pages $25.00 February

Life Chances

Approaches to Social and Political Theory
Ralf Dahrendorf

Dahrendorf argues that the concept of life chances—the options that individuals in society face and the linkages which enable them to make their choices—should be a key concept for the understanding of social change.

Cloth 192 pages $15.00 Available

The Joyful Community

An Account of the Bruderhof—A Communal Movement Now in Its Third Generation
Benjamin Zablocki

"Although *The Joyful Community* is nominally about a religious cult, it is really an old fashioned community study—but one that is full of new insights, particularly on the relationships between community social structure and individual emotional structure. Ben Zablocki's book belongs on the shelves and reading lists of every social scientist concerned with communities and "community."—Herbert J. Gans, Columbia University

Paper 362 pages $3.95 February

W. E. B. Du Bois on Sociology and the Black Community

Edited and with an Introduction by Dan s. Green and Edwin D. Driver

With this volume, Dan Green and Edwin Driver make available to sociologists and other interested scholars a representative selection from Du Bois's most productive years as a sociologist (1896–1910), including many papers that are now out of print and difficult to obtain.

Paper 328 pages $5.50 February

The Black Extended Family

Elmer P. Martin and Joanne Mitchell Martin

The Martins point out that what appear to be fatherless, broken homes in our cities may really be vital parts of strong and flexible extended families based hundreds of miles away—usually in rural areas.

Paper 144 pages Illus. $3.95 February

The Gray Lobby

Henry J. Pratt

"*The Gray Lobby* is a success because it goes beyond the sphere of old age and provides an excellent model, based on the senior citizen movement, to explain the mass character of social movements in general, as well as policy systems and strategies for successful advocation and implementation of legislation."—Hedy Weinberg, *Social Service Review*

Paper 264 pages $5.95 February

The Self-Production of Society

Alain Touraine

Translated by Derek Coltman

In this rich, theoretical work, the distinguished French sociologist Alain Touraine states his conviction that society not only reproduces itself but also produces its own categories of functioning.

Paper 488 pages $7.50 February

The Organization of Interests

Incentives and the Internal Dynamics of Political Interest Groups

Terry M. Moe

"*The Organization of Interests* is an original and important book that will begin a new wave of studies connecting why people join groups to how they behave in them to what they accomplish."—Aaron Wildavsky

Cloth 296 pages $21.00 February

The University of Chicago Press

Chicago 60637

DORSEY 1980

INTERVIEWING: Strategy, Techniques, and Tactics,

Third Edition

Raymond L. Gorden, *Antioch College*

Presents basic concepts relevant to the interviewing process, richly illustrates them with concrete examples, and provides dozens of skill building activities in the form of critical questions, laboratory problems, and field problems.

EXPERIENCING SOCIAL RESEARCH,

Revised Edition (paperbound)

John F. Runcie, *Development Analysis Associates, Inc.*

A combination of text and carefully chosen problems stressing the learning of the more theoretical aspects of research methods and the application of what has been learned in the solving of problems.

THE URBAN ENVIRONMENT (paperbound)

Samuel E. Wallace, *University of Tennessee*

Integrates and synthesizes urban theory and research within the comprehensive perspective of ecology. Analyzes the historical past as it structures the present, the present as urbanites live it, and the future as seen in contemporary patterns.

INTRODUCTION TO SOCIAL WELFARE AND SOCIAL WORK: Structure, Function, and Process

Beulah R. Compton, *University of Minnesota*

Introduces students to the development of social welfare, the profession of social work, and the issues involved in the development of both the institution and the profession.

Examination copies for adoption consideration available on request. Please indicate course title and text presently used.

THE DORSEY PRESS
Homewood, Illinois 60430

St. Martin's Press

Fieldwork Experience

Qualitative Approaches to Social Research

edited by WILLIAM SHAFFIR, McMaster University, ROBERT A. STEBBINS, University of Calgary, and ALLAN TUROWETZ, Dawson College.

Through engaging first-person accounts, *Fieldwork Experience* introduces students to participant observation and unstructured interviewing as important social research methods. Twenty-one papers, all written especially for this volume, are organized according to the chronological sequence of field research: *Getting In, Learning the Ropes, Maintaining Relations,* and *Leaving the Field.* Together, they cover a wide range of research subjects, including drug-users, widows, delinquents, elites, pain sufferers, hustlers, artists, porno shops, gays, police, retardates, Moonies, and others. The editors provide introductions to each section and a comprehensive opening chapter.

February 1980 **350 pp.** (approx.) **paper $8.95** (tent.)

Aging, the Individual and Society

Readings in Social Gerontology

edited by JILL QUADAGNO, University of Kansas.

With nearly forty selections spanning the field of social gerontology, integrated by substantial editorial essays, *Aging, the Individual and Society* combines the depth of a reader with the coherence of a text. Articles are organized in twelve sections: *Aging and Modernization; Theories of Aging; Stereotypes of Aging; Adult Socialization and the Life Cycle; Living Environments; Aging and Family Relationships; Work, Retirement, and Leisure; Minority Groups and the Aged; The Politics of Aging; The Economics of Aging; Health Care and Institutionalization; Death and Dying.* In introductions to each section Professor Quadagno reviews the relevant literature and discusses subjects beyond those covered in the papers themselves. Headnotes for each of the articles are provided as well.

February 1980 **448 pp.** (approx.) **paper $8.95** (tent.)

To request a complimentary examination copy, please write (specifying course, present text, and approximate enrollment) to:

St. Martin's Press Box 5352 New York, N.Y. 10017

Students get the total picture with these Harper & Row sociology texts:

INTRODUCTORY SOCIOLOGY

Kenkel
SOCIETY IN ACTION: Introduction to Sociology
Second Edition
2/80. 624 pages (tent.). ISBN 0-06-384486-9.

Sherman & Wood
SOCIOLOGY: Traditional and Radical Perspectives
7/79. 413 pages. Paper. ISBN 0-06-046107-1.

SOCIAL PROBLEMS

Coleman & Cressey
SOCIAL PROBLEMS
12/79. 544 pages (tent.). ISBN 0-06-041326-3.

6 NOV 1980

SOCIAL PSYCHOLOGY

Karp & Yoels
SYMBOLS, SELVES, AND SOCIETY: Understanding Interaction
3/79. 212 pages. Paper. ISBN 0-397-47395-8.

SOCIAL DEVIANCE

Schur
INTERPRETING DEVIANCE: A Sociological Introduction
8/79. 560 pages. ISBN 0-06-045811-9.

SOCIOLOGY OF MEDICINE

Rosengren
THE SOCIOLOGY OF MEDICINE: Diversity, Conflict, and Change
10/79. 446 pages. ISBN 0-06-045582-9.

INDUSTRIAL SOCIOLOGY

Miller & Form
INDUSTRIAL SOCIOLOGY: Work in Organizational Life
Third Edition
10/79. 834 pages. ISBN 0-06-044471-1.

To request examination copies, write to Suite 5D (755), Harper & Row, 10 East 53d St., New York, N.Y. 10022.

Please include course title, enrollment, and present text.

Harper & Row
New York • San Francisco

American Sociological Review

Fertility and Female Employment 167
James C. Cramer

Political Protest Orientations among Black and White Adults 191
Larry Isaac, Elizabeth Mutran, and Sheldon Stryker

The Place of Theory in Quantitative Historical Research 214
Barbara Laslett

Firearms Ownership for Sport and Protection 229
Alan J. Lizotte and David J. Bordua

The Tacit Conventions of the Modernity School 244
William C. Rau

The Transition to Adulthood As a Career Contingency 261
Dennis P. Hogan

An Opportunity Labor Demand Model 276
Suresh L. Konda and Shelby Stewman

Research Notes
Truth Tests and Utility Tests 302
Carol H. Weiss and Michael J. Bucuvalas

The Spatial Structure of Rural Income Distribution 313
David A. McGranahan

Comments 325
Paul Attewell and Robert Fitzgerald; John Braithwaite and David Biles; Norval D. Glenn and Sandra L. Albrecht; Robert W. Jackman; Michael W. Martin and Jane Sell

CALCUTTA UNIVERSITY

Volume 45 April 1980 Number 2

Notice to Contributors

A processing fee of $10 is required for each paper submitted, except for comments; such fees will be waived for student members of ASA. A check or money order, made payable to the American Sociological Association, should accompany each submission. The fee must be paid in order for the review process to begin.

The length of an *ASR* paper is generally ten (10) printed pages or thirty (30) typed pages. However, the *ASR* is happy to accept manuscripts up to fifty (50) typed pages in length for review. We also are interested in receiving manuscripts for a Research and Notes Section.

To permit anonymity in the review of manuscripts, keep identifying material out of the manuscript. Attach a cover page giving authorship, institutional affiliation and acknowledgments, and provide only the title as identification on the manuscript and abstract.

ASA Multiple Submissions Policy: Submission of a manuscript to a professional journal clearly implies commitment to publish in that journal. The competition for journal space requires a great deal of time and effort on the part of editorial readers whose only compensation for this service is the opportunity to read papers prior to publication and the gratification associated with discharge of professional obligation. For these reasons, *the American Sociological Association regards submission of a manuscript to a professional journal while that paper is under review by another journal as unacceptable.*

SUBMISSION AND PREPARATION OF MANUSCRIPTS

1. Submit four (4) copies and retain the original for your files. Copies may be Xerox, mimeograph or multilith, but not carbons.
2. Enclose a stamped self-addressed postcard for acknowledgment of manuscript receipt. *Manuscripts will not be returned.* Please do not send your original copy.
3. All copy must be typed, doublespaced (including indented material, footnotes and references) on 8½ by 11 inch white opaque paper. Lines must not exceed six (6) inches. Margins must be a minimum of one inch.
4. Include four (4) copies of an abstract of no more than 150 words.
5. Type each table on a separate page. Insert a location note at the appropriate place in the text, e.g., "Table 2 about here." Send copies, retain originals.
6. Figures must be drawn in India ink on white paper. Send copies, retain originals.
7. Clarify all symbols with notes in the margins of the manuscript. Circle these and all other explanatory notes not intended for printing.
8. Footnotes should not be placed in the body of the text. Type them (doublespaced) and attach them as a separate appendix to the text. Number them consecutively throughout the text. Footnotes are to be kept to a minimum and used only for substantive observations. Source citations are made within the text rather than in footnotes.
9. Acknowledgments, credits and grant numbers are placed on the title page with an asterisk.

REFERENCE FORMAT

A. *In the text:* All source references are to be identified at the appropriate point in the text by the last name of the author, year of publication and pagination where needed. Identify subsequent citations of the same source in the same way as the first, not using *"ibid.," "op. cit.," or "loc. cit."* Examples follow:

1. If author's name is in the text, follow it with year in parentheses. [". . . Duncan (1959) . . ."]
2. If author's name is not in the text, insert, in parentheses, the last name and year, separated by a comma. [". . . (cf. Gouldner, 1963) . . ."]
3. Pagination (without "p." or "pp.") follows year of publication after a colon. [". . . Kuhn (1970:71)."]
4. Give both last names for dual authors; for more than two use "et al." in the text. When two authors have the same last name, use identifying initials in the text. For institutional authorship, supply minimum identification from the beginning of the complete citation. [". . . (U.S. Bureau of the Census, 1963:117) . . ."]
5. Separate a series of references with semicolons and enclose them within a single pair of parentheses. [". . . (Burgess, 1968; Marwell et al., 1971; Cohen, 1962) . . ."]

B. *In the appendix:* List all source citations by author, and within author by year of publication, in an appendix titled "References." The reference appendix must be complete and include all references in the text. The use of "et al." is not acceptable in the appendix; list the names of all authors. (See A. 4. for text format.)

If there is more than one reference to the same author and year, distinguish them by the letters, a, b, etc. added to the year. [". . . Levy (1965a:331) . . ."] Give the publisher's name in as brief a form as is fully intelligible. For example, John A. Wiley and Sons should be "Wiley." If the cited material is unpublished, use "forthcoming" with name of journal or publisher; otherwise use "unpublished."

Use no underlining, italics or abbreviations.

Examples follow:

1. *Books:* Jud, Gerald J., Edgar W. Mills, Jr. and Genevieve Walters Burch
   1970 Ex-Pastors. Philadelphia: Pilgrim Press.
   U.S. Bureau of the Census
   1960 Characteristics of Population. Volume 1. Washington, D.C.: U.S. Government Printing Office.
   Bernard, Claude
   [1865] 1957 An Introduction to the Study of Experimental Medicine. Tr. Henry Copley Greene, New York: Dover.
2. *Periodicals:* Conger, Rand
   Forthcoming "The effects of positive feedback on direction and amount of verbalization in a social setting." Pacific Sociological Review.
   Merton, Robert K.
   1963a "The ambivalence of scientists." Bulletin of The Johns Hopkins Hospital 112:77–97.
   1963b "Resistance to the systematic study of multiple discoveries in science." European Journal of Sociology 4:237–82.
3. *Collections:* Davie, M.
   1938 "The pattern of urban growth." Pp. 133–61 in G. Murdock (ed.), Studies in the Science of Society. New Haven: Yale University Press.

See recent issues for further examples.

Revised 1980 ASR

AMERICAN SOCIOLOGICAL REVIEW

Official Journal of the American Sociological Association

APRIL, 1980 VOLUME 45, NUMBER 2

ARTICLES

Fertility and Female Employment: Problems of Causal Direction .... James C. Cramer 167

Political Protest Orientations among Black and White Adults .... Larry Isaac, Elizabeth Mutran, and Sheldon Stryker 191

Beyond Methodology: The Place of Theory in Quantitative Historical Research .... Barbara Laslett 214

Firearms Ownership for Sport and Protection: Two Divergent Models .... Alan J. Lizotte and David J. Bordua 229

The Tacit Conventions of the Modernity School: An Analysis of Key Assumptions .... William C. Rau 244

The Transition to Adulthood As a Career Contingency .... Dennis P. Hogan 261

An Opportunity Labor Demand Model and Markovian Labor Supply Models: Comparative Tests in an Organization .... Suresh L. Konda and Shelby Stewman 276

RESEARCH NOTES

Truth Tests and Utility Tests: Decision-Makers' Frames of Reference for Social Science Research .... Carol H. Weiss and Michael J. Bucuvalas 302

The Spatial Structure of Income Distribution in Rural Regions .... David A. McGranahan 313

COMMENTS AND REPLIES

Comparing Stratification Theories (A Comment on Robinson and Kelley, ASR February, 1979) .... Paul Attewell and Robert Fitzgerald 325

Synthesis and Comparison of Stratification Theories: A Reply .... Robert V. Robinson and Jonathan Kelley 328

Empirical Verification and Black's *The Behavior of Law* (Comment on Gottfredson and Hindelang, ASR February, 1979) .... John Braithwaite and David Biles 334

Trite but True (Response to Braithwaite and Biles) .... Michael R. Gottfredson and Michael J. Hindelang 338

Is the Status Structure in the United States Really More Fluid for Women Than for Men? (Comment on Glenn, Ross, and Tully, ASR October, 1974) .... Norval D. Glenn and Sandra L. Albrecht 340

The Impact of Outliers on Income Inequality (Comment on Hewitt, ASR June, 1977; Stack ASR, February, 1979; and Hewitt, ASR February, 1979) .... Robert W. Jackman 344

Reply to Jackman .... Christopher Hewitt 347

In Defense of Microeconomic Theory (Comment on Cook and Emerson, ASR, October, 1978) .... Michael W. Martin and Jane Sell 349

Does Microeconomic Theory Need a Defense? (A Reply to Martin and Sell) .... Karen S. Cook, Mary R. Gillmore, and Toshio Yamagishi 351

OTHER

The Editor's Items 354

Editor: RITA J. SIMON

Deputy Editors: CLARK McPHAIL AND JOE L. SPAETH

Associate Editors: PAUL BURSTEIN, ROBERT EVAN COLE, THOMAS J. FARARO, BERNARD FARBER, DANIEL GLASER, MICHAEL J. HINDELANG, GISELA HINKLE, ROSABETH KANTER, JAMES R. KLUEGEL, JOHN LOFLAND, MURRAY MELBIN, MICHAEL MOCH, ANTHONY OBERSCHALL, VALERIE K. OPPENHEIMER, JEFFREY PFEFFER, ALEJANDRO PORTES, SAMUEL PRESTON, BARBARA RESKIN, RICHARD RUBINSON, DAVID R. SNYDER, CHARLES W. TUCKER, JOHN WILSON, MAYER N. ZALD, LYNNE G. ZUCKER.

Copy Editor: ELIZABETH ROBINSON NEUMANN

Managing Editor: LINNA McDADE

Editorial Assistant: HELEN CURLEY

Executive Officer: RUSSELL R. DYNES

SCOPE AND MISSION OF AMERICAN SOCIOLOGICAL REVIEW: The *American Sociological Review* publishes work of interest to the discipline in general, new theoretical developments, results of research that advance our understanding of the most fundamental social processes, and important methodological innovations. Like other publications of the Association, emphasis is given to exceptional quality. Unlike the more specialized journals of the Association, the *American Sociological Review's* primary focus is on whatever has the most general bearing on the knowledge of society.

Concerning manuscripts, address: Editor, *American Sociological Review*, Department of Sociology, University of Illinois, Urbana, Illinois 61801. (217) 333-6515.

The *American Sociological Review* (ISSN 0003–1224) is published at 49 Sheridan Avenue Albany, New York, bimonthly in February, April, June, August, October, and December.

Applications for permission to quote from this journal should be addressed to the Executive Office

Copyright © 1980 American Sociological Association.

Annual membership dues of the American Sociological Association, $20–$120 ($10 of this amount may cover a subscription to the *American Sociological Review*); Associate, $25; International Associate, $25; Student Associate, $12.

Application for membership and payment of dues should be made to the Executive Office. Subscription rate for members, $10; nonmembers, $15; institutions and libraries, $30; nonmember students, $10. Single issue, $4.

New subscriptions and renewals will be entered on a calendar year basis only.

Change of address: Six weeks advance notice to the Executive Office and old address as well as new are necessary for change of subscriber's address.

Concerning advertising, changes of address, and subscriptions, address: Executive Office, American Sociological Association, 1722 N Street, N.W., Washington, D.C. 20036.

Claims for undelivered copies must be made within the month following the regular month of publication. The publishers will supply the missing copies when losses have been sustained in transit and when the reserve stock will permit.

Second class postage paid at Washington, D.C. and additional mailing offices.

MEMBERS OF THE COUNCIL, 1980

Officers of the Association

PETER H. ROSSI, President
University of Massachusetts, Amherst
HELEN MacGILL HUGHES, Vice President
Cambridge, Massachusetts
WILLIAM FOOTE WHYTE, President-Elect
Cornell University
RENEE C. FOX, Vice President-Elect
University of Pennsylvania
JAMES F. SHORT, JR. Secretary
Washington State University
HERBERT L. COSTNER, Secretary-Elect
University of Washington
HUBERT M. BLALOCK, JR., Past President
University of Washington
RUSSELL R. DYNES, Executive Officer
Ohio State University

Elected-at-Large

PAULINE B. BART
University of Illinois, Chicago Circle
NORMAN BIRNBAUM
Amherst College
ERNEST Q. CAMPBELL
Vanderbilt University
ARLENE K. DANIELS
Northwestern University
IRWIN DEUTSCHER
University of Akron
WILLIAM GAMSON
University of Michigan
HELENA Z. LOPATA
Loyola University of Chicago
THOMAS F. PETTIGREW
Harvard University
MORRIS ROSENBERG
University of Maryland
IMMANUEL WALLERSTEIN
SUNY, Binghamton
CHARLES V. WILLIE
Harvard University
MAURICE ZEITLIN
University of Cal., Los Angeles

(Articles in the REVIEW are indexed in the Social Sciences Index, Psychological Abstracts, Sociological Abstracts, Ayer's Guide, University Microfilms, Abstracts for Social Workers, International Political Science Abstracts, and United States Political Science Documents.)

AMERICAN SOCIOLOGICAL REVIEW

FERTILITY AND FEMALE EMPLOYMENT: PROBLEMS OF CAUSAL DIRECTION*

JAMES C. CRAMER
University of California at Davis

American Sociological Review 1980, Vol. 45 (April):167–190

Seemingly contradictory results have been reported recently on the causal relationship between fertility and female employment. Data from the Panel Study of Income Dynamics are used to investigate four possible explanations of the contradictions: (1) multicollinearity in nonrecursive models; (2) misspecification of models; (3) discrepancies between attitudes or intentions and behavior; and (4) differences between static and dynamic models. All four explanations are pertinent to the controversy. It is found that the dominant effects are from fertility to employment in the short run and from employment to fertility in the long run. Multicollinearity often is a serious problem in nonrecursive models. Dynamic models are much more informative than static models. Plans generally seem to be consequences more than determinants of behavior. Results (sometimes) depend upon how variables are measured, upon what time intervals are used, and upon aggregate trends and cycles and historical circumstances. Other substantive findings and general methodological issues are discussed briefly.

Many of the major controversies in the study of fertility involve questions of causal direction. At the macrotheoretical level it has been debated—at least since Malthus—whether economic well-being determines fertility or fertility determines well-being. In recent decades, "economic development" has been the popular name for well-being and, currently, income distribution is becoming a new focus for the same debate (Lindert, 1978). At the microlevel of households, related controversies are more complex because household income can come from different sources; so theories must consider sex roles as well as fertility and level of income. Among the many issues here, the most important problem is the relationship between fertility and female employment.[1] Many studies indicate that a wife's childrearing responsibilities constrain her labor force activity (Bowen and Finegan, 1969; Sweet, 1973). On the other hand, wife's labor force activity or plans often are found to be good predictors of her expected fertility (Bumpass and Westoff, 1970; Ryder and Westoff, 1971; Whelpton et al., 1966).

Rather than debate which causal interpretation of female employment and fertility is correct, demographers have been inclined to believe that both are correct (Bagozzi and Van Loo, 1978; Easterlin, 1978; Hawthorn, 1970; Turchi, 1975b; Willis, 1973). This approach to the problem of causal direction has been stimulated greatly by an increasing awareness of the development, originally in macroeconomics, of statistical models for estimating reciprocal causal relationships. The problem of causal direction is common in other areas of sociology besides fertility, and the use of models of reciprocal causality is increasing rapidly (Land and Felson, 1978). In the various demographic and sociological applications one senses a tempting hope that such nonrecursive models can answer questions of causal direction simply and empirically—just specify both directions,

---

* Direct all correspondence to: James C. Cramer; Department of Sociology; University of California; Davis, CA 95616.

The research for this paper was supported by NICHD grant 5-RO1-HD10233-02. The author gratefully acknowledges computing assistance from Bernadette Tarallo and helpful suggestions from John Czajka, Thomas Dietz, Maurice MacDonald, and the anonymous referees.

[1] The ambiguity of causal direction is evident also in policy proposals. Smith (1972) recommends reducing poverty by lowering fertility, while Stolzenberg and Waite (1977) discuss the possibilities of lowering fertility by raising female earnings.

and see which is stronger. But the hope of such a simple solution is naive.

Some of the difficulties encountered in empirical research using nonrecursive models can be detected from a comparison of two recent studies, by sociologists, of the relationship between fertility and wife's employment. Both studies were stimulated by the developing economic theories of household decision-making as well as by social-psychological research on female employment, sex roles, and fertility. Both studies used large national samples of relatively young married couples, and the samples were interviewed at roughly the same date (1970 and 1973)—yet the two studies obtained seemingly opposite results. Waite and Stolzenberg (1976; Stolzenberg and Waite, 1977) examined the relationship between expected family size and wife's expected employment status at age 35.[2] They found a strong effect of employment plans on expected fertility and only a very weak effect in the opposite direction. Smith-Lovin and Tickamyer (1978) studied the number of years worked by the wife since marriage and the number of live births during the same time period. They found a strong effect of fertility on years employed and only a very weak effect in the opposite direction.

There are a number of possible explanations for the difference in results. (1) The difference may be due to technical problems in estimating the nonrecursive models, particularly multicollinearity (Turchi, 1975a:88). (2) The models could be misspecified. As Land and Felson (1978) note, most sociological nonrecursive models are arbitrarily identified according to availability of data. The two models of fertility and wife's employment use different instrumental variables and define some variables in controversial ways. (3) One of the models deals with expectations (attitudes), while the other deals with reported behavior; consistent results may not be reasonable if fertility and employment attitudes and behavior are only weakly associated (due perhaps to biological or labor market contingencies). (4) Finally, one of the models is static, while the other ostensibly is dynamic. The relationship between causal processes and outcomes is extremely subtle and complex (Carlsson, 1972). It may be that the two models can be reconciled and shown not to be inconsistent after all.

The primary purpose of this paper is to explore the causal relationship between fertility and wife's employment using the studies by Waite and Stolzenberg and by Smith-Lovin and Tickamyer as a point of departure. Other nonrecursive demographic models have been estimated, but these two studies illustrate quite nicely most of the important issues. These issues—multicollinearity, model specification, attitudes (plans, anticipations) and behavior, and static and dynamic models—confront all sociological models of reciprocal causal relationships, yet they are seldom discussed adequately. A second, and not unimportant, purpose of this paper is to illustrate these general issues during the exploration of the fertility-female employment relationship.

The organization of the paper is as follows. The problem of multicollinearity is defined in the first section, and its symptoms are identified in the studies by Waite and Stolzenberg and by Smith-Lovin and Tickamyer. These two studies are replicated as closely as possible in the second section and evaluated by a sensitivity analysis; this section serves also as an introduction to a relatively complicated data set. In the third section, several specification problems are discussed, such as the measurement of economic variables and time intervals in dynamic models, and a dynamic model of fertility and employment behavior is estimated. Additional specification problems are discussed in the following section (for example, distinguishing between if and when events occur in dynamic models), and a dynamic model incorporating both employment and fertility intentions and behavior is estimated. In the final section the substantive and methodological results are summarized and evaluated.

The data analyzed here are from the Panel Study of Income Dynamics, a large national sample of households interviewed in 1968 and reinterviewed annually (Duncan and Morgan, 1976). Members of 1968 families who subsequently split and formed new households were followed and interviewed each year after splitting off, so cross-sectional samples remain nationally representative and do not necessarily become progressively older (that is, there are about as many young respondents in 1976 as in 1968).[3] Many questions were asked every year and can be used for longitudinal analyses of stable families. Some questions were asked only in certain years and so can be analyzed only for subsamples of families in existence in those years. An important case of unique questions occurred in 1976, when wives also were interviewed in addition to the usual annual interview with husbands or unmarried heads of households. All analyses reported here are based on samples of young married couples. Beyond this pervasive characteristic, the models use different questions and different subsamples of families; specific details are discussed for each new model.

---

[2] Waite and Stolzenberg report results both for currently married and single women, but only the results for married women are discussed here.

THE PROBLEM OF MULTICOLLINEARITY

The general structure of nonrecursive models and the problems of identification and estimation have been presented quite clearly by Blalock (1969), Duncan (1975), Goldberger (1973), and others, and the reader is assumed to have some familiarity with this material. Our first concern, in analyzing nonrecursive models of fertility and wife's employment, is the problem of multicollinearity, which often is alluded to in the technical literature but seldom is discussed directly or in accessible jargon (Duncan, 1975: 86–90; Fisher, 1971; Johnston, 1972:280–1, 393, 410–12; Rao and Miller, 1971: 216–20). Relatively simple nonrecursive models usually are estimated by two-stage least squares (2SLS), an estimator often characterized as "consistent but inefficient." The inefficiency of 2SLS is most acute with cross-sectional survey data (Rao and Miller, 1971:220) and, thus, is more likely to be a problem for sociologists than is indicated in the econometrically oriented technical literature. One of the less-often noted sources of inefficiency is multicollinearity. This problem, which on casual inspection of the empirical literature seems to be common,[4] can be described by a simple hypothetical example.

In a nonrecursive model with two endogenous variables (x_1 and x_2), each with one instrumental variable (x_3 and x_4 respectively) and a joint exogenous predictor x_5 (where all variables are measured without error as deviations from their means), the equation for x_1 is estimated by 2SLS as

$$x_1 = b_{12}\,\hat{x}_2 + b_{13}\,x_3 + b_{15}\,x_5 + U$$

where $\hat{x}_2$ is the predicted value of x_2 obtained from the regression of x_2 on all predetermined variables x_3, x_4, and x_5 (the first-stage regression). If x_4 is a weak instrument for x_2, then $\hat{x}_2$ will be nearly a perfect linear combination of x_3 and x_5 and multicollinearity will be a potentially serious problem in the x_1 equation. x_4 is "weak" either if x_2 and x_4 are relatively weakly correlated or if x_4 is highly correlated with x_3 or x_5. A useful measure of the weakness of x_4 as an instrument is the coefficient of determination (R^2) from regressing $\hat{x}_2$ on the other predictors of x_1 (that is, x_3 and x_5); a coefficient near 1.0 indicates high multicollinearity. This specialized use of R^2, equivalent to a measure proposed recently by Chatterjee and Price (1977), will be denoted M^2.

---

[3] In estimating the regression models, weights are used to adjust the data for disproportionate stratified sampling, differential response rates, and split-offs. The design effects of the clustered sampling and weighting are not known, so estimates of standard errors are only approximate. These estimates are not presented with the regression results, and questions of statistical significance are treated cautiously and conservatively.

[4] The same problem of multicollinearity can arise in recursive models if one of the independent variables is constructed from a prediction equation involving the other independent variables. This occurs often in fertility models because of the assumed importance of future (expected or permanent) income. For example, Rosenzweig and Seiver (1975) construct a measure of permanent income using husband's education, age, and occupation and then use both permanent income and husband's education to predict fertility. Their results suggest possible problems of multicollinearity: large standard errors and implausible coefficients for the two variables.

The major consequence of multicollinearity is that estimates of the regression coefficients are unreliable. The standard error of b_{12} is inversely proportional to $(1-M^2)$; with high multicollinearity this quantity is close to zero, so the standard error is large. The standard errors of the other coefficients (b_{13} and b_{15}) are not independent, and they, too, are large to the extent that x_3 and x_5 are correlated with $\hat{x}_2$.[5] In any particular sample there is a substantial risk that the estimates of b_{12} and other slopes are quite far from the "truth." The slopes of the reciprocally causal variables, in particular, may be implausibly large or have the "wrong" signs. Slopes of endogenous variables with surprising magnitudes and signs are found in the two nonrecursive models of female employment and fertility described earlier. These are symptoms that multicollinearity may be serious, although definite judgments are impossible because of insufficient published information. The evidence for high multicollinearity will be described briefly, and its possible consequences are explored in the next section.

Waite and Stolzenberg (1976) estimate a model of expected family size (EFS) and of whether the woman expects to be employed at age 35 (LFPP), for a sample of women under age 30; separate (but similar) estimates are reported for married and unmarried women. Each endogenous variable has two instruments and, in addition, wife's age, education, race, and husband's income (in the "married" sample) are specified as joint predictors. The instrumental variables predicting expected family size are ideal family size and wife's number of siblings. The latter variable is irrelevant in their models, but ideal family size is very strongly associated with EFS. Using 2SLS, EFS is estimated to have only a very weak effect on LFPP.

The instrumental variables predicting employment plans are the labor force activity of the woman's mother and an index of attitudes regarding working mothers. Both instruments are only weakly related to LFPP, but so are the joint predictors. The importance of ideal family size in the first-stage regression cannot readily be ascertained from the published information, but it seems likely that it is a moderately strong predictor and that the instruments for LFPP may be relatively weak (that is, M^2 may be large). The results are symptomatic of multicollinearity: using 2SLS (but not OLS), LFPP is estimated to have a very large effect on EFS. The authors themselves express surprise at the results, although they do not include multicollinearity among their post-hoc explanations.

In our second case study, Smith-Lovin and Tickamyer (1978) estimate a model of number of children ever born (presumably since marriage) and number of years employed, since marriage, for a sample of once-married wives at age 30. Each endogenous variable has several instruments, and there are a number of joint predictors of which only wife's education, husband's income, and marital duration are at all important. The instrumental variables predicting family size are indexes of religious affiliation and commitment, sterility, and number of siblings. The latter variable is irrelevant but the other instruments appear to be acceptably strong, although marital duration (a joint predictor) clearly is the main determinant of number of children ever born. Using 2SLS, family size is estimated to have a quite strong effect on wife's years employed.

The instrumental variables predicting wife's employment since marriage are her employment experience before marriage and her mother's employment status. The latter variable seems to be unrelated to wife's employment and fertility and is irrelevant as an instrument. Premarital employment experience is significant—but almost certainly (the relevant information is not reported), it is nearly collinear with other predictors and, thus, is probably a weak instrument. The sample is an age cohort, so marital duration also identifies age at marriage. "Years worked before marriage" is defined to exclude part-time work while in school, and most women in the sample completed schooling before marriage, so years of schooling and years employed before marriage must nearly define age at marriage. The problem of high multicollinearity among these three variables is particularly serious because age at marriage/marital duration is a very strong predictor of both employment and fertility. Perhaps the multicollinearity associated with the weakness of premarital employment as an instrument explains the unexpected result that wife's employment is estimated to have a very small, but positive, effect on fertility.

---

[5] The interdependencies among standard errors are described mathematically by Johnston (1972:160–3). Other problems associated with 2SLS besides multicollinearity are discussed by Fox (1978), Hout (1977), and Luskin (1978).

Despite the evidence of weak instruments in these two studies, it cannot be shown that the results are distorted by multicollinearity. Multicollinearity simply leads to unreliable estimates; in any particular sample, the estimates may be either accurate or highly misleading. One possible response to this uncertainty is to eschew 2SLS, but simulation studies suggest that OLS and alternative estimators often are worse evils (Atkinson, 1978; Johnston, 1972; Rao and Miller, 1971). A second response is to compare estimates from different samples. Waite and Stolzenberg's results are supported by Cain and Dooley (1976), who found a large effect of female employment on fertility (and a negligible reciprocal effect) from aggregated 1970 census data; and by Butz and Ward (1977) in an aggregate time series analysis. Smith-Lovin and Tickamyer's results also have been supported in different models using the same data (Cain, 1979) and using other data (Cramer, 1979a; Hout, 1978). The supporting evidence is inconclusive, however, because many of these studies also show indications of problems with multicollinearity. A third response is to perform sensitivity analyses using alternative sets of instrumental variables (Land and Felson, 1978). This approach will be taken with Income Dynamics data.

SENSITIVITY ANALYSIS OF MULTICOLLINEARITY

Sensitivity analyses may be designed as experiments in which the alternative sets of instrumental variables represent a range of degrees of multicollinearity. Such an experiment is not conclusive, but it may provide evidence on the stability of results. The drawback, of course, is theoretical impurity: causal models are supposed to be theoretically derived and meaningful, not arbitrarily specified experiments. Probably it is premature to worry about this drawback, as most sociologists seem to work in a theory-construction mode. As Land and Felson note (1978), much research employs large data sets (often collected for other purposes) with many variables and weak theories that are relatively permissive regarding specification of exogenous variables. The two studies examined above conform to this description: both are secondary analyses of multipurpose data sets and are somewhat arbitrarily specified.[6]

Employment and Fertility Plans

Questions about expected family size and wife's employment plans were asked in 1972, but Waite and Stolzenberg's model cannot be replicated exactly. Expected family size is reported by the husband, and, for our purposes, we must assume (as is common in microeconomic models) that husbands and wives have identical preferences. Husbands reported wives' labor force plans "in the next few years," rather than "at age 35"; and there are no data on wives' employment attitudes or mothers' employment. Other variables in their model are available but, sometimes, only from interviews taken before 1972. Alternative models are estimated, using weak and strong instruments for both labor force plans (LFP) and expected family size (EFS). The instrumental variables for EFS are ideal family size (strong) and religion and number of siblings (weak). The instruments for LFP, in place of work attitudes, are wife's prior work experience[7] (whether employed at any time, 1967–1969, a strong instru-

---

[6] That is, the selection of variables is influenced by theories and is plausibly justified, but it is not derived from theory; it is also heavily influenced by what variables happen to be available in the data sets.

[7] The assumption here is that work experience is strongly indicative of employment attitudes and tastes; Heckman and Willis (1977), among others, provide evidence on this. Hout (1978) also uses work experience as an instrumental variable (for current employment, not employment plans).

Table 1. Standardized Regression Coefficients from Three Nonrecursive Models of Wife's Labor Force Plans (LFP) and Expected Family Size (EFS) in 1972

	LFP As Dependent Variable			EFS As Dependent Variable		
	1	2	3	1	2	3
LFP[a]				−.26	−.29	−.24
EFS[a]	−.17	.77	−.10			
Prior employment	[b]	.47	.37	[b]	[c]	[c]
Youngest child 0–2	−.07	[b]	[b]	[c]	[b]	[b]
Wife's age	.06	−.17	.10	.28	.32	.28
Wife's education	.13	.26	.09	−.12	−.13	−.12
Race	.06	−.09	.02	.11	.13	.11
Husband's income	−.23	−.09	−.16	−.13	−.13	−.13
Ideal family size	[c]	[b]	[c]	.26	[b]	.26
Catholic	[b]	[c]	[b]	[b]	.06	[b]
Number of siblings	[b]	[c]	[b]	[b]	.11	[b]
M^2 of endogenous predictor's instruments	.71	.95	.72	.92	.32	.32
R^2 from first-stage regression	.07	.20	.20	.24	.19	.24

[a] The standardized coefficients for the endogenous predictors are estimated by a two-step process using Hout's recommended formula (Hout, 1977).

[b] Variable excluded from this model.

[c] Variable excluded from this equation but included in model.

ment) and age of youngest child in 1972 (whether under age three, a weak instrument). Wife's age, education, race (whether black), and husband's earnings are joint predictors. The sample is restricted to couples stably married 1968–1972, wife under age 37 in 1972, and either no children in 1972 or youngest child under age eight (N = 639).[8]

Results for three models are presented in Table 1. Models 1 and 2 each feature a strong instrument for one of the endogenous variables and a weak instrument for the other. In model 3 both endogenous variables have strong instruments. All the LFP equations exhibit fairly high multicollinearity, although ideal family size is a somewhat stronger instrument for EFS (M^2 under .75) than are religion and number of siblings, together (M^2 = .95). The estimated effects of EFS and the joint predictors seem to be sensitive to the small differences in multicollinearity, with highly implausible estimates in the model with weak instruments. In the better models, expected family size seems to depress labor force plans by a small amount. There is much greater variation in the multicollinearity present in the EFS equations, as prior employment is a much stronger instrument for LFP than is age of youngest child. In spite of this, all estimates are similar in the three models: employment plans depress expected family size by a moderate amount. Thus, multicollinearity is a potential danger in these models, but its effects are unpredictable. Employment plans clearly seem to have an effect on expected family size, but there appears to be a reciprocal effect, also, and the latter is not clearly smaller than the former.[9]

Expected family size and employment plans were also asked about, in the 1976 interviews of wives, permitting an alternative sensitivity analysis of Waite and Stolzenberg's model. The instrumental variables for EFS and the joint predictors are the same as before. Prior employment (whether employed at any time, 1971–1973) and age of youngest child (whether under age of three in 1976) are, again, instruments for LFP. An additional instrument for LFP is whether the wife was ever

---

[8] Wife's age is restricted so that, on average, labor force plans "in the next few years" refer to age 35. Other departures from Waite and Stolzenberg's sample criteria are due to data limitations or to constraints imposed by other uses of these data in later sections.

[9] That is, the difference between estimated coefficients probably is not statistically significant (this cannot be estimated reliably with these data and the available computer software). Effects of other variables in the models generally are in expected directions with plausible magnitudes. Similar results are obtained with EFS dichotomized as under three children or three or more (that is, scaled 0–1, the same as LFP).

Table 2. Standardized Regression Coefficients from Three Nonrecursive Models of Wife's Labor Force Plans (LFP) and Expected Family Size (EFS) in 1976

	LFP As Dependent Variable			EFS As Dependent Variable		
	1	2	3	1	2	3
LFP				−.29	−.22	−.19
EFS	.85	.02	.18			
Prior employment	.27	.14	[a]	[b]	[b]	[a]
Husband's approval	.52	.46	.50	[b]	[b]	[b]
Employed before marriage	[a]	[a]	.04	[a]	[a]	[b]
Youngest child age 0–2	[a]	[a]	−.06	[a]	[a]	[b]
Wife's age	−.26	−.09	−.15	.16	.17	.15
Race	−.08	.02	.01	.11	.12	.10
Wife's education	.20	.06	.11	−.12	−.14	−.11
Husband's earnings	−.07	−.03	−.09	.04	.08	.08
Ideal family size	[a]	[b]	[b]	[a]	.26	.26
Catholic	[b]	[a]	[b]	.10	[a]	.08
Number of siblings	[b]	[a]	[b]	.11	[a]	.10
M^2 of endogenous predictor's instruments	.91	.67	.71	.27	.23	.30
R^2 from first-stage regression	.32	.30	.30	.15	.21	.23

[a] Variable excluded from this model.

[b] Variable excluded from this equation but included in model.

employed before marriage. Also, in 1976, wives were asked,

> "How does your husband feel about (your working/the possibility of your working)? Is he very much in favor of it, somewhat in favor of it, neither for nor against it, somewhat against it, or very much against it?"

A nearly identical question was used by Waite and Stolzenberg as an instrument for LFP but was found to be irrelevant; nevertheless, husband's approval is used here as an instrument for LFP. The sample is restricted to couples stably married, 1971–1976, under age 35 in 1972, and having at least one child under age six in 1972 (N = 512).[10]

Results for three models are presented in Table 2. As before, multicollinearity in the LFP equations ranges from high to very high, and the estimated effect of EFS varies accordingly. The most plausible estimate, from Model 2, is that expected family size has no effect on employment plans. A quite different estimate is obtained in Model 3, but not because of greater multicollinearity; sensitivity analysis creates problems of specification error, and these are discussed in a later section. Husband's approval is very strongly associated with wife's employment plans (unlike in Waite and Stolzenberg's data), and multicollinearity is very low in all of the EFS equations. As in the models of husband's expected family size, wife's employment plans depress wife's expected family size by a moderate amount.

The evidence in Tables 1 and 2 mostly supports Waite and Stolzenberg's conclusions. Employment plans clearly seem to depress expected family size. The reciprocal effect may or may not be present but, almost certainly, it is not larger than the effect from LFP to EFS. The instruments for expected family size are, generally, weak, and better ones are needed for more definitive results. Aside from problems of multicollinearity, estimates vary with the specification of the LFP equations, and more care is needed here.

Actual employment and fertility

Smith-Lovin and Tickamyer's model of cumulative employment and fertility cannot be replicated because there is no measure of wife's employment since marriage in the Income Dynamics data. It is possible, however, to estimate an analogous model that adequately represents their

---

[10] The same sample is used for a later model, and the criteria were determined by that use. Husband in the labor force and nuclear family (only husband, wife, and children present) also are criteria for inclusion.

Table 3. Standardized Regression Coefficients from Three Nonrecursive Models of Wife's Labor Force Activity (LFA) and Fertility (FF) during a Six-Year Period, 1968–1973

	LFA As Dependent Variable			FF As Dependent Variable		
	1	2	3	1	2	3
LFA				−.86	.03	.04
FF	−.60	.32	.10			
Wife's employment before marriage	−.07	[a]	[a]	[b]	[a]	[a]
Wife's LF experience, 1968	.25	.23	.21	.21	[b]	[b]
Husband's wage rate	−.21	−.22	−.24	−.19	.02	[b]
Wife's education	.05	.03	.02	.06	.02	.04
Race	.12	.09	.11	.13	.04	.03
Age of youngest child, 1968	.07	.26	.20	−.04	−.21	−.21
Wife's age	−.22	−.00	[b]	−.32	−.24	−.25
Number of children, 1968	−.06	[b]	[b]	−.10	−.06	−.11
Ideal family size	[b]	[a]	−.11	.05	[a]	.13
Catholic	[b]	[a]	[b]	.05	[a]	.10
M^2 of endogenous predictor's instruments	.91	.99	.52	.90	.68	.45
R^2 from first-stage regression	.16	.13	.14	.19	.16	.18

[a] Variable excluded from this model.

[b] Variable excluded from this equation but included in model.

underlying theory by focusing on a segment of married life rather than on cumulative experiences. The endogenous variables are wife's number of years employed from 1968 to 1973 and number of births during the same six-year period. Most of the predetermined variables used by Smith-Lovin and Tickamyer are specified here, although a few have been omitted or replaced because they were irrelevant or unavailable. Wife's employment before marriage (coded 0, 1, or 2 for not employed or employed half- or full-time, respectively) is the instrument for recent employment. Religion and ideal family size are instruments for recent fertility. Wife's education, husband's wage rate, race, and wife's age are joint predictors. Finally, other joint predictors have been added to the model to control for experiences before 1968 (without imposing questionable causal assumptions or constraints), thus making the model comparable to Smith-Lovin and Tickamyer's model. These variables are age of youngest child and number of children in 1968, and wife's years employed from age 18 to 1968. The sample is restricted to couples stably married, 1968–1976, wife under age 35 in 1968, and having at least one child under age six in 1968 (N = 442).[11]

Results for this model are presented as Model 1 in Table 3. All of the instrumental variables are weak, and multicollinearity is very high (M^2 over .90). The coefficients for the endogenous predictors are quite large in absolute value, and other coefficients also are implausible. Very clearly the model is inadequate. Results are even worse if the controls for initial conditions (age of youngest child, number of children, work experience) are omitted.

The logic of Model 1 is that early experiences influence subsequent behavior (if religion and ideal family size can be interpreted as fertility norms and preferences learned during adolescent socialization). Model 2 tests whether the instruments used in Model 1 are simply too remote temporally. In Model 2 the earlier instruments are dropped, and wife's work history up to 1968 and number of children in 1968 are used as instruments. Results for this model are not much better than for Model 1, with continued high multicollinearity and implausible estimates.

A major difficulty with Model 2 is the high correlation between wife's age and number of children in 1968 (r = .59). The impact of multicollinearity on the entire model is reduced if both variables are used as instruments for fertility.[12] Religion, also, is used as an instrument in Model 3, but ideal family size is specified as a joint predictor because it could reflect beliefs about female employment as well as about fertility. Husband's wage rate is shown in Model 3 as an instrument for wife's employment. This is an empirical expedient to reduce multicollinearity, not a theoretical proposition. In several alternative specifications of Model 3, husband's wage rate was strongly related to wife's employment, but not to fertility (as in Model 2); and using it as an instrument has little effect on the estimates, but improves their stability. Multicollinearity is greatly reduced in Model 3, and most coefficients have plausible signs and magnitudes. In this model there is no direct causal link between actual employment and fertility; both are determined by interrelated prior events and characteristics.

---

[11] Wife's employment and fertility are not counted up to 1976 because in this sample there were very few births after 1973. Wife's age could not be restricted more so as to be comparable to Smith-Lovin and Tickamyer's sample and still maintain a large enough sample, but there is nothing in their particular theory that would suggest that the discrepancy is important. Husband in the labor force and nuclear family also are criteria for inclusion.

DYNAMIC BEHAVIORAL MODELS

The results in Table 3 do not support Smith-Lovin and Tickamyer's conclusion that cumulative fertility strongly affects cumulative labor force experience; nor is there any good evidence of a reciprocal effect. The main finding is that the estimates are very sensitive to the model's general specification. It has become clear that this is more than just a problem of multicollinearity (although the latter is not at all negligible), and we turn now to the more general problems.

Specification Problems

A major difficulty with the Smith-Lovin and Tickamyer model is its ambiguous theoretical structure. Clearly, the model depicts behavior, but it is not clear that it depicts dynamic processes. There is no temporal relationship between cumulative fertility and employment and, indeed, the endogenous variables are nearly as static as the "lifetime decisions made at marriage" that characterize most microeconomic models. Dynamic models make essential references to time lags and intervals. Smith-Lovin and Tickamyer specify background variables and controls for marriage and premarriage durations, but these are weak references to time. Hout's model (1978), in contrast, specifies 3-year lagged fertility and employment variables and, clearly, is a dynamic model of behavioral processes. Recently, there have been numerous criticisms of the static nature of microeconomic models and appeals for dynamic models (Bagozzi and Van Loo, 1978; Heckman and Willis, 1976; Hout, 1978; Masnick and McFalls, 1976; Namboodiri, 1972; 1974; 1975; Rosenzweig, 1976; Ryder, 1973). Smith-Lovin and Tickamyer's attention to behavior is a welcome—but insufficient—step in the right direction, and Cain's reworking of the same data (1979) is only a partial improvement in this regard.

A dynamic model of cumulative employment and fertility can be specified with the data used in Table 3. Among the couples who had babies between 1968 and 1973, the exogenous variables are measured in the year preceding birth; and wife's employment is measured both before and after birth.[13] Among couples who did not have babies, the same temporal sequence of measurements is followed, using 1970 as "the year the baby was not born." This model is used to estimate whether prior employment determined who had a baby (called "fertility status"), whether fertility status determined subsequent employment, and what effects the exogenous variables had on both behavioral sequences. The model consists of unambiguously ordered behavioral sequences (plans and expectations are studied in a later section), and it is recursive, so there is no difficulty with instrumental variables.[14] Centering the measurements around the time of birth eliminates the confusion caused by timing decisions and directs attention to the important questions of who has a baby and does a baby affect wife's employment.[15]

---

[12] This does not reduce the correlation between age and parity, of course, but it eliminates other, more critical, sources of multicollinearity in the second stage of 2SLS.

[13] In the present dynamic model fertility and employment are measured during calendar years with one-year lags; for example, employment is measured in 1969 and 1971 for a birth during 1970. Alternative models with two-year lags between initial employment and fertility, and with initial employment status (a dichotomy) lagged one and two years, give similar results as those reported here, so one year seems to be an appropriate lag for decisions about pregnancy, abortion, and birth (and it is unlikely that there is a reciprocal effect of pregnancy on initial employment). For couples with two or more babies from 1968 to 1973, employment is measured before and after the first of these babies. In 22 cases, the second baby was born the year following the first; because of the difficulty in measuring post-birth employment, these couples are excluded from the sample.

In the attempts above to replicate other models, little was said about the exogenous variables; but more care is needed now. In dynamic behavioral models, the endogenous variables represent changes in initial conditions, so our exogenous variables must include measures of the wife's initial "stocks" of fertility and employment, both current and cumulative: number of children, age of youngest child, and years employed since age 18 (as well as initial or prebirth employment). Wife's age, race, and religion also are essential to the model. Husband's approval of wife's employment and other attitudinal variables are omitted here, but are examined later. Macrolevel demand for labor probably influences individuals' decisions, but the measures available—county unemployment rate and county labor force participation rate of married women, both taken from 1970 census data—are not the most appropriate variables and are largely irrelevant in our models. Because of its theoretical interest (Bowen and Finegan, 1969; Cain and Dooley, 1976), the latter variable is included in the model anyway as a proxy for the local demand for female labor. Other variables were considered but have been omitted on empirical grounds in order to keep the model simple: wife's age at marriage was highly correlated with other variables and added little to the model; farm background, number of siblings, and husband's unemployment also were irrelevant, though not because of high correlations with other variables.

Spouses' abilities to earn income are prominent predictors in microeconomic theories of employment and fertility, but there is little agreement about how to measure them. Both Smith-Lovin and Tickamyer and Waite and Stolzenberg use husband's annual earnings to measure financial pressures on the wife, and wife's education to measure her incentive to work, but economists generally consider these to be inappropriate measures. A predicted (or potential) wage rate for the wife usually is advocated in place of education.[16] Husband's earnings reflect endogenous decisions about hours worked as well as transitory constraints (for example, unemployment) and measurement errors; and current earnings may not accurately indicate expected purchasing power in the future, as the children grow up. Finally, identical levels of husband's earnings represent different financial pressures in different sizes of families.

A variety of income measures have been proposed to solve these problems. Potential income, constructed from husband's wage rate, removes variations in hours worked. Permanent income is either a short-term average, eliminating errors and random fluctuations, or a predicted future value (for example, income expected at age 40) that incorporates trend, as well as current income level. Income adequacy is a measure of income standardized for the basic consumption needs of the husband's family, based mainly on family size and composition.

Various combinations of these measures of husband's earnings can be estimated with Income Dynamics data; they are described and summarized in Table 4. All monetary variables are adjusted to

---

[14] This is true only because the problem of autocorrelated disturbances in the employment equations is being ignored until a later date. The statistical models available to handle this problem are not yet clearly established (Cook and Campbell, 1979; Hannan and Young, 1977; Hibbs, 1974).

[15] Average birth intervals are about two years (Ryder and Westoff, 1971), so probably few couples with an open birth interval of at least six years are still postponing another baby.

[16] Hout (1978) discusses some of the biases inherent in this approach. Wife's wage rate is assigned here on the basis of a nonlinear, nonadditive model estimated for employed wives using wife's education and work history, age, race, farm background, husband's wage rate, county unemployment rate, and county labor force participation rate of married women.

Table 4. Summary of Alternative Measures of Economic Predictors: Definitions, Means, and Partial Correlations with Wife's Initial Hours Employed and Fertility Status

				Partial Correlation[b]	
Name	Description	Definition[a]	Mean	Hours Employed	Fertility Status
HWAGE	Husband's wage rate	Average hourly earnings	4.23	−.10	.00
HEARN	Husband's earnings	Annual earnings	9965	−.11	.03
HIAD	Income adequacy	HEARN/NEED	2.56	−.15	.04
PIAD	Potential income adequacy	HWAGE • 2400/NEED	2.62	−.13	.03
AVEW	Average wage rate	9-year average, 1967–75	4.51	−.15	.09
AVEIA	Permanent wage rate adequacy	AVEW • 2400/NEED	2.85	−.18	.12
TREND	Average annual increase in wage rate during 1967–75.	Slope from regression of wage rate on year	0.14	−.06	.09
TRENDIA	Average annual increase in current income adequacy	TREND • 2400/NEED	.089	−.07	.10
TRENDAV	Relative wage rate trend	100 • TREND/AVEW	2.82	−.02	.07
TRENDHW	Current relative trend	100 • TREND/HWAGE	3.93	−.01	.09
WAGE	Wife's potential wage rate	Assigned from regression model	2.21	−.12	.01
ED	Wife's education	Years of schooling completed	12.0	−.04	.04

[a] NEED is the Social Security Administration's estimate of basic consumption needs based mainly on family size and composition (Duncan and Morgan, 1976). All monetary variables are adjusted to 1969 dollars (before construction of economic predictors). Unless otherwise indicated, measures are defined for the year preceding birth.

[b] In the employment model the variables controlled are initial family size, age of youngest child, wife's years worked since age 18, wife's age, race, and religion. In the fertility status model, initial employment also is controlled.

1969 dollars. The longitudinal data are used to estimate trends directly (by regressing annual real wage rates on time for 1967–1975 for each husband), thereby avoiding predictions based on other cohorts' experiences. The partial correlations between these measures and wife's initial hours employed and fertility status, controlling for the other exogenous variables, are shown for comparison and evaluation. Partial correlations for wife's potential wage rate and education also are compared in Table 4. In general, standardizing for income needs and averaging several years' values both seem to improve the predictive power of husband's wage rate.[17] Trends, no matter how they are standardized, do not predict as well as average wage rate levels in this sample.[18] The superiority of husband's average wage rate standardized for needs (AVEIA) over conventional single-year income measures, or measures of trend; persists in most of the models reported in this and the following section; the figures in Table 4 are typical of these general results. Often, results for AVEIA (called "permanent wage rate adequacy") are significant and consistent with economic theory, while results for annual wage rate or earnings are negligible or inconsistent, indicating the importance of correctly specifying variables. Wife's potential wage rate is used in our models rather than education both because it predicts better and because it is theoretically preferable.

A final specification issue is interaction effects. For example, Hout (1978) and Namboodiri (1972; 1974) argue that the determinants of additional fertility are different for women at different initial parities, because of strong normative pressures to have at least two children and because of (presumed) rapidly declining marginal satisfactions from high-order births.[19] An additive model will be estimated first and then used to test for parity and other types of interactions.

---

[17] It is assumed here that husband's wage rate is not affected by having a baby, but that future wage rates are anticipated and can influence fertility decisions (Cramer, 1979b; 1979c). Standardizing for income needs is a valid use of initial conditions in dynamic models but is never seen in static life cycle models.

[18] Trend is irrelevant when average level is controlled, but the converse is not true. Income trend might be more relevant in a sample of younger, recently married, less economically established couples. Controlling for wife's potential wage rate increases the partial correlations of trend and other husband's measures, but does not change their rank order; the correlation between WAGE and AVEIA is .63.

Results

Estimates are shown in Table 5 for a recursive model of wife's initial hours employed, fertility status, and subsequent hours employed. Very similar results are obtained if wife's employment status is used instead of hours worked. The equation for initial employment is a standard cross-sectional model of labor supply, and the coefficients are in the expected directions with the expected relative magnitudes. The main determinants of initial employment are financial pressures, demographic constraints, and prior work experience. There is a large racial difference in initial employment, but the standardized coefficient for race is not especially large because the sample is predominantly nonblack. Wife's potential wage rate has little effect on labor supply; a similar result is found with wife's education, and the result conforms with past research (Cramer, 1979a; Sweet, 1973).

Table 5. Standardized Regression Coefficients from a Recursive Model of Wife's Fertility Status from 1968 to 1973 (DFF) and Hours Employed Before (BLF) and After (ALF) Having a Baby (or "Not Having a Baby in 1970")

	BLF	DFF	ALF
Fertility status (DFF)			−.21
Employment before birth (BLF)		−.12	.52
Husband's permanent wage rate adequacy	−.14	.22	−.02
Wife's potential wage rate	.02	−.13	−.01
Age of youngest child	.20	−.13	.06
Number of children[a]	−.05	.01	.01
Wife's years worked since age 18	.43	.08	.15
Wife's age	−.35	−.35	−.04
Race	.16	.02	.07
Catholic	−.01	.05	.01
County labor market for women	.10	−.02	.02
R^2	.27	.21	.46

[a] Number of children in 1968 is dichotomized (1 or 2 children, and 3 or more) so as to be consistent with subsequent tables.

The second equation in Table 5 predicts who has another baby. The main determinant, of course, is wife's age; undoubtedly, this reflects subfecundity as well as the growing cultural preference not to have children after age 35 (Ryder and Westoff, 1971). Wife's initial employment depresses fertility slightly but significantly; the estimate here is somewhat larger than those by Hout (1978) and Smith-Lovin and Tickamyer (1978). The unstandardized coefficient is −.08, indicating that the percentage of couples who had a baby was 8% lower for each 1,000 hours of initial employment (for example, the difference between half-time and full-time employment), other things being equal. About half the sample (53%) had a baby, so the effect of initial employment is not trivial. There is no discontinuity or nonlinearity "at the corner," to differentiate nonemployed women from women with very low levels of employment.[20] One would expect a discontinuity if the effect of employment were due to sex role attitudes or tastes, so initial employment is best interpreted as an opportunity cost. This interpretation is buttressed by the small effect (in the opposite direction) of wife's prior work experience, which is much more likely to represent tastes than opportunity cost; the effect is even smaller if current (initial) employment is omitted from the equation. Furthermore, age of youngest child is moderately related to subsequent fertility; this may be interpreted both as a sign of subfecundity and as another measure of cost (Cain, 1979; Lindert, 1978; Turchi, 1975a). Finally, husband's financial well-being encourages fertility while wife's potential wage rate (another measure of cost) discourages it, consistent with microeconomic theories of fertility.[21] The estimates may be somewhat exaggerated by multicollinearity, but from Table 4 there is no doubt that husband's potential wage rate adequacy has a positive effect.

---

[19] Stolzenberg and Waite (1977) argue that the interactions are due to maturation and learning associated with wife's age, not parity. The theoretical issues are complex. In exploratory tests with the present model parity interactions were found to be substantial but not age interactions (by themselves or within parity groups), so age interactions are ignored here.

[20] Corner solutions are described by Cramer (1979a). The results for the employment corner solution are not shown in Table 5, but the relevant F-value is 0.2 (without correction for design effect, heteroskedasticity, etcetera).

The third equation, predicting wife's employment after having a baby (or after "not having a baby" in 1970), is more easily summarized. Having a baby substantially reduces wife's employment, as shown also in past research (Cramer, 1979a; 1979c; Smith-Lovin and Tickamyer, 1978). In addition, employment depends on initial employment and prior work experiences, but not on any of the other variables in the model. In this equation there is a significant discontinuity "at the corner" of initial employment: wives who were not employed initially had much lower subsequent employment than wives who were initially employed only a little. This is evidence of a tendency for wives to remain either employed or not employed, according to individual preferences more than to "market forces," and supports Heckman and Willis's (1977) bipolar "mover-stayer" model of labor supply. These results reinforce the interpretations of the fertility equation.

The most important results in Table 5, for present purposes, are that wife's employment and fertility each affect the other, but the effect of fertility on employment is relatively greater. In addition, both variables are determined jointly by exogenous factors such as financial status, age of youngest child, and wife's age. Nearly identical results are obtained if wife's employment status is used in place of hours worked, although its effect on fertility (−.07) is less than the effect of hours worked (−.12) while the reciprocal effect of fertility is the same. Most past research has used information on employment status but not on hours worked, so the effect of employment on fertility has been underestimated.

Hout's (1978) hypotheses regarding differences by parity in the determinants of fertility and employment are plausible but are not soundly based on theory, so parity interactions will be presented here as empirical regularities for which there are not yet good explanations. In order to investigate interactions, the dynamic model was estimated first among couples with only one or two children initially (two-thirds of the sample), and then among couples with three or more children.

In the equation predicting fertility, Hout (1978) suggests that husband's permanent income should be important only at low parities (due to timing of births); wife's potential wage rate should be more important at higher parities (where norms have been satisfied and costs become important); and age should be more important at lower parities (for unclear reasons). His results are consistent with all three hypotheses. All three hypotheses also are confirmed with the present models. The metric coefficients for husband's permanent wage rate adequacy in the low and high parity samples are .103 and .025, respectively; for wife's potential wage rate, −.069 and −.210; and for wife's age, −.054 and −.009. Interaction effects involving other predictors in the model have not been reported in past studies, and none of any importance or interest was found here.

In the equation predicting employment, Hout found that both prior employment and fertility have greater effects at higher parities. In our model, also, the effect of initial employment on subsequent employment is larger in the higher parity subsample, but not by much (coefficients of .545 and .619, respectively). The difference is small, partly because of the inclusion of wife's work history in our model, which also is more important at higher parities. When the effects of work history are constrained to be the same (as in Table 6), the coefficients for initial em-

---

[21] Because wife's potential wage rate is an assigned value from a regression model estimated with a different sample, its variance is underestimated by an unknown amount and, thus, its standardized coefficient also is underestimated (Hout, 1977). An alternative measure of relative importance that is unbiased is elasticity at the mean. By this criterion, wife's potential wage rate (−.45) is at least as important a predictor as husband's permanent wage rate adequacy (.38).

Table 6. Unstandardized Regression Coefficients from Recursive Models of Wife's Hours Employed in 1975, 1974, and the Year Following Childbirth: Full Sample of all Wives, and Subsample of Wives with, at Most, One Baby from 1968–1973[a]

	Full Sample, Just After Childbirth	Subsample, At Most One Baby		
		Just After	1974	1975
Fertility status				
If 1 or 2 children[b]	−393	−363	−384	−349
If 3 or more children[b]	−222	−248	−220	−153
Employment before birth, (000)				
If 1 or 2 children[b]	520	574	456	360
If 3 or more children[b]	663	693	476	366
Husband's permanent wage rate adequacy	−10	4	−26	1
Wife's potential wage rate	−14	−54	−121	−148
Age of youngest child[b]	22	12	−5	−6
Number of children[b]	−120	−86	−46	−74
Wife's years worked since 18[b]	27	27	29	44
Wife's age[b]	−9	−6	−10	−14
Race	162	161	287	238
Catholic	18	21	43	44
County labor market for women[b]	2	−3	2	11
Open birth interval			23	89
R^2	.47	.49	.35	.27

[a] The full sample, used for Table 5, includes 420 respondents. The subsample excludes couples who had a baby in 1974 or 1975 and couples who had 2 or more babies 1968–1973, leaving 347 respondents.

[b] These variables are measured the year before childbirth. Number of children (initially) is dichotomized for the sake of specifying interactions. The coefficient for number of children is an intercept (constant) for the interaction terms, unlike in Table 5.

ployment are substantially different. Hout's results for the effects of fertility conflict with past studies (Lindert, 1978; Turchi, 1975a). In our model, the effect of a baby on subsequent employment is smaller at higher birth orders (coefficients of −413 and −233), consistent with the other studies rather than with Hout. As in past research with these data (Cramer, 1979a; 1979c), the difference between birth orders is not significant. Thus, although our results replicate most of Hout's, the one interaction effect of direct relevance here (the effect of fertility on employment) is not replicated—and, indeed, is in the opposite direction, and is not significant. There are no parity differences in the effect of initial employment on fertility.

The above estimates of the effect of a baby on wife's employment are measured when the baby is about one year old. Both cross-sectional and longitudinal studies (Bowen and Finegan, 1969; Cramer, 1979c; Lindert, 1978) have indicated that the effect of a baby may decline as the baby becomes older. The model in Table 5 can be modified to study this possibility, although for convenience the sample must be limited to couples who had at most one baby from 1968 to 1973 and no babies in 1974 and 1975.[22] The modifications are to measure wife's employment in 1974 and 1975 as well as the year following birth, and to control for the number of years from birth to 1974 and 1975 (that is, open birth interval, or age of youngest child). As before, among couples who did not have a baby, employment "before birth" is measured in 1969, and open interval is measured from 1970. In 1975, the open interval ranged from two years to seven years with a mean of 5.1. Results for equations predicting wife's hours employed the year following birth and in 1974 and 1975, showing the effect of a baby from age one to average age five, are presented in Table 6. Parity interaction effects are shown for fertility status and initial employment. Unstandardized coefficients are used to permit comparisons across equations. Unstandardized coefficients also are shown for the "year after birth" equation for the full Table 5 sample.

---

[22] Forty-seven couples are excluded from the Table 5 sample because they had more than one baby during 1968–1973, and an additional 26 are excluded because they had a baby in 1974 or 1975 (ten of whom had not had a baby during 1968–1973), leaving 347 couples in the Table 6 subsample.

The effects of most variables are approximately the same in the full sample and the subsample the year following birth and in the subsample at the three points in time. The coefficients for wife's potential wage rate are not significantly different in the four equations and show signs of being somewhat distorted by multicollinearity, so the apparent differences probably should be disregarded. The coefficients for race clearly show the more rapid labor force reentry by blacks (Sweet, 1972). By 1975, the coefficient for open interval is about the same as the coefficient for age of youngest child in the prebirth cross-sectional model of labor supply in Table 5, suggesting that the dynamic model is approaching stability five years after the "shock" of childbirth.

The effect of employment before birth on subsequent employment clearly diminishes over time as intervening events and contingencies become more common and more important (reflected also in declining values of R^2). In addition, the difference in this effect between parities diminishes over time and disappears in four years (from "after birth" to 1975).

The most important results in Table 6, for fertility status, also are the most difficult to interpret. Among couples who already had at least three children, the effect of another baby on wife's employment declines as the baby ages, particularly as the baby approaches school age (from 1974 to 1975). Among couples with only one or two children initially, however, there is no apparent trend in the effect of another baby. According to the available estimates of standard errors (which are biased and unreliable), the differences in effects of fertility status between parity groups and between equations are not significant. If, however, the coefficients are more reliably estimated than are the standard errors, then a consequence would be that estimates of the effect of fertility on employment would be smaller if samples are drawn at more advanced life cycle stages, or if the reference is to expected family size rather than to current family size (thus, somewhat clarifying Waite and Stolzenberg's results). This issue may deserve further research.

A DYNAMIC MODEL OF PLANS AND BEHAVIOR

The preceding models assume that people act spontaneously, subject to constraints from past actions and influences but without foresight or expectations. Certainly the models are incomplete, as people do respond to anticipated future conditions. Anticipations and plans affect behavior in two principal ways. First, they may affect actual causal relationships; for example, the effect of a baby on employment may depend on whether fertility status was planned or accidental (that is, unwanted pregnancies or sterility). Second, plans may not affect causal relationships within behavioral sequences but they may determine the number of such sequences. For example, fertility plans may not alter the impact of a baby, but they may determine how many babies are born. In this case, a major issue is whether plans are exogenous or are subject to revision pending behavioral experiences.

Employment and fertility plans were examined briefly, in Tables 1 and 2. The results resemble past research and, thus, establish the credibility of the data, but, otherwise, those models are unsatisfactory because they ignore the processes by which plans are formed. Incorporating plans into dynamic behavioral causal models is a formidable theoretical challenge. A modest beginning can be made with the Income Dynamics data by making strict use of temporal sequences. It is assumed that plans stated in 1972 causally precede behavior between 1972 and 1975, which, in its turn, causally precedes revised plans stated in 1976. Thus, for example, it is assumed that expected family size is reliably reported and that, over time, this expectation may change—that is, the 1972 and 1976 responses are not simply inaccurate measures of the same underlying, immutable Goal.[23] Substantial

---

[23] With these data it must also be assumed that spouses have identical expectations or that they re-

Table 7. Standardized Regression Coefficients from a Recursive Model of Wife's Fertility Status from 1972 to 1974 (DFF), Hours Employed before (BLF) and after (ALF) Having a Baby (or "Not Having a Baby in 1973"), and Hours Employed in 1975 (LF75).

	BLF	DFF	ALF	LF75
Fertility status (DFF)[a]			−.14	−.15
Employment before birth (BLF)		−.09	.52	.38
Husband's permanent wage rate adequacy	−.21	−.07	−.08	−.13
Wife's potential wage rate	.16	.07	.03	.09
Age of youngest child	.08	.06	.11	.19
Number of children[b]	−.07	−.24	−.01	.01
Wife's years worked since age 18	.45	−.01	.19	.16
Wife's age	−.29	−.14	−.21	−.25
Race	.07	.05	.02	.03
Catholic	−.06	.06	.01	.03
County labor market for women	.06	.10	−.02	−.04
R^2	.30	.10	.48	.35

[a] In the LF75 equation DFF is coded 2 for the 8 couples who had babies in both 1972 and 1974. In the ALF equation it is coded 1 for these same couples because ALF is measured after the first of these two births (that is, in 1973).

[b] Number of children is dichotomized, as in Tables 5 and 6.

support for these assumptions exists (Bumpass and Westoff, 1970; Hout, 1978; Namboodiri, 1972; 1974; Ryder, 1978), although alternative assumptions also are plausible and may be investigated elsewhere.

With these assumptions, dynamic recursive models of plans and behavior can be constructed from data on births from 1972 to 1974, employment in 1975 (after birth) as well as before birth, and expected labor force activity and fertility reported both in 1972 and in 1976. The sample used earlier to analyze 1976 plans (Table 2) is used again here, although couples with births in consecutive years or in 1975 have been excluded (N = 461). In this sample, plans and behaviors are related but distinct. Nearly 40% of the sample in 1972 expected to have more children, 29% did have another baby by 1974 and, in 1976, 14% still expected more children. In 1972, 54% of the wives expected to be employed "in the next few years" (42% actually were employed before birth). The correlation between wife's expected employment and expected family size in 1972 is −.12, but the correlation between expected employment and expected additional births is .14. In 1976, 72% expected to be employed soon; the correlation between employment plans in 1972 and 1976 is only .28.

Because of data limitations, the sample and model used here differ in several important respects from those used in the previous section to anlayze dynamic behaviors, and the present task is more complicated than merely adding plans to the previous model. Births during a six-year-period were studied above; most couples who were going to have a baby did so in that time interval, so the problem of confounding quantity and timing of births was slight. Here, because plans were ascertained only in 1972 and 1976, births during a three-year-period are studied. The equation for fertility status predicts decisions both to increase family size and not to postpone having the baby. A second difference is changed historical circumstances. For example, 53% of the 1968–1973 sample (Table 5) had a baby during the period of observation compared with 29% of the 1972–1974 sample; the difference is due both to the different time durations and to the accelerating decline in fertility from 1967 to 1975. Because of these differences, it is necessary to begin by comparing Table 5 with the same behavioral model estimated for the more recent sample (1972–1974 births), shown in Table 7.

The equation for hours employed before birth (BLF) in Table 7 is reasonably simi-

---

port the compromise, "couple's expectation," because expected family size was reported by the husband in 1972 and by the wife in 1976.

lar to the BLF equation in Table 5. In all cases, the coefficients have the same sign, and in both samples wife's age and work experience are the most important predictors of BLF. The magnitudes of several coefficients differ in the two samples, but these differences seem to be due to small differences in simple correlations in the two samples and can be attributed mostly to chance sampling fluctuations or measurement errors.

The effect of initial employment on fertility status (DFF) is the same in both samples. In both samples, also, older women were less likely to have another baby, but the importance of wife's age is much less in the 1972–1974 time period (Table 7). This may be due to historical conditions that affected timing decisions. Ward and Butz (1978) argue that births were compressed or accelerated from 1967 to 1972 and were being postponed from 1972 to 1975. Younger wives are much more likely to postpone births (because they have time to make them up), so the age difference in fertility diminished during this time period. Other differences between the DFF equations in Tables 5 and 7 seem to be due to high correlations between age and other variables and to sampling fluctuations magnified by multicollinearity. For present purposes, however, the most important conclusion is that the effect of prior employment (BLF) is the same in both samples.

The determinants of wife's employment after having a baby (ALF) are fairly similar in the two samples. Initial employment, work history, and fertility status are among the most important predictors in both samples and have consistent effects (same signs, similar magnitudes). The impact of a baby is smaller in the more recent sample, but the difference disappears when the earlier sample also is restricted to births during a three-year period (Cramer, 1979c) so it may be an artifact of timing decisions. The impact of a baby is due partly to increasing employment among wives who do not have a baby; the increase may be less if babies are simply being postponed. The reasons for the greater importance of age in the 1972–1974 sample are unclear, although clues may be found in trends in age specific female wage rates (Ward and Butz, 1978:49–51) and in the changing relationship between age and fertility discussed earlier. Coefficients for other variables are similar in the two samples.

Table 7 also includes a comparison of equations for wife's employment in 1975, and just after having a baby. As expected, the effect of initial employment is smaller at the more distant sequel (1975). Other variables have similar effects in the two equations.

The analysis of Table 7 indicates that fertility timing decisions and historical circumstances do affect the behavioral model, but that the major problems are with the exogenous variables. Relationships between employment and fertility are relatively robust and stable across samples, so plans can usefully be incorporated into the model, as long as attention is focused on these endogenous variables. Plans in 1972 are specified as correlated exogenous variables, and plans in 1976 are specified as outcomes of earlier plans and intervening behavior. In order to simplify the model, differences between initial parity groups are not reported and parity is not dichotomized; interactions with exogenous variables make little sense in this sample because of unclear patterns of fertility timing decisions, and interactions with endogenous variables give results very similar to those in Table 6. Results for the additive recursive model of plans and behavior are presented in Table 8.

Number of additional births expected in 1972 is a very strong predictor of actual births during 1972 to 1974. Most other variables affect fertility status only through their effects on fertility plans. About 10% of the couples who did not expect more children had a baby while 40% of the couples who expected more did not have a baby (yet), so most of the gap between planned and actual fertility is due to time truncation, deliberate postponements, and downward revisions of plans. Employment plans in 1972 had essentially no effect, directly or indirectly, on subsequent fertility. Actual employment before birth, however, is significantly associated with fertility status: employment plans, fertility plans, and other factors being equal, wives who worked more hours were less

Table 8. Unstandardized Regression Coefficients from a Recursive Model of Expected Family Size (EFS76) and Labor Force Plans (LFP76) in 1976, Wife's Hours Employed in 1975 (LF75), and Fertility Status from 1972–1974 (DFF)

	DFF	LF75	EFS76	LFP76	
				1	2
Hours employed in 1975, LF75 (000)			.04	.222	
Fertility status DFF[a]		−269	.76	.019	−.041
Fertility plans 1972[b]	.501	44	.19	.045	.055
Employment plans, 1972 LFP72[c]	.052	76	−.01	.131	.148
Hours employed before birth, BLF (000)	−.069	385	−.08	−.037	.049
Husband's permanent wage rate adequacy	−.039	−70	−.01	.004	−.012
Wife's potential wage rate	.058	101	.10	.072	.094
Age of youngest child	.004	86	−.03	.001	.020
Number of children[d]	−.030	13	.86	.018	.021
Wife's years worked since age 18	−.007	30	−.01	.005	.012
Wife's age	−.001	−47	−.02	−.017	−.027
Race	.037	87	.25	.028	.048
Catholic	.020	52	.15	.090	.101
County labor market for women	.004	−4	.01	−.002	−.003
R^2	.35	.36	.79	.25	.16

[a] DFF as a dependent variable is dichotomous, but as a predictor it is number of babies born (only 8 couples had 2 babies).

[b] Fertility plans in 1972 are dichotomous in the DFF equation and "number of additional children expected" in the other equations.

[c] Employment plans in 1972 are measured the same as in 1976: working at the time of interview or plans to work in the near future (dichotomous).

[d] Unlike in Tables 5–7, number of children (initially) is not dichotomized.

likely to have a baby. In addition, this relationship differs by planning status. Among couples who expected no more children, employment before birth is not related to fertility status (suggesting that the births were truly accidental). Among couples who expected more children, the effect of employment is −.108, larger than the average estimate in Table 8: other things being equal, the proportion of wives who had a baby was 11% lower, on average, for each 1,000 hours worked (the difference between full-time and half-time employment). The same effect of employment before birth is found for expected family size in 1976, so it indicates reduced (rather than delayed) fertility.

Employment in 1975 is unrelated to both employment plans and fertility plans in 1972, so the estimates for this equation in Tables 7 and 8 are similar and need little discussion. Employment in 1975 is strongly related to fertility status, prior employment, and economic pressures and incentives. The effect of fertility status does not depend on whether more children were expected in 1972. Too few couples expected more births, after 1976, for us to test the related conjecture that the effect of having a baby depends on whether it is the last child expected.

The main determinants of expected family size in 1976 are cumulative fertility (initial parity and additional births, with nearly equal effects), demographic constraints (wife's age and age of youngest child initially), race, and religion. Fertility plans in 1972 have a small additional effect, but employment plans in 1972, and actual employment in 1975, are irrelevant. As noted above, employment before birth does reduce expected family size in 1976. Because current family size is controlled, results for additional births expected in 1976 are identical to these results for expected family size.

Employment plans in 1976 depend mainly on actual employment after birth (in 1975), but earlier employment plans, wife's potential wage rate, and age also are significant. Prior expected and actual fertility have negligible effects on employment plans. Similar results are obtained in a second equation for employment plans in which actual employment is omitted, indicating that the fertility variables are not very important even indirectly. Conclusions from these results, however, must be qualified by the observation that none of the variables explains employment plans very well (that is, R^2 is small), undoubtedly due in part to the very crude way in which employment plans are measured.

The main conclusion to be drawn from Table 8 is that both employment plans and fertility plans depend primarily on recent behavior—employment plans on employment, and fertility plans on fertility.[24] In both cases there is little "cross-over," in the sense of one type of behavior affecting the other type of plan. Prior plans have only small direct effects on current plans, again with no cross-over. The recent behaviors, on which current plans depend, generally do not themselves depend on earlier plans, with the major exception of the strong relationship between expected family size and fertility. Rather, recent behaviors mainly depend on earlier behaviors (and exogenous factors).

Employment and fertility do not seem to be related by way of plans and expectations. There are two behavioral links, both noted in the previous section. First, employment before birth tends to reduce the probability of having more children. This effect is relatively strong among couples who expect more children and it is absent among couples who expect no more children.[25] Second, having a baby strongly depresses employment. This is true regardless of fertility expectations. The effect of a baby on actual employment seems to decline as the child becomes older, and the effect on employment plans is negligible. Thus, in the short run, fertility has a strong effect on employment, but, in the longer run, the effect of employment on fertility may be stronger.

Brief mention should be made of several other types of attitudes and expectations. Husband's attitude toward wife's employment was defined above in connection with Table 2. Wives also were asked in 1976:

> "How does your husband feel about having (more) children? Is he very much in favor of it, somewhat in favor of it, neither for nor against it, somewhat against it, or very much against it?"

This question is particularly interesting because expected family size was reported by husbands in 1972 and by wives in 1976. Both attitudes contribute significantly to the model of 1976 expectations,[26] but in very limited ways. Husband's support for his wife's employment is strongly related to her employment plans, but controlling for his attitude has no impact on the effects of other variables, and his employment attitude is unrelated to expected family size. Similarly, his reported desire for more children significantly affects expected family size but does not change the effects of other variables, and it is unrelated to wife's employment plans. Thus, incorporating these attitude items into the models does not change the results, but it tends to reinforce the impression that fertility plans and employment plans are not strongly related.

Finally, Butz and Ward (1977), Lindert (1978), and Willis (1973) have argued, in different ways, that the determinants of fertility are different depending on whether the wife intends ever to be employed or not. Eventual intentions were not asked in the Income Dynamics study. As an approximation, wives with under three years of employment experience since age 18, and who reported in both 1972 and 1976 no intention to seek employment soon, were excluded from the sample (39 cases). Models estimated for the remainder of the sample do not differ noticeably from Tables 7 and 8, so this hypothesis does not seem to be supported.

---

[24] The correlation between residuals from the 1976 expected family size and employment plans equations is .09, indicating that probably there is little simultaneous dependence between the two 1976 plans.

[25] It is assumed in this paper that the results indicate a causal effect of employment on fertility. An alternative interpretation (which cannot be tested with the available data) is that employment interacts with historical trends and cycles but does not, itself, cause fertility. The aggregate decline in total fertility rate and expected family size during the years of the Income Dynamics study may have occurred for reasons that are unrelated to the processes specified in our models, and employed wives may simply have responded to these historical trends first, or most enthusiastically. This would imply that the effect of employment on fertility is different under different historical circumstances.

[26] The attitudes are not used to predict actual behavior because of our adherence to strict temporal sequences. An alternative treatment is presented elsewhere (Cramer, 1979c). The correlation between the two attitudes is .03.

CONCLUSIONS

The dynamic models in Tables 6 and 8 permit a partial resolution of the debate about causal direction between fertility and wife's employment, but, as always, a complete synthesis must await further research. As many demographers have suspected (but recent research failed to show), causation appears to go in both directions. Certainly, in the short run, fertility has a large impact on wife's employment which overshadows any impacts in the opposite direction. The same result was obtained by Smith-Lovin and Tickamyer (1978), but their evidence is shaky because of the high likelihood of serious problems of multicollinearity. In addition, they probably underestimate the reciprocal effect of employment on fertility by counting years of employment but not hours per year. Finally, they tend to exaggerate the effects of fertility because their model emphasizes short-run impacts, as they readily acknowledge.

Estimating the impacts of fertility on employment is difficult because there may be several different types of impact. We have found that the effect of a baby does not depend on whether it was expected, but, as in previous research (Cramer, 1979a; 1979c), evidence on differences between birth orders is not conclusive. A more serious issue is that of distinguishing between the impact of an extra child and the effects of age of youngest child. Results reported here, and elsewhere (Bowen and Finegan, 1969; Cramer, 1979c; Lindert, 1978; Sweet, 1973), indicate that the impact of a baby declines rather sharply as it approaches school age, at least among higher birth orders. It seems very likely that most of the impact of a baby is due to its young age (except possibly with adoptions), and that, in the long run, the impact of fertility on employment may be relatively slight (depending, of course, on whether one measures annual or cumulative employment).

Waite and Stolzenberg rely heavily on this long-run argument to explain their finding that the dominant direction of causality is from expected labor force participation to expected family size. Our quasi-replications of their model give quite similar results with both 1972 and 1976 data on fertility and employment plans, despite moderate to high levels of multicollinearity. But the dynamic models of causal processes suggest that the static model of long-run plans is as misleading as it is correct. The main difficulty is that employment plans appear to be largely irrelevant; they generally have very little effect (directly or indirectly) on fertility status, actual employment after birth, and subsequent employment and fertility plans.[27] Actual employment, on the other hand, does reduce actual and expected fertility. This effect builds over time (as initial employment and reduced fertility both stimulate higher subsequent employment) so that, in the long run, employment may have a substantial effect on fertility. This process is influenced by exogenous factors such as economic incentives and pressures, but net of such factors there is no evidence that the long-run impact on expected or actual family size is a premeditated consequence of employment plans.

Fertility plans, on the other hand, are strongly related to actual fertility, although fertility status certainly is not completely planned, and plans can change. In addition, the effect of employment on fertility depends on whether more children are expected. Otherwise, fertility plans are important only indirectly through the effects of actual fertility. In particular, initial fertility plans are only weakly correlated with initial employment plans and have no direct effect on subsequent employment or employment plans. Nevertheless, the indirect effects of fertility plans are quite important, and it is ironic indeed that in static models employment plans—rather than fertility plans—seem to be the driving force.

---

[27] Admittedly, this may be due partly to poor measurement.

Because of the focus on the causal relationship between fertility and wife's employment, results for other variables have been discussed relatively briefly—and only to determine the adequacy of the data and models. Some of these results are substantively interesting, and it is worthwhile to summarize them here. The major theoretical basis for the models is the "new home economics," in which economic factors are presumed to be important determinants of household allocative decisions. The models of wife's employment before birth generally conform to economic expectations. The model of fertility status from 1968 to 1973 (in which timing considerations are minimized) also generally conforms to economic theories. Husband's economic resources stimulated fertility, while all three proxies for costs—wife's employment and potential wage rate, and age of youngest child—depressed fertility. Results from this model also support most of Hout's (1978) hypotheses about differences by parity. Other hypotheses about interaction effects proposed by Stolzenberg and Waite (1977), Willis (1973), and others were not supported, although the tests were not elegant or decisive.

The general drift toward dynamic models of fertility that characterizes recent research seems to be well advised. The static models estimated here are technically deficient and generally not very informative. Sequences of decisions about fertility and employment are influenced by such factors as economic incentives and pressures, but it seems unlikely that the sequences can be usefully summarized in terms of final outcomes and initial characteristics. Some contingencies, such as timing decisions associated with age and open interval, may be essentially random and can safely be averaged-out in static models. Other contingencies depend on aggregate historical trends and cycles, parity, and expected family size, and these cannot be understood in a static framework. Hout's explanations of interaction effects due to parity actually depend upon comparisons of parity with both social norms and individual fertility preferences, so these interactions are a specific example of contingencies that are best understood in dynamic models.

The case for dynamic models is weakened by at least three structural deficiencies in the present models (other, more specific, deficiencies will be noted later). First, the models incorporate fertility expectations only at middle and late stages of family formation. Information on the determinants of initial expectations and early changes in expectations is needed for a complete comparison of static and dynamic models. Second, our models treat households mainly in isolation. The only clear link to society is county demand for female labor, and this link is inadequate. Finally, it is unclear whether historical changes are endogenous or exogenous to the models. For example, aggregate changes in the timing of births were considered exogenous constraints in the anlaysis of Table 7, but with data on individuals' perceptions of business cycles and short-term economic prospects, the dynamic models should explain decisions about the timing of births. Clearly, the problem of relating aggregate trends and cycles with individual life cycle dynamics is related to the broader problem of linking household to society (Nerlove, 1974).

The discussion of fertility and female employment has been used to illustrate several methodological problems or issues. The methodological lessons certainly are not original, and sometimes are pretty mundane (for example, measuring income correctly), yet they seem often to be overlooked or poorly understood. Three general points have been illustrated.

(1) Nonrecursive models are not cheap devices for easily determining causal direction. The main problem, aside from specifying all relevant variables (a problem in all models), is weak instrumental variables, or multicollinearity. This is a common problem, because strong predictors are likely to directly affect both endogenous variables, especially in static models. The theoretical challenge of identifying instrumental variables is matched, especially with microlevel data, by the empirical problem of finding good instruments. If instruments are weak, the

"medicine" of 2SLS may be worse than the "disease" of unclear causal direction.

(2) Many research reports conclude with an expression of hope that dynamic (especially panel) models can solve problems of causal direction. This may be true, but dynamic models have their own inherent problems. Many of these problems involve time intervals. Problems encountered in the present research include the lag time between initial employment and fertility (compounded by measures defined over intervals, not at points in time, with the hazard of reciprocal effects from pregnancy to initial employment), when to measure consequences of fertility, and the length of the panel period (that is, the problem of time truncation). The latter problem was partially solved by studying behavioral sequences instead of time intervals, but this may not always be possible or appropriate.

(3) Historical trends and cycles may impinge significantly on dynamic models of individual behavior. For example, the relationship between fertility and employment may vary over the business cycle. Consideration of historical changes almost invariably creates problems of distinguishing among age, period, and cohort effects. The comparison of Tables 5 and 7 emphasized age and period effects, but the two samples also differ significantly with respect to cohort composition so the interpretation of Table 7 may be incorrect.

The discussion of methodological "lessons" is not intended to imply that the present research is exemplary. In fact, there are rather serious faults, and these will be mentioned briefly to indicate directions for future research. The most obvious problem is the specification of variables. Fertility and employment plans and intentions (including timing preferences) need to be measured much more carefully and extensively. A number of potentially important variables are missing from the present models: sex role attitudes, age at marriage, paid child care and family help, wife's commute time and other "costs" of employment, relative income, and child-rearing attitudes and practices are clear candidates. Equally obvious problems are the numerous simplifying assumptions that have been made in order to estimate and interpret the dynamic models. The major statistical and mathematical issues associated with dynamic models are complex and not yet fully understood (Blalock, 1969; Coleman, 1968; Cook and Campbell, 1979; Hannan and Young, 1977; Hibbs, 1974; Johnston, 1972: 379–80; Wheaton et al., 1977), but it is clear that they are at least as challenging as the problems with static nonrecursive models.

REFERENCES

Atkinson, S. E.
1978 "Small-sample properties of simultaneous equation estimators with multicollinearity." Journal of the American Statistical Association 73:719–23.

Bagozzi, Richard P., and Frances Van Loo
1978 "Fertility as consumption: theories from the behavioral sciences." Journal of Consumer Research 4:199–228.

Blalock, Hubert M.
1969 Theory Construction. Englewood Cliffs: Prentice-Hall.

Bowen, William G., and T. A. Finegan
1969 The Economics of Labor Force Participation. Princeton: Princeton University Press.

Bumpass, Larry, and Charles F. Westoff
1970 The Later Years of Childbearing. Princeton: Princeton University Press.

Butz, William P., and Michael P. Ward
1977 "The emergence of countercyclical U.S. fertility." Rand:R-1605-NIH.

Cain, Glen, and Martin D. Dooley
1976 "Estimation of a model of labor supply, fertility, and wages of married women." Journal of Political Economy 84:S179–99.

Cain, Pamela S.
1979 "The determinants of marital labor supply, fertility, and sex role attitudes." Presented at annual meeting of Population Association of America, Philadelphia.

Carlsson, Gusta
1972 "Lagged structures and cross-sectional methods." Acta Sociologica 15:323–41.

Chatterjee, Samprit, and Bertram Price
1977 Regression Analysis by Example. New York: Wiley.

Coleman, James S.
1968 "The mathematical study of change." Pp. 428–78 in Hubert M. and Anne Blalock (eds.), Methodology in Social Research. New York: McGraw-Hill.

Cook, Thomas D., and Donald T. Campbell
1979 Quasi-Experimentation. Chicago: Rand McNally.

Cramer, James C.
1979a "Employment trends of young mothers and the opportunity cost of babies in the United States." Demography 16:177–97.
1979b "The effects of fertility on husband's economic activity: evidence from static, dynamic, and nonrecursive models." Re-

search in Population Economics. Vol. 2, forthcoming.
1979c "The impacts of a child on parents' time use and income: multiple estimates of levels, trends, and differentials." Unpublished ms.
Duncan, Greg, and James N. Morgan
1976 A Panel Study of Income Dynamics: Procedures and Tape Codes, Wave IX. Ann Arbor: Institute for Social Research.
Duncan, Otis Dudley
1975 Introduction to Structural Equation Models. New York: Academic Press.
Easterlin, Richard A.
1978 "The economics and sociology of fertility: a synthesis." Pp. 57–133 in Charles Tilly (ed.), Historical Studies of Changing Fertility. Princeton: Princeton University Press.
Fisher, Franklin M.
1971 "The choice of instrumental variables in the estimation of economy-wide econometric models." Pp. 245–73 in Hubert Blalock (ed.), Causal Models in the Social Sciences. Chicago: Aldine.
Fox, John
1978 "Simultaneous equation models and two-stage least squares." Pp. 130–50 in Karl Schuessler (ed.), Sociological Methodology 1979. San Francisco: Jossey-Bass.
Goldberger, Arthur S.
1973 "Structural equation models: an overview." Pp. 1–18 in Arthur Goldberger and O.D. Duncan (eds.), Structural Equation Models in the Social Sciences. New York: Seminar.
Hannan, Michael T., and Alice A. Young
1977 "Estimation in panel models: results on pooling cross-sections and time series." Pp. 52–83 in David R. Heise (ed.), Sociological Methodology 1977. San Francisco: Jossey-Bass.
Hawthorn, Geoffrey
1970 The Sociology of Fertility. London: Collier-Macmillan.
Heckman, J. J., and R. J. Willis
1976 "Estimation of a stochastic model of reproduction: an econometric approach." Pp. 99–138 in Nestor E. Terleckyj (ed.), Household Production and Consumption. New York: National Bureau of Economic Research.
1977 "A beta-logistic model for the analysis of sequential labor force participation by married women." Journal of Political Economy 85:27–58.
Hibbs, Douglas A., Jr.
1974 "Problems of statistical estimation and causal inference in time series regression models." Pp. 252–308 in Herbert L. Costner (ed.), Sociological Methodology 1973–74.
Hout, Michael
1977 "A cautionary note on the use of two-stage least squares." Sociological Methods and Research 5:335–45.
1978 "The determinants of marital fertility in the United States, 1968–70: inferences from a dynamic model." Demography 15:139–60.
Johnston, John
1972 Econometric Methods, Second Edition. New York: McGraw-Hill.
Land, Kenneth C., and Marcus Felson
1978 "Sensitivity analysis of arbitrarily identified simultaneous-equation models." Sociological Methods and Research 6:283–307.
Lindert, Peter
1978 Fertility and Scarcity in America. Princeton: Princeton University Press.
Luskin, R. C.
1978 "Estimating and interpreting correlations between disturbances and residual path coefficients in nonrecursive (and recursive) causal models." American Journal of Political Science 2:444–74.
Masnick, G. S., and J. A. McFalls, Jr.
1976 "A new perspective on the twentieth-century American fertility swing." Journal of Family History 1:217–44.
Namboodiri, N. K.
1972 "Some observations on the economic framework for fertility analysis." Population Studies 26:185–206.
1974 "Which couples at given parities expect to have additional births? an exercise in discriminant analysis." Demography 11:45–56.
1975 "Review symposium of Schultz's 'economics of the family'." Demography 12:561–9.
Nerlove, Marc
1974 "Household and economy: toward a new theory of population and economic growth." Journal of Political Economy 82:S200–18.
Rao, Potluri, and R. L. Miller
1971 Applied Econometrics. Belmont: Wadsworth.
Rosenzweig, Mark R.
1976 "Female work experience, employment status, and birth expectations: sequential decision-making in the Philippines." Demography 13:339–56.
Rosenzweig, Mark, and D. A. Seiver
1975 "Comment on Namboodiri's 'Which couples at given parities expect to have additional births?'" Demography 12:665–8.
Ryder, N. B.
1973 "A critique of the national fertility study." Demography 10:495–506.
1978 "A model of fertility by planning status." Demography 15:433–58.
Ryder, Norman B., and Charles F. Westoff
1971 Reproduction in the United States, 1965. Princeton: Princeton University Press.
Smith, James D.
1972 "Birth control and economic well-being." Pp. 501–22 in Burkhard Strumpel, James N. Morgan, and Ernest Zahn (eds.), Human Behavior in Economic Affairs. San Francisco: Jossey-Bass.
Smith-Lovin, Lynn, and Ann R. Tickamyer
1978 "Labor force participation, fertility be-

havior, and sex role attitudes." American Sociological Review 43:541–56.

Stolzenberg, Ross M., and Linda J. Waite
1977 "Age, fertility expectations, and plans for employment." American Sociological Review 42:769–82.

Sweet, James A.
1972 "Labor force reentry by mothers of young children." Social Science Research 1:189–210.
1973 Women in the Labor Force: New York: Seminar Press.

Turchi, Boone A.
1975a The Demand for Children: The Economics of Fertility in the United States. Cambridge: Ballinger.
1975b "Microeconomic theories of fertility: a critique." Social Forces 54:107–25.

Waite, Linda J., and Ross M. Stolzenberg
1976 "Intended childbearing and labor force participation of young women: insights from nonrecursive models." American Sociological Review 41:235–51.

Ward, Michael P., and William P. Butz
1978 "Completed fertility and its timing: an economic analysis of U.S. experience since World War II." Rand, R-2285-NICHD.

Wheaton, Blair, Bengt Muthen, Duane F. Alwin, and Gene F. Summers
1977 "Assessing reliability and stability in panel models." Pp. 84–136 in David Heise (ed.), Sociological Methodology 1977. San Francisco: Jossey-Bass.

Whelpton, Pascal K., A. A. Campbell, and J. E. Patterson
1966 Fertility and Family Planning in the United States. Princeton: Princeton University Press.

Willis, Robert J.
1973 "A new approach to the economic theory of fertility behavior." Journal of Political Economy 81:S14–S64.

MANUSCRIPTS FOR THE
ASA ROSE SOCIOLOGY SERIES

Manuscripts (100 to 300 typed pages) are solicited for publication in the *ASA Arnold and Caroline Rose Monograph Series*. The Series welcomes a variety of types of sociological work—qualitative or quantitative empirical studies, and theoretical or methodological treatises. An author should submit three copies of a manuscript for consideration to the Series Editor, Professor Suzanne Keller, Department of Sociology, Princeton University, Princeton, NJ 08540.

POLITICAL PROTEST ORIENTATIONS AMONG BLACK AND WHITE ADULTS*

LARRY ISAAC
Florida State University

ELIZABETH MUTRAN
Duke University

SHELDON STRYKER
Indiana University

American Sociological Review 1980, Vol. 45 (April):191–213

The political protest activities of the late 1960s and early 1970s stimulated a flurry of empirical research by social scientists seeking explanations. Most of the research, aimed at explaining individuals' radical political orientations, willingness to use protest and perhaps violence, or actual riot or protest participation, has been guided by either a mass society perspective or some variant of the relative deprivation model. After outlining the basic elements of these two theories as well as an alternative differential socialization theory, central causal features of each theory are integrated into a structural equation model designed to explain individual political protest orientation, and the model (allowing for measurement error in the endogenous constructs) is estimated on separate samples of white and black adults. The results indicate strong race-specific differences in the formation of political orientations. Explanations for these differences are examined; the empirical patterns are interpreted as supporting a theory of differential political socialization.

INTRODUCTION

The widespread protest activity and collective violence of the 1960s and early 1970s,[1] outgrowths of the civil rights and antiwar movements, stimulated considerable research into the "preconditions, causes, dynamics, efforts to control and political significance of disorder" (Snyder and Kelly, 1979:1). Much of this research was guided (either explicitly or implicitly) by one of two relatively longstanding explanations for protest orientations and activity: a mass society explanation emphasizing political primitivism and isolation, or some variant of the relative deprivation argument predicated on a psychological frustration-aggression paradigm (see Useem, 1975, for cogent overviews). Particular versions of these arguments have received research attention at both aggregate and individual levels of analysis. The focus here is at the individual level, consistent with recent arguments concerning the appropriate level of analysis in examining various theories of collective political protest (see, especially, Snyder, 1978).

There is an alternative view of protest orientation and participation processes,[2] arguing the significance of differential political socialization (Portes, 1971a; 1971b)[3] and seeing protest orientations as

---

* Address all communications to: Sheldon Stryker, Department of Sociology, Indiana University, Bloomington, Indiana 47401.

The ordering of authors is alphabetical. This research was partially supported by United States Public Health Service Grants MH 16939 and MH 14660. Thanks are due George Bohrnstedt, Peter Burke, Larry Griffin, Lowell Hargens, Elton Jackson, William Kelly, Robert Parker, Alejandro Portes, David Snyder, John Zipp and anonymous referees for helpful comments on drafts of this paper.

[1] Snyder and Kelly (1979) cite their U.S. city-level data as well as Spilerman's (1970) as evidence for the unprecedented nature of racial violence during this period.

[2] There are, of course, additional alternative views; for example, the more purely "political model" that focuses on variables such as political efficacy, trust, etc. (see Gamson, 1968). We think this political model overlaps with what we here refer to as the differential political socialization perspective. However, we do not deal directly in our analysis with this particular alternative.

[3] The assembling process framework developed by McPhail and Miller (1973) might be considered as a behavioral analogue to the differential political socialization framework, since the former stresses the flow of information, social density and other proximate features of context which either facilitate or impede the assemblage of persons in time and space.

the product of a complex learning process tied to social structure. This view does not necessarily see relative deprivation or isolation/integration as unimportant; they may well be aspects of the protest orientation formation process, but are likely to have varying effects in subgroups differently located in the social structure.

To examine components of these theoretical perspectives, we focus on individual level political protest orientations. Protest orientation is central to both the mass society and relative deprivation explanations of protest activity: the causal imagery embedded in these perspectives typically takes some version of protest orientation ("political radicalization," "politicalized discontent," "potential for political violence," etc.) as antecedent to actual behavioral or participation dimensions (Useem, 1975). In a sense, then, we provide a partial (but central) test of the traditional arguments. Whatever the direct relation of orientation to activity, however, protest orientation is important to understand because widespread approval of protest and violence as means to political ends creates a climate of support (and actual support) for the use of protest and violence. The relevance and implications of focusing on protest orientations rather than actual protest involvement are elaborated later.

Research on protest orientations and behaviors has had persistent shortcomings: (a) hypotheses tested generally are not tied explicitly to theoretical framework; thus, basic underlying assumptions typically do not surface (see Portes, 1971a; 1971b for major exceptions); (b) measurement of central explanatory concepts such as relative deprivation often fit these concepts badly (cf., McPhail, 1971; Crosby, 1976); (c) most studies do not go beyond the examination of simple bivariate measures of association between theoretically important variables (see McPhail's, 1971, extensive review); (d) few researchers have compared the strength of crucial relationships, or specified how processes might operate differentially, in important subpopulations (e.g., blacks and whites, or middle-class and working-class students [but see Long's, 1977, comparison of black and white adolescents]); and (e) virtually none of the many investigations have attempted to elaborate models integrating central components from more than one theoretical perspective (see Portes and Ross, 1974, for an empirical exception; Korpi, 1974, and Orum, 1974, for theoretical exceptions). None among the various exceptions cited simultaneously avoids all of the shortcomings mentioned.

All are addressed, in varying degree, in this article. Specifically, we (1) juxtapose three major explanations for protest orientations: versions of the mass society, relative deprivation, and differential political socialization arguments; (2) develop and employ a more theoretically grounded measure of relative deprivation (based on labor market experience) than is typically used; and (3) integrate major lines of causal argument in a structural equation model that is estimated separately for a sample of urban white and black adults. We think this race-specific analysis is crucial; it allows us to examine expected differences in the influence of important variables by race anticipated by the differential political socialization perspective.

THEORETICAL OVERVIEW

This section contains a general review of the three perspectives noted. It also makes three important points: First, there are critical differences in assumptions and causal mechanisms suggested by each perspective; second, political orientation toward the use of protest and collective violence is a central construct in the perspectives; third, while the mass society and relative deprivation perspectives tend to predict common effects of central causal variables in any subgroup in a population, the differential socialization approach is capable of accounting for differences between subgroups.

1. Mass Society

A number of variants of mass society theory (Kornhauser, 1959; Lipset, 1963; Fromm, 1963; Hoffer, 1966; Gusfield, 1962) exist which have several common features. All are concerned with "political extremism," a tendency to antidemocratic politics. Since compromise and a gradualist view of change are cornerstones of democratic politics, individuals who harbor favorable attitudes toward violence or indeed participate in protest or violent demonstrations are by definition extremists.

The making-of-the-extremist is seen to be due to special features of social life, particularly in the lower classes of mass societies.[4] "Political primitivism" (Portes, 1971a) is presumably produced by the lack of education and general information characteristic of the lower classes, and political primitives are expected to be prone to extremism (Lipset, 1963: chap. 4).

Social isolation, taken as characteristic of the lower-class social situation as well as a basic feature of mass societies, also presumably contributes to an extremist orientation toward politics (Gusfield, 1962:23). Thus, primary group integration and secondary association participation are central features in mass society theory's causal scheme. Primary group instability and consequent social isolation among the lower classes are sources of a lack of integration into the larger social order, as is the relative lack of secondary association participation (participation in schools, community organizations, etc.) through which the tenets of the democratic political process are thought to be learned.

The political imagery of mass society theory is linked to pluralist theory as its obverse; that is, extremism is viewed as an irrational political form within a rational political structure (Gusfield, 1962; Gamson, 1975). From this view, political orientations and actions are simplistic, do not reflect concrete socioeconomic interests, and derive from a particular irrational social psychology inherent in mass society (Portes, 1971a).

The mass society thesis thus translates into four hypotheses specifying conditions leading to political extremism: (1) primitivism (i.e., lack of education and general information), (2) structural and normative isolation of groups,[5] (3) individual isolation and detachment from secondary associations, and (4) individual isolation and detachment from primary groups.

Empirical work has found little support for the primitivism hypotheses (cf., Flacks, 1967; Marx, 1967; Paige, 1971; Portes, 1971a; 1971b).[6] The primary group integration hypothesis has received the least attention of all the basic mass society hypotheses. Moreover, what evidence does exist is not systematic enough to allow an informed judgment (but see Warren, 1970; Gerlach and Hine, 1970; Portes, 1971a; Pinard, 1971; Orum, 1972; Oberschall, 1973; Woelfel et al., 1974; Marx and Wood, 1975; Wilson and Orum, 1976, for predominantly negative evidence and theoretical arguments contrary to the mass society position). Our anlaysis bears on these two mass society hypotheses.

2. *Relative Deprivation*

Probably the most common explanation for the racial protest and radicalization of the black population during the 1960s and early 1970s is some version of the relative deprivation argument (e.g., National Advisory Commission's report on civil disorders, 1968). Typically, the gap between individuals' expectations and achievement on some value is posited as the primary force moving individuals to radical political orientations and activating participation in protest activities (e.g., Feierabend and Feierabend, 1966; Geschwender and Geschwender; 1973; Grofman and Muller, 1973; Gurr, 1970; Sears and McConahay, 1970).

---

[4] A fear of the lower classes is a central feature of the "elitist" view of the revolutionary crowd in history in which mass society theory has its roots. For a succinct overview of the images of the crowd and general theoretical frameworks embedded in the historical writing of the French Revolution, see Tilly (1964).

[5] This is the collective counterpart to individual isolation, also identifiable in the mass society literature (e.g., Kerr and Siegel, 1954). However, we do not deal with this particular version of the argument.

[6] Interestingly enough, the "primitivism" notion of the mass politics literature predicts the same negative effect of various dimensions of socioeconomic position (e.g., education, occupation, income) on political extremist orientations and actions as do absolute deprivation arguments (see, e.g., Caplan, 1970). They differ, however, in the interpretation of the processes thought to produce such effects.

Most empirical studies do not conceptualize the relationship of the perception or objective indication of what one has relative to what one thinks one should have as the primary causal variable in relating an expectations-achievement gap to radical political orientations. Rather, the inferred psychological force of frustration-anger[7] intervenes to produce the outcome. This suggests that the subjective evaluation of any perceived gap is central to political radicalization.

A large number of studies have tried to assess various forms of the relative deprivation argument. In general, these studies provide little convincing evidence of the impact of relative deprivation on protest orientations (see Muller, 1972; Grofman and Muller, 1973), individual protest participation (see McPhail, 1971, for a review) or protest/riot incidence in ecological units (see Snyder, 1978, for a review). Apart from theoretically based doubts concerning the centrality of relative deprivation in explaining collective violence incidence (e.g., Snyder and Tilly, 1972) or participation (e.g., McPhail, 1971), there are methodological grounds for remaining unconvinced by extant research. Specifically, these studies are plagued by two fundamental analytical problems beyond the fact that typically only bivariate relationships are examined (McPhail, 1971).

First, measures of relative deprivation bear only weak or partial resemblance to theoretical statements. The most pervasive measurement problem is that relative deprivation indicators typically do not assess anything beyond a perceptual difference in standing on some dimension of well-being (e.g., occupation, income, housing, etc.) thought to be important (e.g., Crawford and Naditch, 1970; Muller, 1972; Grofman and Muller, 1973; Geschwender and Geschwender, 1973). Such measures assume that perceived differences are synonymous with the deprivation-frustration-aggression model upon which they are based. There is, however, no reason to assume a priori that a perceived difference is a source of frustration or anger, much less an antecedent to radical political orientations or aggression (see also McPhail, 1971:1063). A sound measure of relative deprivation should include not only a value and comparison standard (Geschwender and Geschwender, 1973:404), but also the respondent's feeling-state toward the differential (cf., Morrison, 1973:153 for a similar but less explicit view).[8] In the analysis presented later, measures of relative deprivation are operationalized in precisely this way.

Second, there is the pervasive problem of treating societies as monoliths in aggregate level studies (Miller et al., 1977), or assuming constant relative deprivation effects across important subgroups in individual level studies.[9] Not only can aggregate data conceal important group differences, but individual level studies which lump together important subgroups (e.g., blacks and whites) may also mislead by assuming that relative deprivation is translated into political orientations in the same way in those subgroups. This is obviously a highly questionable assumption[10] which should be empirically examined.

3. *Differential Socialization*

Noting a consistent lack of support for the primitivism and isolation hypotheses of mass society theory in explaining leftist radical political orientations in a sample of urban slum-dwellers in Chile, Portes (1971a:829) points to four assumptions of that theory which his findings contradict: that leftist radicalism is (1) a simplistic political response, (2) an irrational orientation, (3) an abnormal or deviant process of attitude formation, and (4) a reactive formation process.

---

[7] Rather than a primitive frustration-anger response, an injustice-indignation response is probably more appropriate conceptually (see Lupsha, 1971).

[8] Nagel's (1974) argument is consistent with ours. His measure, however, is formulated at the aggregate level and so is unable to adequately capture the logic he so painstakingly puts forth.

[9] Even in individual level studies few group comparisons can be found. For a comparison of black and white adolescents, see Long (1977); for a comparison of race-region-specific results, see Miller et al. (1977); and for a comparison of males and females, see Orum and Orum (1968).

[10] Theoretical justification for this doubt is presented in the following section.

As an alternative interpretation of his data, Portes argues that leftist radicalism emerges within the context of a normal, complex attitude formation or learning process, "a period of intellectual apprenticeship" (Portes, 1971a:829) in which the personal problems of living in a lower-class situation come to be seen within "the general interpretative framework of a coherent theory of classes, class-interests, and class conflict" (Portes, 1971a:829).[11] This alternative suggests that differential socialization is the source of leftist radicalism. Leftist radicalism is seen as the outcome of exposure to a structurally oriented political ideology learned over a period of time through a variety of interactional or information sources—for example, from parents and peers, or through referent others in occupational or neighborhood milieux who have learned such orientations.[12] The differential socialization model thus relies on processes of interaction and information flow to explain radical political orientations, rather than relying on an "abnormal" explanation which sees this outcome as a response to precisely the opposite set of conditions (the lack of information, detachment, and isolation).

Contrary to the more orthodox perspectives, radical political orientations and protest participation are viewed as predominantly rational by this perspective. Embedded in a primary group structure, identified with persons—family, friends, neighbors—in that structure, protesters seek to advance the interests of self and others who compose that structure (see Wilson and Orum, 1976, for a relevant discussion). In this sense, the differential socialization view converges with emerging resource mobilization perspectives (cf., Tilly, 1978; Oberschall, 1973; Gamson, 1975), and may be useful in augmenting the individual interest component of mobilization models.[13]

The differential socialization perspective has still other implications worth noting. It leads us to view broad structural positions (e.g., race and class) as operating through interactional and "definitional" processes to affect political orientations and behaviors. It suggests that the impact of other influences on protest orientations and behavior—relative deprivation, for one—will vary with the "embeddedness" of people in group structures and the interests of those groups. It thus leads us to expect the varying import of such influences in different subgroups of a population. While the orthodox perspectives—mass society and relative deprivation—predict basically identical influences of forces such as isolation or relative deprivation throughout a population, the differential socialization argument does not. Beginning with this framework, we find that there are sound structural reasons for expecting the formation of protest orientations to vary by race,[14] reasons resting on important aspects of the political economy of race in American society.

---

[11] "Normal," here, should not be confused with prevalent. While the formulation of such political orientations may be said to occur through a normal learning process, the likelihood of learning abstract interpretive frameworks which link concrete working-class experience to a coherent theory of classes, etc., is not great. Mann (1970:437) suggests the explanation for this rather rare occurrence rests in the performance of dominant institutions (e.g., schools): "What has been ignored in childhood is unlikely to be grasped in adulthood, given working class difficulties with abstract concepts."

[12] There are, of course, other agents or institutions of political socialization. One which has received considerable attention is the school system. Such studies have typically concluded that schools (at least in the U.S. and Britain) are channels of political conservatism, especially in the case of working-class students (Mann, 1970). On the learning of deviant or radical political orientations, Mann (p. 436) states: "Presumably the working class child learns the latter from his *family* or *peers*; certainly he experiences something of the former when he enters the world of work, . . ." (emphasis added).

On the efficacy of schooling and work experience for children and adults respectively, see Orum (1976). However, Mann does note (p. 436, footnote 6) that there is conflicting evidence on the impact of the family in particular. There is evidence to suggest the "liberalizing" influence of the qualitative dimension of colleges in the U.S. (e.g., Knoke and Isaac, 1976). However, this finding does not refer to political means that might be employed in effecting change (our focus here) or on more radical political orientations concerning fundamental economic and political institutions (e.g., as in Portes, 1971a).

[13] It has become increasingly apparent that individual level processes need to be considered to fully explain protest, insurgency and collective violence (see, e.g., Snyder, 1978). While it is beyond the scope of this paper to develop, it is worth suggesting that this differential socialization approach may provide a logical individual level extension of the more organizationally oriented resource mobilization perspective.

Political economy of race and differential socializations. Race is obviously a highly salient ascribed status in American society, one which has been shown to be relevant in other areas of socialization (e.g., Porter, 1974, on educational and occupational attainment processes). Important racial differences have been shown in protest attitudes and participation, with blacks viewing protest consistently more favorably than whites (Eisinger, 1974).[15] Yet detailed arguments suggesting why we should expect such interactions by race in the formation of protest orientations have not been adequately spelled out.

There are several strong economic reasons for expecting the socialization process to differ by race; these ought to be mentioned but need not be rehearsed at length. Most simply stated, blacks have not done well in American society economically, whether measured absolutely or in human capital terms—i.e., relative to "investment." In the late 1960s, significant inequalities between whites and blacks remained on virtually every major dimension of material well-being: education, employment, income, wealth, etc. (e.g., see Franklin and Resnick, 1973; Farley, 1977; Jhabvala, 1977). Too, underlying labor market experiences have a basically dualistic structure, with economic institutions playing an important part in allocating blacks differentially into relatively disadvantaged sectors (e.g., see Baron and Hymer, 1977, on the Chicago labor market). More generally, race historically has been a major source of labor market segmentation in the United States (Edwards et al., 1975). And, of course, the labor market experience of many blacks has frequently been transitory and subject to repeated interruptions.

The civil rights movement did little to alter racially based economic inequalities, but did change political conditions considerably (Piven and Cloward, 1977). The post-World War II period undoubtedly saw an increased perception of political efficacy among blacks, both in terms of protest and institutionalized politics. There is evidence, for example, that welfare purses and rolls expanded nationally as a function of black political disorder (Piven and Cloward, 1971; Isaac and Kelly, 1979). There is also evidence of increased participation in more conventional or institutionalized politics (Danigelis, 1978), perhaps spilling over from the heightened sense of insurgent-based political efficacy.[16] It is these basic changes in the political climate for blacks, together with the relatively unchanged economic circumstances, which provide the grounds for expecting the political orientation formation process to differ by race.

Changes in the political climate of the sort noted here probably interacted with economic inequalities and labor market experiences to alter definitions of longstanding human capital problems among blacks. The argument previously made concerning relative deprivation is that the conceptualization of that phenomenon must go beyond an objective or perceived gap to the definition of that gap in negative terms that leads to frustration and anger. Those who experience labor market-based deprivations and learn to interpret them structurally or as the responsibility of the system—and integration into webs of affiliation facilitates this learning—are more likely to define the gap negatively, to become frustrated by and angry about it, and subsequently to develop radical political protest orientations. The civil rights and the welfare rights movement provided black Americans with the opportunity for learning "appropriate" political interpretations. Working-class or poor whites may have experienced similar labor market problems, but lacked the interactional base for this political learning process.

---

[14] It would, of course, be possible to present compelling theoretical reasons for expecting different processes within class categories. Given the lines along which protesters were mobilized in the late 1960s and early 1970s, comparing the processes by race is an important first step.

[15] Eisinger's (1974) results are based on survey data similar to ours taken from Milwaukee in 1970.

[16] The gap in black-white institutionalized political participation reversed after 1960 and the peak for blacks occurred over the 1964–1968 period for both North and South (Danigelis, 1978). This was also the peak of racial disturbance frequency and severity (Isaac and Kelly, 1979).

THE GENERIC MODELS

Prior attempts to evaluate the central hypotheses of one or the other of the theories reviewed as they may apply to the formation of protest orientations or to actual protest participation have not integrated hypothesized causal factors into a single model. By failing to do so, the risk is run of interpreting spurious relationships, as is the risk of misreading the evidence concerning alternative theoretical explanations. We want to motivate a simple generic causal model which subsequently can be more elaborately specified and estimated. The value in doing so is that several of the central alternative predictions of competing explanations can then be evaluated simultaneously.

Figures 1-A through 1-C represent the generic models underlying the components of mass society and relative deprivation theory on which we focus.

Figure 1-D represents a simplified generic version of the model which we will subsequently estimate. This model incorporates central causal components of the mass society,[17] relative deprivation and differential socialization arguments outlined. Individuals' background characteristics are taken to be reasonable indicators of location in the social structure. The ultimate (endogenous) variable of interest is political protest orientation. We consider relative deprivation and primary group integration as intervening between the background factors and protest orientation. While it would be possible to develop causal arguments concerning the relationship between relative deprivation and primary group integration, this is not the focus of our analysis, and we leave the relationship between these intervening constructs unanalyzed. Both intervening constructs and protest orientations are taken to be partial functions of social structural position.[18]

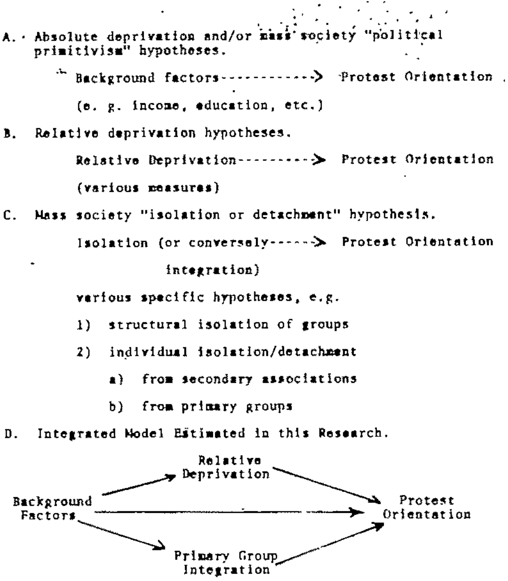

A. Absolute deprivation and/or mass society "political primitivism" hypotheses.
Background factors----------> Protest Orientation
(e. g. income, education, etc.)
B. Relative deprivation hypotheses.
Relative Deprivation---------> Protest Orientation
(various measures)
C. Mass society "isolation or detachment" hypothesis.
Isolation (or conversely-----> Protest Orientation
integration)
various specific hypotheses, e.g.
1) structural isolation of groups
2) individual isolation/detachment
a) from secondary associations
b) from primary groups
D. Integrated Model Estimated in this Research.
Relative Deprivation
Background Factors
Protest Orientation
Primary Group Integration

Figure 1. Generic Models for the Determination of the Protest Orientation Formation Process

DATA AND METHODS

The Sample

Our data come from a survey directed by one of the authors (Stryker), focusing on political attitudes and orientations, carried out as the 1969 Gary Area Project of the Institute of Social Research at Indiana University. Interviews were conducted during January, 1969, by trained graduate students and professional interviewers. The survey employed probability sampling of blocks (and block clusters), relying on quota sampling of individuals within blocks (a procedure much like that used by NORC).[19] The final sample consisted of 800 adult residents of Gary, Indiana, including 432 whites and 357 blacks. Eliminating all respondents for whom there were missing data on any of the variables in our model resulted in a useable sample, for the present analysis, of 247 white and 204 black adults. Tests employed to determine whether deleted cases (those with missing data) were different from those retained, with respect to the dependent protest orientation variable, revealed no significant difference for either racial group (see Cohen and Cohen, 1975, for further discussion of the procedure).[20]

---

[17] The mass society hypothesis (other than the primitivism argument) for which we have data and that would logically fit into the integrated model is the primary group isolation/detachment hypothesis.

[18] In fact, as repeatedly noted, we see relative deprivation as arising from more than structural position alone. It should arise from structural positions that place one in a position of being differentially treated due to ascribed characteristics; the actual difference must be perceived, and once perceived must be evaluated negatively, and not interpreted as an individual shortcoming. The negative evaluation is most problematic, involving learning to interpret the differential within a structural or power difference framework as opposed to an individual responsibility-failure framework. For previous related work which emphasizes the importance of learning structural blame, see Portes (1971a; 1971b) and Portes and Ross (1974).

[19] In this procedure, quotas are established, based on the probabilities of particular kinds of persons not

Several aspects of the political history of the site of data collection are important. Gary was founded in 1906 by the United States Steel Corporation and eventually became the largest company town in the United States. Since its founding, Gary has been the site of an extremely diverse ethnic-based political history (see Mohl and Betten, 1972). Gary was one of the first large United States cities to elect a black mayor (in 1967; see Starnes, 1973, for further detail), and also one of the important locations of black protest during the 1960s (see National Advisory Commission's report on civil disorders, 1968). These features of Gary's history should be borne in mind when considering the results and general theoretical implications of our analysis.

Variables

The variables in the analysis correspond generally to the four blocks shown in the full generic model (Figure 1-D). The number of variables, however, is substantially greater than four. Three measures of primary group integration are used, and there are multiple indicators for these constructs, as well as for relative deprivation and political protest orientations.

Exogenous variables.

Five exogenous variables are employed in this analysis; four are background status variables and one is a proxy for proximity to radical protest attitudes and information. These are: (1) *age*—the respondent's age (in years) on his/her most recent birthday; (2) *education*—indicated by a five-point item with attainment categories ranging from less than ninth grade to college graduate; (3) *income*—indicated by total family income before taxes in 1967, coded in $2,000 unit intervals and ranging from less than $2,000 to $16,000 or more that year;[21] (4) *occupational status*—indicated by the Duncan SEI; and (5) areal referent others (ARO)—an ecological proxy for referent others, dominant political protest orientations. Following Portes's (1971a; 1971b) early work, this is an attempt to get at the neighborhood political milieux in which our respondents operated, on the assumption that neighborhood settings represent socialization contexts of potential import. In the absence of data directly reflecting the political orientations of re-

---

being at home when an interviewer calls in order to compensate for these not-at-homes. Quotas were established for the variables—sex, age of males, and female employment status. Comparisons of sample distributions with 1960 and 1970 census distributions indicate that the sample provided disproportionately more blacks than were in Gary in 1960 and less than in 1970, as would be expected by shifts in residential patterns that occurred during this time interval. Other differences between sample and census distributions reflected the proportion of blacks in the sample. The refusal rate was approximately ten percent. All in all, the evidence is that the sample represented Gary reasonably well.

[20] Further, a comparison of the covariance matrix for those with complete information on all variables with the matrix derived from a pairwise estimation procedure was conducted, using an omnibus test for equality of matrices (Specht and Warren, 1976). This comparison was completed separately for each racial category and the matrices were found not to differ significantly. Consequently, we can conclude that the pattern of results would be substantially the same, regardless of which matrix is used. By basing the analysis on only those cases with complete information, we have the advantage that all estimates are based on consistent samples. Missing data are relatively equally scattered across the variables in the analysis. While these observations do not obviate the missing data problem, they do serve to mitigate it. It might be noted, in passing, that any accommodation to missing data has its own difficulties (Kim and Curry, 1977).

[21] This measure of family income, as scaled, is obviously truncated to some extent. We suspect that the restriction in income variance is likely to be more pronounced for whites than blacks. Therefore, income effects on any endogenous variable in the model are likely to be somewhat attenuated, and more so for the white than black results.

spondents' neighborhood contacts, we resorted to this ecological proxy. It was constructed by assigning a binary code to each respondent, dependent on whether she/he resided in a census tract that was below (0) or above (1) the grand mean on the political protest orientation construct.[22]

Relative deprivation. Consistent with our previous discussion of the measurement of relative deprivation, two relative deprivation indicators were constructed, both referring to labor market experiences: (1) *relative income deprivation* (RID) and (2) *relative occupational deprivation* (ROD). Each was constructed using a two-item conditional score to capture both a perceived differential and the degree of negative evaluation concerning that differential. One of the component items asked for the perceived income (occupational) differential using "most people with the same amount of education" as the reference group; and the other component concerned the degree to which the perceived differential bothered the respondent.[23] In the models to be estimated below, the two measures—RID and ROD—are taken as observable indicators of a general labor market experiences *relative deprivation* construct.

Primary group integration. Three unmeasured constructs are used as measures of primary group integration: (1) *friends' integration* (Fr.I.), (2) *family integration* (Fam.I.), and (3) *neighborhood integration* (N.I.). Each construct is indicated by four observable items which have been scaled to equal intervals ranging from zero to one.[24]

Political protest orientation. The endogenous construct of primary interest is political protest orientation (PO). Protest orientation is also treated as an unmeasured variable indicated by six observable variables dealing with various types of collective protest or political insurgent action the respondent feels are appropriate for members of dissatisfied groups to employ. It is best conceived not as a construct of actual protest involvement or of explicit intention to become involved in such action, but a construct of approval of protest and insurgent politics as a legitimate means of attaining political goals.[25]

---

[22] At the time this research was conducted, Gary had twenty-seven census tracts. No sample blocks were drawn in one of them, a tract which contained 327 households. All remaining tracts, with households numbering from 966 to 4,560, were the source of at least two sample clusters of ten respondents each, with the maximum being seventy. Thus, at minimum, the areal referent other measure was based on the responses of twenty persons: 44% of the respondents lived in tracts (N=14) below the mean, 56% in tracts (N=12) above the mean. This measure correlates strongly (above .90) with a measure omitting respondents' own scores. We dichotomized this measure in order to place the individual in a social context and to avoid the assumption of a linear relationship between individuals' attitudes and those of the neighborhood in which they live.

[23] Specifically, the relative deprivation indicators were constructed as follows: Relative income (R.I.)—"Think now of people who have about as much education as you have (your husband has), do you think that most people with that amount of education have—much higher incomes (0), somewhat higher incomes (1), about the same income (2), or somewhat or much lower incomes (3)?" Perceived deprivation (P.D.)—"Does this bother you—not at all (2), somewhat (1), or very much (0)?" If relative income (R.I.) equals 1 or 0 and perceived deprivation (P.D.) equals 0, then relative income deprivation (R.I.D.) equals 3. If R.I. equals 1 or 0 and P.D. equals 1, then R.I.D. equals 2. If R.I. equals 1 or 0 and P.D. equals 2, then R.I.D. equals 1. If R.I. equals 2 or 3, then R.I.D. equals 0. The relative occupational deprivation indicator was constructed in precisely the same manner, with the item content referring to the job instead of income.

[24] The indicators for the primary group integration constructs are: (A) *Integration with friends* (Fr. I.)—(1) When I am with friends I can just be myself, (2) When I talk to my friends I get nervous (reverse coded), (3) My friends don't appreciate the things I do for them (reverse coded), (4) I can count on my friends (all items, 0=never, .33=seldom, .67=often, 1=always). (B) *Family integration* (Fam. I.)—(1) My family makes me nervous (reverse coded), (2) I can act the way I want when I am with my family, (3) All things considered, I get a lot of satisfaction from my family, (4) For any problem I can count on my family for help (all items, 0=never, .33=seldom, .67=often, 1=always). (C) *Neighborhood integration* (N.I.)—(1) Do your neighbors ask you for information or discuss things with you? (2) Whenever people in the neighborhood get together to get something done, do you take an active part? (both items, 0=never, .33=seldom, .67=often, 1=always). (3) People have different ideas of how they themselves fit into their neighborhood. How would you say you fit into this neighborhood? Are you a person who: is not really a part of this neighborhood at all (0); is just an ordinary neighborhood member (.33); is active but not one of the decision makers (.67); contributes to neighborhood decisions (1)? (4) On the average how many times per week do you talk with any of your neighbors? Don't talk with any (0); less than once a week (.2); once or twice a week (.4); three-four times a week (.6); five-six times a week (.8); every day or more (1.0)?

Although direct measures of participation in protest activities as well would be highly desirable, a focus on the formation of political orientations as conceptualized is important for several reasons. First, while the surmise is untested here, it is likely that such orientations dispose individuals to participate or not participate, at least to some degree. Second, as suggested earlier, such orientations are important in their own right to the extent that they reflect potential for "third party" support in the form of "public opinion." Third, as also suggested earlier, such "radical" orientations are embedded (to varying degrees) in the major perspectives which we are proposing to test in this analysis. Finally, these general orientations toward protest and insurgent politics bear on a more general question assumed in most treatments of pluralist political theory: the degree of political procedural consensus (in this case, "dissensus"). The process by which persons (and subgroups) form political orientations antithetical to a fundamental premise of a major view of United States politics is certainly worthy of attention.

The individual items in the PO measure refer to the appropriateness of employing the following forms of social protest: (a) "sit-in" and "walk-outs," (b) mass demonstrations, (c) civil disobedience, (d) use of violence to attain goals for minorities in general, (e) use of violent versus nonviolent politics for blacks in particular, and (f) the extent to which civil disobedience is a method to disrupt and destroy our society (reverse coded). As with the primary group integration measures described previously, all variables have been scaled so that the response range is in equal intervals from zero to one. What is unique about our treatment of protest orientations is that we specify them as indicators of an unmeasured construct, which (a) allows explicit introduction of the measurement structure while not presuming the items to be perfect (no measurement error) measures of the construct of interest, and (b) allows the examination of effects of antecedent variables on particular attitudinal items separately (hence we need not assume that all items are affected homogeneously as with the use of traditional composite indexes).

Model Specification and Estimation

The basic model to be estimated derives from our earlier theoretical explication and parallels the generic model shown in Figure 1-D. Verbally, the system contains five equations in which relative deprivation (RD), the three primary groups' integration constructs (Fr.I., Fam. I., N.I.) are all taken to be functions of the five exogenous variables (AGE, ED, INC, OCC, and ARO), but not causally related to each other (however, we do allow these four intervening constructs to be related through their error structure);[26] and the protest orientation (PO) construct is also taken to be dependent on all five exogenous variables plus the four intervening constructs. Symbolically, the system is as follows:

$$\text{RD} = \gamma_{11}\,\text{AGE} + \gamma_{12}\,\text{ED} + \gamma_{13}\,\text{INC} + \gamma_{14}\,\text{OCC} + \gamma_{15}\,\text{ARO} + \zeta_1 \qquad (1)$$

$$\text{Fr.I.} = \gamma_{21}\,\text{AGE} + \ldots + \ldots + \ldots + \gamma_{25}\,\text{ARO} + \zeta_2 \qquad (2)$$

$$\text{Fam.I.} = \gamma_{31}\,\text{AGE} + \ldots + \ldots + \ldots + \gamma_{35}\,\text{ARO} + \zeta_3 \qquad (3)$$

$$\text{N.I.} = \gamma_{41}\,\text{AGE} + \ldots + \ldots + \ldots + \gamma_{45}\,\text{ARO} + \gamma_4 \qquad (4)$$

---

[25] Others have employed similar attitudinal measures but usually in a composite scale. Probably most similar to our construct is the Potential for Political Violence (PPV) scale developed by Muller (1972) and also employed by Grofman and Muller (1973). Portes's (1971a; 1971b) scale is, in content, much more a measure of left-wing political orientations than our measure. See Fendrich (1977) for a different, but similar, left-wing political attitude scale.

[26] It is worth mentioning at this point that a number of alternative specifications were examined (in particular, we allowed recursive causal relations between the intervening constructs and also estimated the system by fixing the measurement structure a priori. The resulting estimations fail to alter in any meaningful sense the major patterns and conclusions observed below.

$$PO = \gamma_{51}\ AGE + \ldots + \ldots + \ldots + \gamma_{55}\ ARO + B_{51}\ RD + B_{52}\ Fr.I. + B_{53}\ Fam.I. + B_{54}\ NI + \zeta_5. \quad (5)$$

Each of the constructs on the left-hand side of equations (1) through (5) is indicated by more than one observable variable, and thus implies a set of auxiliary or measurement equations embedded within the overall structural system. The parameters associated with the measurement structure are also estimated in the context of the overall model.[27]

Parameter estimates for the system of equations were obtained through the use of a full-information maximum likelihood program (Jöreskog and Sörbom, 1976). This program is well-suited for present purposes since it allows a simultaneous estimation of all model parameters, including the measurement structure and the correlated disturbances in the equations, as well as correlated measurement errors. The program also provides a test statistic for goodness-of-fit which, under the assumption of multivariate normality, is distributed as χ^2 with degrees of freedom equal to the number of overidentifying restrictions. Furthermore, this program provides information which may be useful in respecifying the estimated model.[28]

RESULTS

The analysis proceeds by estimating the model represented in Equations (1) through (5) separately for whites and blacks. Our expectation that the processes of interest would differ by race was initially borne out in a test of the heterogeneity between race-specific covariance matrices. Using the Box M test (see Specht and Warren, 1976), we obtained $\chi^2 = 584.024$, with 325 degrees of freedom and $Z = 7.63$, which led us to reject the null hypothesis of cross-race homogeneity of covariance structures. Tests for differences by sex produced no significant main or interaction effects.

The means, standard deviations and zero-order correlations for all variables specific to race are presented in the Appendix. We do not wish to dwell on these simple relationships. However, it is worth noting that the two observable relative deprivation measures (y_1 and y_2) are weakly, or at best modestly, correlated with the six observable protest measures (y_{15} to y_{20}). This is consistent with previous findings on such items (cf., e.g., McPhail, 1971; Long, 1977). When considering general construct effects (in effect incorporating measurement error, shown later), the findings become substantially more interesting.

White Sample

Table 1 presents the standardized and metric slopes as well as standard errors for the final model based on the white

---

[27] The measurement structure for the five unmeasured endogenous constructs is given by:

$$\begin{aligned}
y_1 &= \lambda_{11}\ RD + E_1\\
y_2 &= \lambda_{21}\ RD + E_2\\
\\
y_3 &= \lambda_{32}\ Fr.I + E_3\\
&\vdots\\
y_6 &= \lambda_{62}\ Fr.I + E_6\\
\\
y_7 &= \lambda_{73}\ Fam.I + E_7\\
&\vdots\\
y_{10} &= \lambda_{10.3}\ Fam.I. + E_{10}\\
\\
y_{11} &= \lambda_{11.4}\ N.I. + E_{11}\\
&\vdots\\
y_{14} &= \lambda_{14.4}\ N.I. + E_{14}\\
\\
y_{15} &= \lambda_{15.5}\ PO + E_{15}\\
&\vdots\\
y_{20} &= \lambda_{20.5}\ PO + E_{20}
\end{aligned}$$

where: y_1 = observed relative income deprivation; y_2 = observed relative occupational deprivation; y_3 to y_6 = observed measures of integration with friends (see fn. 24 for content and order); y_7 to y_{10} = observed measures of family integration (see fn. 24); y_{11} to y_{14} = observed measures of neighborhood integration (see fn. 24); and y_{15} to y_{20} = observed measures of protest orientation (see text).

[28] The program provides the first-order partial derivatives of the fitting function with respect to each of the parameters in the model, whether free or fixed. Examination of these derivative matrices for the element of largest absolute value may lead to useful respecification information if the parameter is identified and it makes theoretical sense to estimate it (see Sörbom, 1975, for a full discussion of the procedure). We do not, however, allow this information to dictate our theoretical argument. The information is employed only as an aid in determining when disturbances and measurement errors should be estimated, rather than assumed to be zero.

Table 1. Standardized, Unstandardized Parameter Estimates and Standard Errors for Political Protest Orientation Model: White Sample

Endogenous Variables	Predetermined Variables								
	X_1 Age	X_2 Ed	X_3 Inc	X_4 Occ	X_6 ARO	η_1 RD	η_2 Fr. I.	η_3 Fam. I.	η_4 Neigh. I.
Relative Deprivation (η_1)	−.147**[a]	.112*	−.213**	.142**	.032	N	—	—	—
	−.011[b]	.101	−.115	.007	.116				
	.005[c]	.070	.037	.003	.223				
Friends' Integration (η_2)	.121*	.202**	.040	−.044	−.129*	—	N	—	—
	.009	.179	.022	−.002	−.457				
	.006	.091	.047	.004	.289				
Family Integration (η_3)	.211**	.187**	.129**	.004	−.034	—	—	N	—
	.016	.169	.070	.000	−.122				
	.006	.080	.042	.004	.252				
Neighborhood Integration (η_4)	.099*	.066	.125**	.035	−.022	—	—	—	N
	.007	.058	.066	.002	−.077				
	.005	.070	.037	.003	.223				
Protest Orientation (η_5)	−.194**	.130	−.076	.063	.134*	.042	−.251*	.085	−.155**
	−.016	.124	−.044	.003	.508	.044	−.270	.090	−.169
	.007	.098	.052	.005	.312	.089	.196	.168	.098

$X^2 = 290.047$, df = 239, p = .01.

[a] Standardized coefficient.
[b] Unstandardized coefficient.
[c] Standard Error of b.
* Significant at $.05 \leq p \leq .10$.
** Significant at $p \leq .05$, one-tailed test.
– Zero restriction.
N Normalization rule imposed.

sample. In arriving at these results, we first estimated a version of the model which assumed all disturbance (in equations) and measurement error covariances to be zero. We then proceeded incrementally to relax these assumptions, where warranted, based on the partial derivatives of the fitting function with respect to the particular constrained parameters (see Sörbom, 1975). We especially expected correlated disturbances for the four intervening constructs since we specified no causal relationship among them. The process continued until freeing-up such covariances produced nonsignificant parameter estimates. (These estimated covariances are shown at the bottom of Figure 2.) This final model resulted in a very reasonable fit of the model to the data ($\chi^2 = 290.047$, df = 239, p = .01).[29]

We are predominantly concerned with the relative magnitude of coefficients within racial models, since the metrics of most of our variables have no inherent meaning.[30] Because of this, and also for convenience, the significant standardized effect parameters are presented in Figure 2.

Focusing first on the effects of the socioeconomic variables (education, income, and occupation) on protest orientation, we find support neither for the mass society "primitivism" hypothesis, nor for the relative deprivation thesis, since none of these coefficients departs significantly from zero. Contrary to the "primitivism" thesis, which predicts a negative education effect, a number of studies (e.g., Portes, 1971a) have reported relatively strong positive education effects on "leftist radicalism" and on other measures similar to ours.

On the other hand, there are several significant direct effects on the protest orientation construct. (All indirect effects are trivial.) Age, integration with friends and into the neighborhood all show moderately large, significant negative effects. The negative age effect is consistent with at least some findings on aging and political orientations (cf., Hendricks and Hendricks, 1977) and recent results concerning party preferences over the past two decades in the United States (see Knoke and Hout, 1974).[31] The negative effects of two of the three integration constructs (i.e., friends and neighborhood) are consistent with the mass society primary group isolation arguments. A more detailed and comparative alternative explanation is postponed until the concluding section of this article.

The only variable in the model which produces more "radical" protest orientations is the referent other proxy. Those residing in areas (census tracts) which are, in general, more "radical" also tend to be more radical in their acceptance or approval of noninstitutionalized political action, a result consistent with the differential socialization flow of information imagery. The primary group integration dimensions, however, play the major role in explaining protest orientations among whites; and they relate negatively (i.e., the higher the integration, the lower the protest orientations).

Black Sample

Table 2 shows the standardized and unstandardized parameter estimates as well

---

[29] The fit per degree of freedom (i.e., χ^2/df) is probably a better indication of overall fit (see Wheaton et al., 1977). The χ^2/df for this model is 1.2—substantially below the ratio of 5 suggested by Wheaton et al. as minimally reasonable.

[30] This is especially the case with the unmeasured variables in the model. In order to provide a metric for such variables some restriction must be made. We chose to follow the Jöreskog and Goldberger (1975) suggestion of fixing the error variance of each unmeasured construct in the model at 1.0. Other decisions which fix the metric of such unobserved variables are, of course, possible. For example, we also treated all primary group integration and the protest orientation constructs as composite scales and estimated the models with the epistemic coefficients fixed a priori based on internal consistency reliabilities. These results (not reported here) gave substantive conclusions consistent with those we have reported in the text.

[31] Knoke and Hout's (1974) study presents evidence for such a life-cycle or aging effect on party preferences; that is, net of period, cohort and other important determinants, age produces shifts away from the Democratic party to Republican and Independent alternatives. We cannot be so confident in interpreting the linear age effect in our results since we are unable to remove the potential confounding effects of cohort (see Mason et al., 1973). There are, of course, no period effects since observations were all taken at the same time.

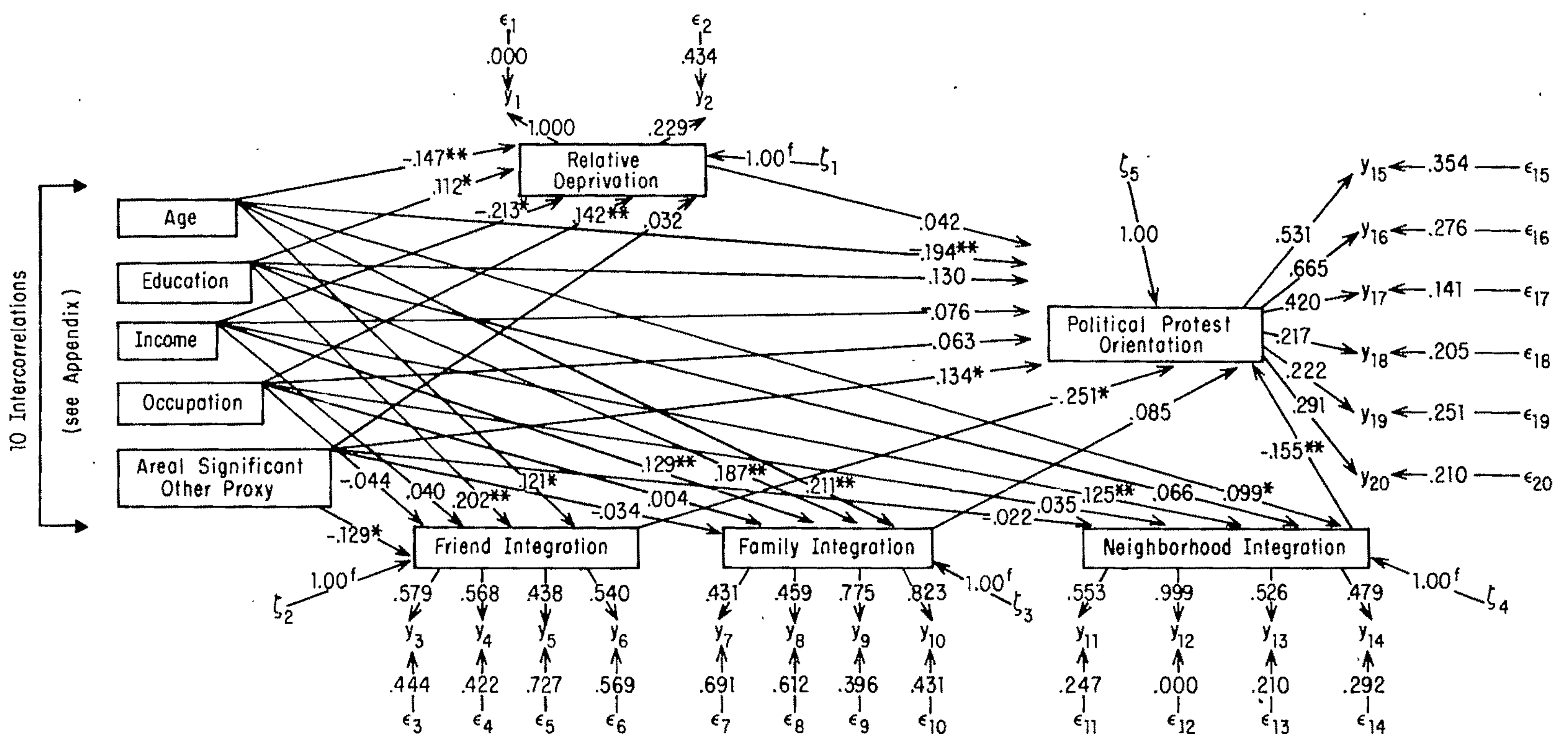

10 Intercorrelations
(see Appendix)
Age
Education
Income
Occupation
Areal Significant Other Proxy
Relative Deprivation
Friend Integration
Family Integration
Neighborhood Integration
Political Protest Orientation
ϵ_1 .000 y_1 1.000
ϵ_2 .434 y_2 .229
1.00f ζ_1
-.147** .112* -.213* .142** .032
.042 -.194** .130 .076 .063 .134*
ζ_5 1.00
.531 y_{15} .354 ϵ_{15}
.665 y_{16} .276 ϵ_{16}
.420 y_{17} .141 ϵ_{17}
.217 y_{18} .205 ϵ_{18}
.222 y_{19} .251 ϵ_{19}
.291 y_{20} .210 ϵ_{20}
-.251* .085 -.155**
-.044 .040 .202** .121* -.034 .004 .129** .187** .211** -.022 .035 .125** .066 .099*
-.129*
1.00f ζ_2
1.00f ζ_3
1.00f ζ_4
.579 y_3 .444 ϵ_3
.568 y_4 .422 ϵ_4
.438 y_5 .727 ϵ_5
.540 y_6 .569 ϵ_6
.431 y_7 .691 ϵ_7
.459 y_8 .612 ϵ_8
.775 y_9 .396 ϵ_9
.823 y_{10} .431 ϵ_{10}
.553 y_{11} .247 ϵ_{11}
.999 y_{12} .000 ϵ_{12}
.526 y_{13} .210 ϵ_{13}
.479 y_{14} .292 ϵ_{14}

FIGURE 2: ESTIMATED STANDARDIZED EFFECT PARAMETERS FOR THE MODEL OF POLITICAL PROTEST ORIENTATION: WHITE SAMPLE (N = 247)

Note: The following measurement errors were found to be significantly correlated: $\rho(\epsilon_2 \epsilon_{18}) = .012$ $\rho(\epsilon_3 \epsilon_8) = .099$ $\rho(\epsilon_3 \epsilon_{16}) = -.045$ $\rho(\epsilon_6 \epsilon_{17}) = -.016$ $\rho(\epsilon_8 \epsilon_{19}) = .019$ $\rho(\epsilon_9 \epsilon_{20}) = -.025$ $\rho(\epsilon_{11} \epsilon_{14}) = .028$ $\rho(\epsilon_{11} \epsilon_{18}) = -.006$ $\rho(\epsilon_{13} \epsilon_{16}) = .012$ $\rho(\epsilon_{17} \epsilon_{20}) = .005$ $\rho(\epsilon_{18} \epsilon_{20}) = .008$

The following errors in equations were found to be significantly correlated: Family Integration and Friend Integration = .602 Family Integration and Neighborhood Integration = .141 Friend Integration and Neighborhood Integration = .305

** Coefficients significant at $p \leq .05$; * $.05 \leq p \leq .10$

f Fixed coefficient

Table 2. Standardized, Unstandardized Parameter Estimates and Standard Errors for Political Protest Orientation Model: Black Sample

Endogenous Variables	Predetermined Variables X_1 Age	 X_2 Ed	 X_3 Inc	 X_4 Occ	 X_5 ARO	 η_1 RD	 η_2 Fr. I.	 η_3 Fam. I.	η_4 Neigh. I.
Relative Deprivation (η_1)	−.022[a]	.062	−.230**	−.120*	.030	N	—	—	—
	−.022[b]	.059	−.115	−.008	.091				
	.007[c]	.096	.045	.006	.246				
Friends' Integration (η_2)	−.090	.007	.034	−.008	−.116	—	N	—	—
	−.007	.006	.017	−.001	−.341				
	.008	.108	.047	.006	.279				
Family Integration (η_3)	.061	−.016	.029	−.077	−.106*	—	—	N	—
	.005	−.015	.014	−.005	−.310				
	.007	.094	.041	.006	.242				
Neighborhood Integration (η_4)	.164**	.145**	.231**	−.066	−.029	—	—	—	N
	.013	.139	.117	−.004	−.087				
	.006	.078	.037	.005	.194				
Protest Orientation (η_6)	−.051	.263**	−.083	.039	.141*	.206**	.249*	−.195*	.030
	−.004	.268	−.044	.003	.456	.221	.274	−.214	.032
	.008	.113	.051	.007	.290	.130	.172	.150	.093

$X^2 = 270.322$, df = 238, p = .07

[a] Standardized coefficient.
[b] Unstandardized coefficient.
[c] Standard Error of b.
* Significant at $.05 \leq p \leq .10$.
** Significant at $p \leq .05$, one-tailed test.
− Zero restriction.
N Normalization rule imposed.

as standard errors for the same model (with the exception of the error structure) based on the black sample. The fit of the model to the data ($\chi^2 = 270.322$, df = 238, p = .07), is quite reasonable.[32]

At this point, we are again most concerned with the within-sample relative magnitude of effects and therefore focus on the standardized estimates presented in Figure 3. Looking first at socioeconomic background variables, education has a sizable significant direct effect on protest orientations, contrary to our results for the white sample. As with the white results, however, this finding is contrary to the mass society "primitivism" hypothesis and is even stronger negative evidence than provided by the data for whites, since the effect is in the opposite direction to that predicted by the hypothesis. This positive direct education effect is also consistent with the findings of other studies from black samples (cf., Marx, 1967; Caplan and Paige, 1968; McCord and Howard, 1968; Tomlinson, 1970. See especially Caplan, 1970, for a more thorough review of empirical generalizations for blacks in the United States). And it is consistent with Portes's (1971a) findings for lower-class urban Chileans. Again, income and occupation play no direct role in influencing protest orientation.

A major departure from the pattern of results for whites is contained in the findings with respect to the primary group integration constructs. For the white sample, friends' integration and neighborhood integration had a "conservatizing" (negative) impact on protest orientation. For the black sample, family integration has a "conservatizing" influence, while integration with friends has a larger "radicalizing" influence. The difference between the direction of friends' and family influence is consistent with some findings on major institutional influence differences in the political socialization of lower-class persons—particularly the ambiguity surrounding the influence of the family (see Mann, 1970).

Again contrary to the results for whites is the substantial positive relative deprivation effect.[33] Blacks who perceive economic differentials between themselves and most others with equivalent education and who are bothered by these perceived differentials are significantly more inclined to positive views toward political protest. There is also, for blacks, some minimal support for the absolute deprivation argument in that income and occupation show negative indirect effects on protest orientation through relative deprivation.

Along with the positive effects already described, there is a strong positive effect of the referent others proxy, suggesting that the protest orientations of individuals are importantly affected by the political orientations of others living in close proximity to them.

These results, particularly the support for the relative deprivation hypothesis in the black sample, are unique in that no other study (to our knowledge) has produced such strong race-specific support for this hypothesis, net of other influences, and at the individual level. Overall, the pattern of race-specific effects suggests quite different pictures of the process by which political orientations develop in black and white populations. For whites, it is the young, those not integrated into friendship and neighborhood structures,

---

[32] The fit per degree of freedom, here, is 1.1, roughly the same as that reported for the white sample, in spite of the differences in probability levels associated with the two χ^2 tests (See fn. 29).

[33] As can be seen in comparing the epistemic coefficients for relative deprivation in the white (Figure 2) and black (Figure 3) models, the relative magnitude of the deprivation validity coefficients are not consistent for the two groups. For whites, the relative deprivation construct is predominantly given its meaning by the relative income deprivation variable ($\lambda = 1.000$), while for blacks, the relative occupational deprivation measure has the largest of the two validity coefficients ($\lambda = .828$). It is also the case that the meaning of an unmeasured variable can be a function of antecedent variables affecting it as well as its indicators (see Burt, 1976). Because we were concerned with the relative validities of the relative deprivation indicators and the fact that the relative deprivation construct was a function of different exogenous influences across samples, we estimated the model for blacks and whites, assuming that the relative deprivation indicators were perfectly measured. The results did not alter our substantive conclusions; for whites neither deprivation variable showed a significant effect on protest orientation and for blacks only relative occupational deprivation had a significant effect on protest orientation.

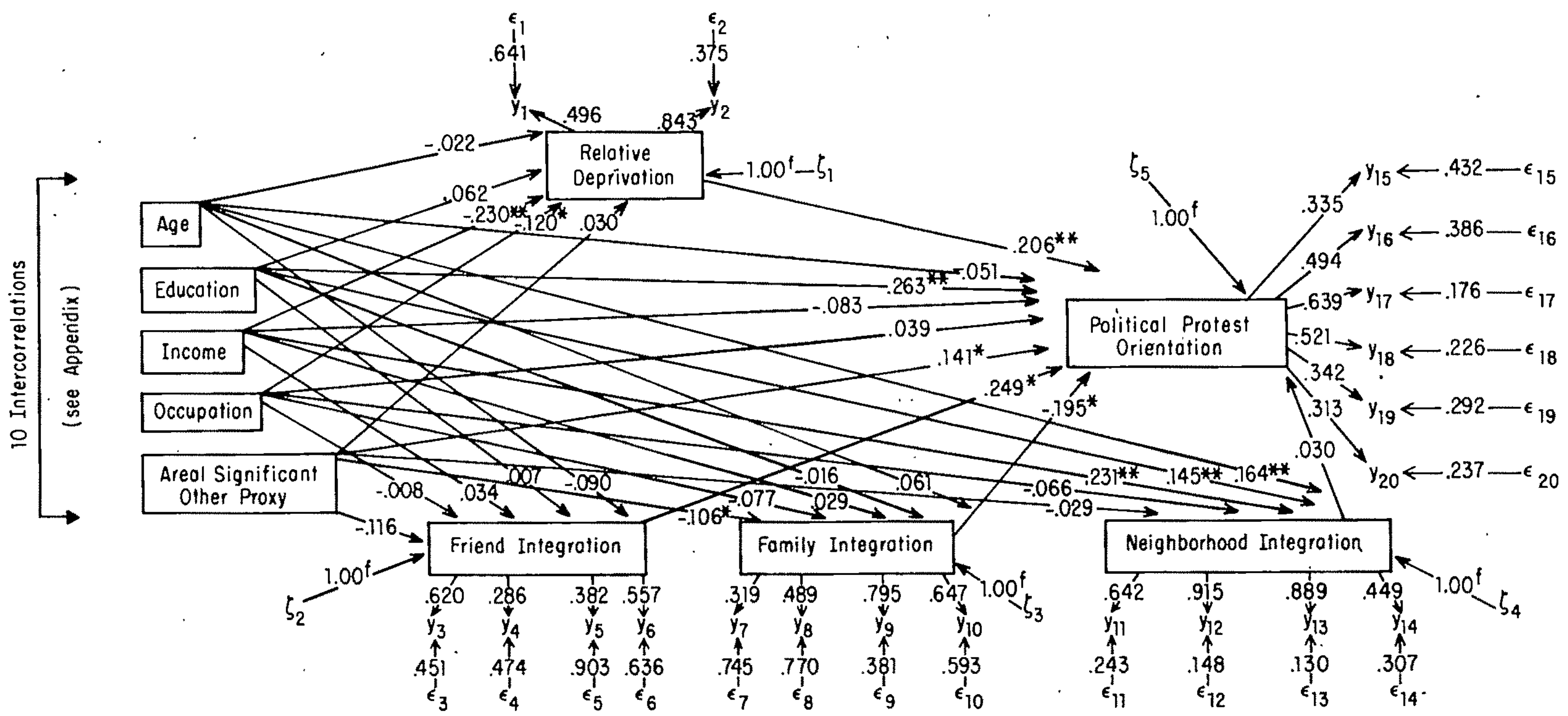

10 Intercorrelations
(see Appendix)
Age
Education
Income
Occupation
Areal Significant Other Proxy
Relative Deprivation
Political Protest Orientation
Friend Integration
Family Integration
Neighborhood Integration
ϵ_1 .641 y_1 .496
ϵ_2 .375 y_2 .843
1.00^f ζ_1
-.022
.062
-.230**
-.120*
.030
.206**
.263**
.051
-.083
.039
.141*
.249*
-.195*
.030
-.008
.034
.007
-.090
-.116
-.106*
-.077
-.016
.029
.061
-.066
-.029
.231**
.145**
.164**
ζ_5 1.00^f
.335 y_{15} .432 ϵ_{15}
.494 y_{16} .386 ϵ_{16}
.639 y_{17} .176 ϵ_{17}
.521 y_{18} .226 ϵ_{18}
.342 y_{19} .292 ϵ_{19}
.313 y_{20} .237 ϵ_{20}
ζ_2 1.00^f
.620 y_3 .451 ϵ_3
.286 y_4 .474 ϵ_4
.382 y_5 .903 ϵ_5
.557 y_6 .636 ϵ_6
ζ_3 1.00^f
.319 y_7 .745 ϵ_7
.489 y_8 .770 ϵ_8
.795 y_9 .381 ϵ_9
.647 y_{10} .593 ϵ_{10}
ζ_4 1.00^f
.642 y_{11} .243 ϵ_{11}
.915 y_{12} .148 ϵ_{12}
.889 y_{13} .130 ϵ_{13}
.449 y_{14} .307 ϵ_{14}

FIGURE 3: ESTIMATED STANDARDIZED EFFECT PARAMETERS FOR THE MODEL OF POLITICAL PROTEST ORIENTATION: BLACK SAMPLE (N = 204)

Note: The following measurment errors were found to be significantly correlated: $\rho(\epsilon_3\epsilon_8) = .115$ $\rho(\epsilon_5\epsilon_8) = .043$ $\rho(\epsilon_{11}\epsilon_{13}) = -.030$ $\rho(\epsilon_{11}\epsilon_{14}) = .031$ $\rho(\epsilon_{15}\epsilon_{16}) = .057$

The following errors in equations were found to be significantly correlated: Relative Deprivation and Family Integration = -.199
Relative Deprivation and Friend Integration = -.267 Family Integration and Friend Integration = .483
Family Integration and Neighborhood Integration = .270

**Coefficients significant at $p \leq .05$; * $.05 \leq p \leq 10$

^{f} Fixed coefficient

and those who live in parts of the city where close contacts are likely to hold similar favorable attitudes toward protest, who are most prone to view political protest and violence favorably. For blacks, it is those who harbor feelings of economic-induced relative deprivation, who have higher education, who are integrated into friendship structures but not integrated into the family, and who also live in parts of the city where close contacts are likely to hold similar favorable attitudes toward protest, who are most prone to view political protest and violence favorably. While the race-specific patterns show interesting differences, the one strong common determinant of protest orientation is the proximity of others who hold similar views.

DISCUSSION

There has been very little empirical analysis directed at the primary group isolation hypothesis of mass society theory. The findings of this study support Pinard's (1971) conclusion that the impact of primary group isolation (integration) is likely to depend on the social context and political orientation of particular primary groups. That conclusion, in turn, reinforces our sense that the proper interpretation of our findings with regard to the primary group integration variables is not that of mass society theory. Rather than the interpretation suggested by the mass society perspective (that isolation breeds a reactive formation), it seems much more reasonable to view the observed effects on protest orientation in differential political socialization terms: Those whites who have roots in a geographical area in the form of ties to family, friends and neighborhood networks with a stake in political stability learn a conservative orientation toward political protest; those blacks who have ties to other blacks who see the interests of blacks as requiring political changes through unconventional political means learn a more radical orientation. We will return to these conclusions.

With regard to another aspect of the mass society thesis, our data strongly suggest that "primitivism" (due to lack of education, information and appreciation for democratic principles) is not at the roots of individual leftist political orientations or mass movements in general. Our findings at this point add to an increasingly large body of negative evidence now accumulated. There is an important duality suggested in these results: For some groups educational attainment may lead to increased institutionalized political participation (e.g., voting); for other, perhaps more powerless, groups it may lead to noninstitutionalized political orientations and participation (e.g., protest).

As earlier noted, there has been little by way of empirical support for the relative deprivation hypothesis (see Caplan, 1970; McPhail, 1971 and Useem, 1975, for comprehensive reviews) when tested on individual level data. The reasonably strong net effect of relative deprivation on protest orientation for our black sample runs contrary to this evidence. It is possible that this finding is attributable to (1) our better measurement of the concept, and/or (2) the fact that we allow for measurement error in the estimation of the structural model. Either or both of these would better allow a "real" effect to appear. Only further research, however, will show whether this finding will replicate. In any event, this supportive evidence for relative deprivation must be tempered. Relative deprivation may indeed be a radicalizing experience for certain subpopulations in different times and places, but the importance of resource mobilization (Snyder and Tilly, 1972; Tilly, 1978; McCarthy and Zald, 1977) and the contingencies of the assembling process (McPhail and Miller, 1973) in the determination of actual collective protest and violence certainly should not be minimized.

Our finding of race-specific patterns can be best understood, we think, within a differential socialization framework (Portes, 1971a; 1971b). Both the white and black results for the primary group integration variables, and the black results for the relative deprivation variable make sense from this perspective. The negative signs on the coefficients relating the integration measures to the protest orientations measure suggest a "disidentification" (alienation) process for whites; the positive sign on the coefficients relating friends' integration to protest orientation for blacks suggests quite the opposite. One way of reading these results is to say that for whites, being "top-dog" in the racial system of the society, integration into the system through friends and neighborhood leads to a protective disinclination to endorse violence, to avoid political means which threaten the system. For blacks, being underdogs in the racial system, that integration leads to identification with the interests of blacks vis-à-vis whites and to an endorsement of violence as a means of shaking up the system.

This interpretation gains substance when one remembers the time of the data collection (January, 1969), a time when the country was extremely sensitized to the issues of racial conflict and violence. Furthermore, the site of the data collection (Gary, Indiana) had at this time a social environment in which race was a central politicized issue, given the political moves and eventual success of Richard Hatcher, its black mayor (see Starnes, 1973, for more detail on the political climate of Gary at this time).

The finding involving relative deprivation for the black sample enhances this socialization interpretation. Indeed, a socialization process is implicit in the very construction of our two relative deprivation indicators. As we argued earlier, the concept of relative deprivation is properly measured when it includes not only a perceived inequity but also a negative evaluative component. In other words, the inequity must be learned to be associated with structural sources rather than accounted for in terms of personal blame. Inequities are ubiquitous in time and space, but not all inequities create relative deprivation. In order for relative deprivation to occur (at least in its reference group form) individuals and groups must learn to look for the source of the inequity outside themselves—in a sense, they must serve a "period of intellectual apprenticeship. . . ." (Portes, 1971a:832)—which creates the feeling of anger or indignation surrounding the perceived inequity.[34] Inner city life in the late 1960s—the height of the black consciousness movement—provided such a learning experience for many blacks.

Our results derive from a cross-section of the black and white adult populations of a midwestern city of 1969. For some effects (e.g., education) there is a substantial amount of consistent evidence. However, for hypotheses concerning complex constructs such as relative deprivation, the consistency in the evidence is difficult to ascertain since measures employed and models estimated vary widely. In this regard, our results contradict, in several ways, aspects of two traditional perspectives and other empirical research. We hope others will attempt to refute these findings by examining similar or related questions for different temporal and spatial locations.

Finally, these race-specific findings suggest that the reasons for the development of unconventional political orientations differ according to structural location. Although there does not appear to be enormous debate over aggregate differences in protest orientations between blacks and whites (see, e.g., Eisinger, 1974), the process through which such orientations are formed is not well known. More generally, these results bear on one of the central components in the pluralist model of American politics, the notion of *procedural consensus* (cf. Mann, 1970; Gamson, 1975:chap. 1). Where procedural consensus does not exist (i.e., where established political procedure is disavowed), such a disavowal appears to be formed in different ways for different groups, and in ways which are theoretically comprehensible.

---

[34] It is quite possible that the relative deprivation effect observed in our analysis understates other more powerful aspects of the concept. Ours is an individualistic comparison measure and does not tap the total group dimension of relative deprivation—for example, blacks as compared to whites in general. Specifically, we have in mind the distinction between egoistic and fraternalistic forms of the concept (see Runciman, 1966; Vanneman and Pettigrew, 1972).

APPENDIX

Correlations, Means and Standard Deviations for all Variables in the Model: White and Black Samples

Variables	1. Age	2. Educ	3. Inc	4. Occ	5. Aro	6. y_1	7. y_2	8. y_3	9. y_4	10. y_5	11. y_6	12. y_7	13. y_8	14. y_9	15. y_{10}	16. y_{11}	17. y_{12}	18. y_{13}	19. y_{14}	20. y_{15}	21. y_{16}	22. y_{17}	23. y_{18}	24. y_{19}	25. y_{20}
1.		−27	−19	−05	12	−14	−07	05	05	00	−01	09	01	18	04	05	05	07	13	−24	−11	−01	−00	−04	−02
2.	−44		41	51	−05	14	−05	06	04	09	15	03	13	12	14	10	11	12	01	04	13	21	−22	−04	−09
3.	−09	27		24	−16	−11	−08	05	−01	13	11	−00	12	13	13	11	15	11	−02	−08	04	04	−13	−00	−11
4.	−16	44	19		03	16	−11	−03	−07	06	17	−03	03	09	11	13	09	05	02	00	14	06	−12	−01	−02
5.	17	−17	−09	−01		05	00	−04	−06	−08	−12	−04	−14	−01	−02	06	−03	−15	00	05	05	12	02	13	12
6.	−11	04	−18	−04	09		23	−08	02	01	04	04	−07	−16	−06	−04	−08	−06	−06	07	07	01	06	01	02
7.	02	−05	−18	−11	02	41		−11	04	04	−04	11	−04	−12	−09	−00	−07	−04	03	−02	04	04	12	02	−09
8.	−03	−01	−06	−05	−06	06	−06		38	27	33	27	31	30	26	−03	11	03	04	01	−12	03	−02	03	−04
9.	−21	14	05	09	−11	−11	−23	17		20	26	46	32	27	28	07	20	06	14	−10	−22	−07	−10	12	−12
10.	−16	17	09	06	−10	−06	−07	28	18		12	06	17	21	12	14	17	18	07	04	05	11	−02	02	−15
11.	00	01	08	02	−05	−06	−23	36	05	19		16	17	19	33	18	26	11	13	−09	−20	05	02	−04	−06
12.	−07	11	−08	−01	−07	−07	−13	13	35	16	08		25	27	38	06	15	08	12	−07	−27	−11	−11	04	−18
13.	02	−01	−00	−14	−08	07	−07	24	−03	01	20	17		33	34	08	10	−01	08	−00	−00	02	−10	06	−11
14.	06	−06	02	−08	−07	−09	−13	23	23	−01	21	26	38		64	07	16	11	08	−09	−00	04	−05	−03	−07
15.	−04	01	05	05	−06	−13	−17	18	16	09	27	17	29	53		06	13	03	11	−08	−10	−11	−12	−04	−05
16.	−02	09	18	03	−05	−07	−05	17	02	−08	12	06	04	14	22		56	25	56	−03	−09	−02	−18	−06	−11
17.	13	06	22	04	−02	−10	−08	09	03	−02	11	07	02	21	25	59		52	48	−06	−17	−12	−13	−08	−12
18.	06	12	21	−03	−06	−09	−02	06	04	09	01	13	03	25	19	24	45		31	−04	03	−02	−11	−10	−12
19.	13	−12	08	−10	−06	−14	−00	11	04	−14	06	15	05	10	17	58	42	22		−09	−09	−08	−11	−08	−17
20.	−25	14	−00	−01	−02	15	11	01	−03	08	03	−02	00	−09	01	−07	−06	03	−07		37	21	11	07	14
21.	−11	13	−03	01	13	06	03	04	−01	10	−03	06	−02	−09	−04	−05	−02	−00	−06	44		30	12	11	18
22.	−10	22	−01	09	02	03	17	−01	03	03	−01	04	−00	−18	−08	−03	05	−01	−03	17	32		17	15	24
23.	−15	15	−00	13	00	06	02	07	04	04	−04	08	−01	−06	07	05	−03	−07	−08	18	30	30		20	24
24.	01	02	−07	06	14	02	01	00	12	−02	05	−03	−03	−03	01	−11	01	−09	−13	09	10	21	27		08
25.	03	05	−02	05	−02	06	03	20	−02	01	11	07	07	09	−11	04	09	11	06	16	12	24	15	07	
Whites																									
Means	44.65	1.71	5.32	38.86	.09	.19	.17	2.64	2.71	2.41	2.51	2.37	2.61	2.67	2.60	.39	.43	.41	.54	.28	.20	.03	.22	.20	.35
S.D.	13.76	1.16	1.92	21.79	.29	.52	.45	.55	.52	.78	.67	.77	.69	.63	.73	.30	.36	.25	.33	.42	.37	.16	.21	.26	.22
Blacks																									
Means	40.26	1.42	4.44	23.96	.86	.31	.28	2.67	2.79	2.21	2.36	2.30	2.35	2.60	2.52	.44	.52	.47	.57	.59	.39	.06	.34	.34	.49
S.D.	13.28	1.09	2.07	16.05	.35	.74	.70	.58	.50	.98	.77	.78	.88	.62	.78	.32	.37	.29	.34	.46	.44	.23	.26	.31	.25

Whites (N = 247) above the diagonal; Blacks (N = 204) below.

REFERENCES

Baron, Harold M. and Bennett Hymer
1977 "Racial dualism in an urban labor market." Pp. 188–95 in David M. Gordon (ed.), Problems in Political Economy. Lexington, Mass.: D. C. Heath.

Burt, Ronald S.
1976 "Interpretational confounding of unobserved variables in structural equation models." Sociological Methods and Research 5:3–52.

Caplan, Nathan
1970 "The new ghetto man: a review of recent empirical studies." Journal of Social Issues 26:59–73.

Caplan, N. S. and J. M. Paige
1968 "A study of ghetto rioters." Scientific American 219:15–21.

Cohen, Jacob and Patricia Cohen
1975 Applied Multiple Regression. New York: Halsted.

Crawford, T. J. and M. Naditch
1970 "Relative deprivation, powerlessness and militancy: the psychology of social protest." Psychiatry 33:208–23.

Crosby, Faye J.
1976 "A model of egoistic relative deprivation." Psychological Review 83:85–113.

Danigelis, Nicholas L.
1978 "Black political participation in the United States: some recent evidence." American Sociological Review 43:756–71.

Edwards, Richard C., Michael Reich and David Gordon (eds.)
1975 Labor Market Segmentation. Lexington, Mass.: D. C. Heath.

Eisinger, Peter K.
1974 "Racial differences in protest participation." American Political Science Review 68:592–606.

Farley, Reynolds
1977 "Trends in racial inequalities: have the gains of the 1960s disappeared in the 1970s?" American Sociological Review 42:198–208.

Feierabend, Ivo K. and Rosalind L. Feierabend
1966 "Aggressive behaviors within polities, 1948–1962: a cross national study." Journal of Conflict Resolution, X:249–71.

Fendrich, James M.
1977 "Keeping the faith or pursuing the good life: a study of the consequences of participation in the civil rights movement." American Sociological Review 42:144–57.

Flacks, R.
1967 "The liberated generation: an explanation of the roots of student protest." Journal of Social Issues 23:52–75.

Franklin, Raymond S. and Solomon Resnik
1973 The Political Economy of Racism. New York: Holt, Rinehart and Winston.

Fromm, Erich
1963 Escape from Freedom. New York: Holt, Rinehart and Winston.

Gamson, William A.
1968 Power and Discontent. Homewood, Ill.: Dorsey.
1975 The Strategy of Social Protest. Homewood, Ill.: Dorsey.

Gerlach, Luther P. and Virginia H. Hine
1970 People, Power, Change: Movements of Social Transformation. New York: Bobbs-Merrill.

Geschwender, Barbara N. and James A. Geschwender
1973 "Relative deprivation and participation in the civil rights movement." Social Science Quarterly 54:403–11.

Grofman, Bernard N. and Edward N. Muller
1973 "The strange case of relative gratification and potential for political violence: the V-curve hypothesis." American Political Science Review 67:514–39.

Gurr, Ted Robert
1970 Why Men Rebel. Princeton: Princeton University Press.

Gusfield, Joseph R.
1962 "Mass society and extremist politics." American Sociological Review 27:19–30.

Hendricks, Jon and C. Davis Hendricks
1977 Aging in Mass Society. Cambridge, Mass.: Winthrop.

Hoffer, E.
1966 The True Believer. New York: Harper and Row.

Isaac, Larry and William R. Kelly
1979 "Racial violence and welfare expansion: a test of the Piven and Cloward thesis." Paper presented at 74th annual American Sociological Association Meeting, August, Boston.

Jhabvala, Firdaus
1977 "The economic situation of black people." Pp. 153–60 in D. M. Gordon (ed.), Problems in Political Economy. Lexington, Mass.: D. C. Heath.

Jöreskog, Karl G. and Arthur S. Goldberger
1975 "Estimation of a model with multiple indicators and multiple causes of a single latent variable." Journal of the American Statistical Association 70:631–9.

Jöreskog, Karl G. and Dag Sörbom
1976 LISREL III: Estimation of Linear Structural Equation Systems by Maximum Likelihood Methods: A Computer Program Manual. Chicago: National Educational Resources.

Kerr, Clark and Abraham Siegel
1954 "The interindustry propensity to strike." Pp. 189–212 in Arthur Kornhauser, Robert Dubin and Arthur Ross (eds.), Industrial Conflict. New York: McGraw-Hill.

Kim, Jae-On and James Curry
1977 "The treatment of missing data in multivariate analysis." Sociological Methods and Research 6:215–40.

Knoke, David and Michael Hout
1974 "Social and demographic factors in American political party affiliation, 1952–72." American Sociological Review 39:700–13.

Knoke, David and Larry Isaac
1976 "Quality of higher education and sociopolitical attitudes." Social Forces 54:524–9.

Kornhauser, William
1959 *The Politics of Mass Society*. New York: Free Press.

Korpi, Walter
1974 "Conflict, power and relative deprivation." American Political Science Review 68:1569–78.

Lipset, Seymour Martin
1963 Political Man: The Social Bases of Politics. Garden City: Anchor.

Long, Samuel
1977 "Perceived relative deprivation and political orientation: a study of white and black adolescents." Journal of Political and Military Sociology 5:99–115.

Lupsha, Peter A.
1971 "Explanation of political violence: some psychological theories versus indignation." Politics and Society 2:89–104.

Mann, Michael
1970 "The social cohesion of liberal democracy." American Sociological Review 35:423–39.

Marx, Gary T.
1967 Protest and Prejudice: A Study of Belief in the Black Community. New York: Harper and Row.

Marx, Gary T. and James L. Wood
1975 "Strands of theory and research in collective behavior." Pp. 363–428 in A. Inkeles, J. Coleman, and N. Smelser (eds.), Annual Review of Sociology VI. Palo Alto: Annual Reviews.

Mason, Karen, O., William M. Mason, H. H. Winsborough and W. K. Poole
1973 "Some methodological issues in cohort analysis of archival data." American Sociological Review 38:242–58.

McCarthy, John D. and Mayer N. Zald
1977 "Resource mobilization and social movements: a partial theory." American Journal of Sociology 82:1212–41.

McCord, W. and J. Howard
1968 "Negro opinions in three riot cities." American Behavioral Scientist 11:24–7.

McPhail, Clark
1971 "Civil disorder participation: a critical examination of recent research." American Sociological Review 36:1058–73.

McPhail, Clark and David L. Miller
1973 "The assembling process: a theoretical and empirical examination." American Sociological Review 38:721–35.

Miller, Abraham H., Louis H. Bolce and Mark Halligan
1977 "The J-curve theory and the black urban riots: an empirical test of progressive relative deprivation theory." American Political Science Review 71:964–81.

Mohl, Raymond A. and Neil Betten
1972 "Ethnic adjustment in the industrial city: the international institute of Gary, 1919–1940." International Migration Review 6:361–77.

Morrison, Donald E.
1973 "Some notes toward theory on relative deprivation, social movements, and social change." Pp. 153–67 in G. Zaltman (ed.), Processes and Phenomena of Social Change. New York: Wiley.

Muller, Edward N.
1972 "A test of a partial theory of potential for political violence." American Political Science Review 66:925–59.

Nagel, Jack
1974 "Inequality and discontent: a nonlinear hypothesis." World Politics 26:453–72.

National Advisory Commission
1968 Report of the National Advisory Commission on Civil Disorders. New York: E. P. Dutton.

Oberschall, Anthony
1973 Social Conflict and Social Movements. Englewood Cliffs: Prentice-Hall.

Orum, Anthony
1972 Black Students in Protest: A Study of the Origins of the Black Student Movement. Washington, D.C.: Arnold and Caroline Rose Monograph Series, American Sociological Association.
1974 "On participation in political protest movements." Journal of Applied Behavioral Science 10:181–207.
1976 "On the explanation of political socialization and political change." Youth and Society 8:147–74.

Orum, Anthony M. and Amy W. Orum
1968 "The class and status bases of Negro student protest." Social Science Quarterly 49:521–33.

Paige, Jeffery M.
1971 "Political orientation and riot participation." American Sociological Review 36:810–20.

Pinard, Maurice
1971 The Rise of a Third Party: A Study in Crisis Politics. Englewood Cliffs, N.J.: Prentice-Hall.

Piven, Frances Fox and Richard A. Cloward
1971 Regulating the Poor: The Functions of Public Welfare. New York: Vintage.
1977 Poor People's Movements: Why They Succeed, How They Fail. New York: Vintage.

Porter, James N.
1974 "Race, socialization and mobility in educational and early occupational attainment." American Sociological Review 39:303–16.

Portes, Alejandro
1971a "Political primitivism, differential socialization, and lower-class leftist radicalism." American Sociological Review 36:820–35.
1971b "On the logic of post-factum explanations: the hypothesis of lower-class frustration as the cause of leftist radicalism." Social Forces 50:26–44.

Portes, Alejandro and Adreain Ross
1974 "A model for the prediction of leftist radicalism." Journal of Political and Military Sociology 2:33–56.

Runciman, W. G.
1966 Relative Deprivation and Social Justice. London: Routledge and Kegan Paul.

Sears, David O. and John B. McConahay
1970 "Racial socialization, comparison levels, and the Watts riot." Journal of Social Issues 26:121–40.

Snyder, David
1978 "Collective violence: a research agenda and some strategic considerations." Journal of Conflict Resolution 22:499–534.

Snyder, David and William R. Kelly
1979 "Strategies for investigating violence and social change: illustrations from analyses of racial disorder and implications for mobilization research." Pp. 212–37 in Mayer Zald and John McCarthy (eds.), The Dynamics of Social Movements: Resource Mobilization, Social Control and Tactics. Cambridge, Mass.: Winthrop.

Snyder, David and Charles Tilly
1972 "Hardship and collective violence in France, 1830–1960." American Sociological Review 37:520–32.

Sörbom, Dag
1975 "Detection of correlated errors in longitudinal data." British Journal of Mathematical and Statistical Psychology 28:138–51.

Specht, David A. and Richard D. Warren
1976 "Comparing causal models." Pp. 46–82 in David R. Heise (ed.), Sociological Methodology. San Francisco: Jossey-Bass.

Spilerman, Seymour
1970 "The causes of racial disturbances: a comparison of alternative explanations." American Sociological Review 35:627–49.

Starnes, Charles E.
1973 "Status and status derived explanations of attitudes toward protest direct action: an assessment of survey data from Gary, Indiana, 1969." Ph.D. dissertation, Department of Sociology, Indiana University.

Tilly, Charles
1964 "Reflections on the revolutions of Paris: an essay on recent historical writing." Social Problems 12:99–121.
1978 From Mobilization to Revolution. Reading, Mass.: Addison-Wesley.

Tomlinson, T. M.
1970 "Ideological foundations for Negro action: militant and non-militant views." Pp. 326–79 in N. E. Cohen (ed.), The Los Angeles Riots: A Socio-Psychological Study. New York: Praeger.

Useem, Michael
1975 Protest Movements in America. Indianapolis: Bobbs-Merrill.

Vanneman, Reeve D. and Thomas F. Pettigrew
1972 "Race and relative deprivation in the urban United States." Race 13:461–86.

Warren, Donald I.
1970 "Suburban isolation and race tension: the Detroit case." Social Problems 17:324–39.

Wheaton, Blair, Bengt Muthén, Duane F. Alwin and Gene F. Summers
1977 "Assessing reliability and stability in panel models." Pp. 84–136 in David R. Heise (ed.), Sociological Methodology. San Francisco: Jossey-Bass.

Wilson, Kenneth and Anthony M. Orum
1976 "Mobilizing people for collective political action." Journal of Political and Military Sociology 4:187–202.

Woelfel, Joseph, John Woelfel, James Gillham and Thomas McPhail
1974 "Political radicalization as a communication process." Communication Research 1:243–63.

ERRATA

- An error occurred on page 708 in the article, "The Open and Closed Question" (ASR, October, 1979), by Howard Schuman and Stanley Presser.

In column one, the third sentence in the first full paragraph began: "As Table 7 shows, 99% of the . . . ." It should have begun: "As Table 7 shows, 98% of the . . . ."

- Errors occurred in Table 3 (page 921) of the article, "Inequality and Police Strength" (ASR, December, 1979), by David Jacobs.

First, in accord with Table 2, each of the four beta weights (.227*, .135, .213*, and .107) for the population variable ("Log Population") should move to the right, one column. Thus, coefficients for this variable should have appeared only in the columns under the headings, "Substantial Intercorrelations."

Second, the coefficient for the "% Black" variable in the fourth column from the left (.206) should also have carried one asterisk (.206*) to indicate that it, too, was significant at the .05 level.

- An error occurred in the article, "Temporal Changes in Work Content" (ASR, December, 1979), by Kenneth Spenner.

On page 971, column two, line 35 reads: "flects new jobs in occupational structures)." It should have read: "flects new jobs in the occupational structure)."

BEYOND METHODOLOGY: THE PLACE OF THEORY IN QUANTITATIVE HISTORICAL RESEARCH*

BARBARA LASLETT
University of Southern California

American Sociological Review 1980, Vol. 45 (April):214–228

This paper analyzes the relationship between theory and methods in that body of research that has come to be called "the new social history." First, an argument is made that the quantitative analysis of individual-level data that characterizes much of what is new in the recent sociohistorical literature is not only a methodology for conducting empirical research but also embodies an implicit theory of social organization that is both quantitative and individual. The power of such a theory to explain social change is limited, however, not only by technical problems associated with measurement and sampling error in historical research but also by the absence of an explicit causal component in the model. An alternative perspective, based on Marx's materialist theory of history, and which, it is argued, overcomes these limitations, is then presented. Empirical findings from research on the family in nineteenth century Los Angeles and Duncan's discussion of structural equation models, are used to illustrate how different theories of social change might be empirically tested.

There is a long tradition of debate about the relationship between theory and methods, between the explanation of empirical observations and the techniques used to generate those observations, in the history and philosophy of the social sciences. The increasing application of quantitative techniques to the analysis of historical data and the longstanding interest among social scientists in theories of social change provide an opportunity to reconsider some of the issues involved in this debate. This paper will present an analysis of the relationship between theory and methods in that body of research that has come to be called "the new social history." Examples will be drawn primarily, although not exclusively, from historical studies of the family.

The investigation of historical subjects is a particularly useful case for exploring the relationship between theory and methods in the social sciences. In contrast to research on contemporary issues, where the possibility exists that information not already available can always be collected (in theory, if not necessarily in practice), historical data cannot be infinitely expanded. Although new historical sources may be discovered, they cannot be manufactured. Under these conditions, explicit theoretical formulations would seem to be of particular value in helping to interpret the answers to historical questions that are often based on less than adequate source materials. Ironically, however, this has not been the case. As Murphey describes it (1973:148):

> Frustrated by the incompleteness of the historical record, and harassed by the knowledge that a malevolent fate will inevitably deny them some portion of the data needed to confirm even the simplest theory, many historians have even renounced theorizing altogether. . . .

The tendency to "renounce theorizing altogether" is one of the ways in which historians and sociologists have traditionally been distinguished from each other. The work of the former has usually been characterized as an effort to describe events, institutions, and individuals from some past era while the work of the latter is more often thought of in terms of an effort to formulate general laws of social organization that are transhistorical, although they are usually based on contemporary data. A more useful way of differentiating types of social science, and social scientists, might be the distinction between empiricists and theorists. I would suggest, however, that there is no necessary correlation between type of scholarly activity and disciplinary affiliation. Most historians *and* sociologists are empiricists; some historians and sociologists are theoreticians.

---

* Direct all correspondence to: Barbara Laslett; Department of Sociology and Anthropology; University of Southern California; University Park; Los Angeles, CA 90007.

Paper presented at the 1979 meeting of the Social Science History Association. Thanks are given to William Bielby, Edna Bonacich, Robert Brenner, Alan Garfinkel, Nora Hamilton, Keith Hopkins, John Laslett, William Roy, James E. Smith, and Julia Wrigley for critical readings of earlier versions of this paper. Helpful comments were also made by Charles Tilly and another, anonymous, reviewer for the *ASR*.

This paper will attempt to show that the methodological approaches which have become a hallmark of the "new social history"—particularly the quantitative analysis of individual-level data—have mistakenly been assumed to provide the most appropriate basis for understanding social change *theoretically*. Thus, it will be argued that an implicit theory of social organization which is both quantitative and individual informs much of the most influential sociohistorical scholarship recently conducted and that this theory limits the ability to develop a causal theory of social change.

An alternative theoretical approach to understanding social change will then be explicated—one which, deriving from Marxist theory, focuses on the social relations and social forces of production, and views changes in class relations and material conditions as key factors in understanding both social organization and social change. An empirical example of changes in the family in 19th century Los Angeles will be used to demonstrate how existing statistical techniques can be employed to explore a theory of this kind. The Los Angeles examples will also be used to suggest that some important conceptual and methodological issues of a general nature are raised by using quantitative techniques to test a theoretical formulation that emphasizes qualitative changes in social organization over historical time.

Theoretical Issues in Historical Description

In recent historical family research, use of nominal listings (such as census schedules, tax rolls, city directories and parish records of vital events) as data sources has had two complementary consequences. First, such sources have made it possible to discuss the history of family life in more general terms than was previously the case because these listings often refer to whole populations rather than particular groups within them. Belief that the family in the preindustrial world was predominantly extended in structure may reflect the data on which this statement was based. Wealthier families tended to live in larger and more complex households (P. Laslett, 1972:154; Berkner, 1972; B. Laslett, 1975) and were also more likely to have left diaries, letters, and other types of literary and material artifacts, which might be preserved. Secondly, the use of various kinds of nominal listings has led to the development and application of techniques for constructing and analyzing historical data quantitatively (Wrigley, 1972; 1973); thus, statistical rather than intuitive criteria can be applied to estimating the reliability of observed family characteristics and the significance of relationships between, for instance, wealth and household structure (B. Laslett, 1975; 1977).

The use of quantifiable sources and advanced statistical procedures in their preparation and analysis and the application of sociological concepts has revolutionized the historical study of the family, but the revolution has not been a bloodless one.[1] While solving certain intellectual problems, others have been raised. As descriptions of the family in past times have become increasingly available, two primary questions about them can be asked: (1) How reliable and valid are these descriptions? and (2) How do they contribute to an understanding of the relationship between social change and the family?

Unfortunately, more attention has been given to the first of these questions than to the second. Thus, one major issue in historical family research is whether the family can best be investigated through research on the residential unit, the household (which census schedules, for instance, usually describe); on the nuclear kinship group (available through family reconstitution based on parish records); or on the extended kinship unit (which often requires the linkage of records beyond those indicating vital events only). (See, for instance, Berkner, 1975; Hareven, 1975a; Mitterauer and Sieder, 1979; Verdon, 1979.) This issue is, in essence, the problem of moving from the concept of "family" to its operationalization. In the fray of battle, however, the distinction between theoretical concept and measured variable has been lost.

---

[1] See Judt (1979) for a particularly vitriolic attack on recent sociohistorical scholarship. For a more useful analysis which, like Judt's, adopts a Marxist perspective, see Stedman Jones, 1976.

The "family" is a concept, an intellectual construct. In contemporary usage, it differentiates those individuals related to each other on grounds of kinship and residence from those who do not combine these characteristics to some degree.[2] Other definitions of "family" can also be considered, one of which might focus not on compositional characteristics of the unit but on social function. According to this perspective, the family could be defined in terms of its responsibility for reproduction—that is, for the renewal of life on a daily basis and intergenerationally.

The first observation, then, that can be made about the concept "family" is that its meaning is not self-evident. While no operationalization which focuses on kinship, residence, or function *alone* may be a complete account of the family as a social institution, empirical results based on any particular and necessarily partial operationalization are not unreliable simply because they are limited by it.[3] As Blalock (1964:5) has observed, there is "*an inherent gap between the language of theory and research* which can never be bridged in a completely satisfactory way" (italics in the original). Thus, debate on how to adequately operationalize the concept "family," while helping to refine empirical research techniques, cannot address the theoretical question of why studying the family is important for understanding social organization and social change.[4]

For instance, if the family's major social importance is seen in its socioemotional impact on individual lives, particularly through the socialization of young children, then operationalizing the family in terms of the coresident domestic group might be most appropriate. (P. Laslett's argument, 1972:65, suggests this perspective.) If, on the other hand, the family is thought of primarily as a unit of production, then operationalization in terms of the personnel involved in the productive activities that maintain the unit—whether kin or nonkin—would be necessary. (See Friedmann, 1978; Goldin, 1979; forthcoming; Goody, 1972; Hareven, 1978; Hareven and Vinovskis, 1978; Middleton, 1979; Tilly and Scott, 1978, for discussions of the family as an economic unit.) And if social reproduction is seen as the concept of primary theoretical interest, then the boundaries between the family and other social institutions become less clear; exploring reproduction in this more general sense may require the investigation of institutions such as schools, trade unions, and political organizations—in addition to the kinship or coresident domestic group. (B. Laslett, 1979, presents a discussion of this approach.)

Replacing debate over theories of the family, theories of social organization, and theories of social change, with arguments for and against the adequacy of residential vs. nonresidential kinship groups as ways to operationalize the concept "family" is, in my view, misleading because it implies that the basic problem in historical family research lies in unreliable (or inadequate) source materials. Limited as the historical records may be, the problem is, more often, inadequacies of the theoretical model. (See C. Tilly,

---

[2] This meaning of "family" is itself historically specific. Berkner (1972) points out that "the word family was not commonly used in German until the eighteenth century—before that people spoke of belonging to a house or household."

[3] It should be noted that each of the operationalizations mentioned in the text presents information that is limited in one way or another. The household unit excludes nonresidential kin while the ability to reconstruct familial groups beyond the coresident domestic group is limited by the often scanty records that make it impossible to trace a systematic sample of kinship units beyond the residential group.

[4] In general, more attention has been given to this theoretical question by Marxist social scientists than by others. For a review of some of the arguments from this perspective, see Humphries, 1977.

1979, for further discussion of this issue.) The problems involved in conducting empirical research have been confused with the difficulties of specifying the theoretical models that guide it. If this is the case, the solution lies not in more and better empirical data, necessary though that may be, but, rather, in the development of better (and necessarily more explicit) theoretical formulations.

Quantification and the Individual in Implicit Theory

Much of the sociohistorical literature which has emerged in recent years reflects a quantitative perspective—not merely as a methodology for conducting empirical research—but as a statement of what is important about changes over historical time. Questions have been asked, for instance, about the distribution of household types; the extent of social mobility; the number of births, marriages, and deaths; the density of kinship networks; the proportion of children enrolled in schools; and the amount of social inequality in past times. (See, for instance, Anderson, 1971; 1976; Berkner, 1972; Furstenberg et al., 1975; Hareven, 1975b; 1978; Hareven and Vinovskis, 1978; Katz, 1975; Knodel and Maynes, 1976; P. Laslett, 1972; Thernstrom, 1973; C. Tilly, 1978; van de Walle, 1976.) Sociohistorical research—with the primary objective of estimating changes in the *number* of extended and nuclear families (P. Laslett, 1972) or the *amount* of occupational mobility (Thernstrom, 1973) at different points in time—implicitly assumes that what is important about history is quantitative change.

From one point of view, an argument can reasonably be made that a concern with *amounts* or *numbers* is a necessary first step in the process of understanding social change; that accurate description of the changes that have occurred over time must be available before it is possible to develop explicit theoretical models which attempt to explain them. But this perspective on the relationship between theory and methods obscures both practical problems within the research process and important theoretical issues.

If social change is conceptualized in terms of variation in the distributions of characteristics—such as wealth, occupation, or family type—within a given population, and if this change is measured by estimating quantitative differences between these distributions at two or more points in time, then both measurement and sampling error (two of the methodological problems that quantification has been used to overcome) are of particular importance: it is on the basis of these distributions alone that an assessment is made about whether or not there is any change to be explained. Measurement and sampling error, of course, cannot be eliminated, both because the researcher does not control the data which forms the basis of her or his observations and, more importantly, because of the inherent gap between theoretical construct and empirical indicator of which Blalock (1964) speaks. Thus, measurement and sampling error pose particularly severe problems when quantitative differences are the criteria for assessing whether or not social change has occurred.[5]

But a further, conceptual, problem is raised by using changing distributions as a measure of social change, even if the relevant distributions can be measured with an acceptable degree of reliability. Duncan (1975:57) distinguishes two types of social change, one which reflects the changing features of a *distribution* of attributes and another which reflects changes in the *relationships* between attributes. When the former occurs, he says,

> . . . . One would then observe "social change" with respect to variances and covariances of the dependent variables. But in another sense no "social change" would be occurring, since the structural coefficients (which represent the relationship between variables) were staying constant. If, on the other hand, the latter should change, one would really be dealing with social change, in a deeper sense of the term.

According to this perspective, social change "in a deeper sense of the term" involves change in social relationships—in social organization as a whole—and not simply changes in the distribution of the individual components within the whole. While changes in distribution may identify variation in composition—that is, in the characteristics of individuals who fill positions within the social order—it does not necessarily mean that the social relationships and processes that define the social order itself have been transformed; social change "in a deeper sense of the term" does have such a meaning. This distinction—between social change that reflects changes in population characteristics (as measured by distributions of attributes) and social change that refers to changes in social relations (as measured by relationships between attributes)—has received relatively little attention, either methodologically or theoretically, within the "new social history."

---

[5] See Furstenberg et al. (1979), for an analysis of measurement error in one historical source. Measurement error is, however, also present in research on contemporary subjects although the ability to estimate its extent may, perhaps, be greater.

Thernstrom (1973), for instance, talks about the need to consider social structure in his study of Boston between 1880 and 1970. But the empirical indicator that he uses to discuss social structure is the distribution of individuals within the occupational structure. His analysis of changes in social structure focuses on the individual's experience of intra- and intergenerational occupational and social mobility—that is, on historical changes in the distribution of individuals within the occupational structure. A similar approach can be found in Katz's (1975) study of Hamilton, Ontario, between 1851 and 1861. Here, again, social structure is defined in terms of quantitative distributions of individual characteristics.

Four measures form the basis of Katz's operationalization of social structure. They are:

> (1) the occupational structure, or the proportion of the population engaged in various sorts of work; (2) the division of wealth, or the proportion of the wealth held by people in each economic rank; (3) the social and demographic identity of different economic ranks, or the characteristics of people with varying amounts of wealth; (4) the distance between the people of different social ranks. (Katz, 1975:44–5)

Information on each of these topics is obtained by counting the number of individuals in categories of some predetermined system of classification, such as occupations or dollars, and then performing an arithmetic computation on that information. Thus,

> . . . what is offered is not the theoretical construction of an object of study but merely the systematization of predefined data . . . a purely quantitative hierarchy of discrete groups who exist in no obvious relationship to one another. (Stedman Jones, 1976)

For Thernstrom and Katz, therefore, the investigation of social change is based on an implicit theory of social organization that is both quantitative and individual. As a consequence, social change "in a deeper sense of the term" can be neither identified nor explored.

It should be noted that there is nothing inherent in the use of *distributions per se* that limits their use to the description of individual characteristics; social relationships as well as individual attributes can also be described in terms of their distribution within a society. Furthermore, social relationships can also be viewed as being embedded in the distributions of individual characteristics. As Duncan points out (1975:166), in discussing *Inequality* (Jencks et al., 1972),

> . . . the "*b*" in his [Jencks's] model—say, dollars of income annually per number of years of schooling completed—is hardly a coefficient that describes merely the behavior of the *i*th individual. After all, it is *someone else* who pays him the income. . . . (Italics in the original)

But the impetus to identify and discuss social relationships from distributions depends on the theory of social change that informs one's research agenda; it is not determined by the technical approaches employed in the study of social change. Quite the contrary, one's theoretical approach—implicit or explicit—affects the methodologies that are developed and employed. Duncan's (1975) identification of structural coefficients as empirical indicators of social relationships is reflective of his theoretical view of what constitutes social structure, rather than being an inherent feature of the coefficients themselves.

Causal Theory in the Study of Sociohistorical Change

Most of the recent sociohistorical studies have been relatively inattentive to the problems of inferring causal relationships from statistical association. (Levine, 1977, is an exception in this respect.) But a causal theory, whether implicit or explicit, is always present in any act of historical interpretation. As Murphey (1973:102) says, "an interpretation always contains a causal model—whatever else it may tell us, it always claims to tell us why certain events or sequences of events occurred." A noncausal "theory" of history is, in essence, neither theory nor history. An attempt will be made in the following section of this paper to discuss the form and content that an explicit causal theory of social change might take.

One of the intellectual problems that has inhibited efforts to combine the traditional interests of historians and sociologists is the seeming contradiction between the former's efforts to preserve the *uniqueness* of place and time and the latter's efforts to develop *generalizations* about social organization and social change. This apparent problem can be expressed by the following question: If each nation or system of nations, each region, each category of individuals, can be properly analyzed only within a historically unique context because of the specific configuration of events and/or forces that define it, then how can a general theory of society be constructed?

What is needed is further specification of the meaning of "generalization" in sociohistorical studies.[6] The type of generalization most often encountered in sociological work is about the outcomes of change. For instance, William Goode (1963) concludes that the predominant change in the family that has accompanied industrialization, urbanization, and modernization is toward what he calls the conjugal family form. Goode emphasizes that the direction of change which results in this outcome may vary in different societies because the family systems from which the contemporary forms emerge are, for historical reasons, different. But his analysis focuses on a convergence in outcomes—toward the conjugal family system—and this result, he suggests (and supports with an impressive array of cross-national empirical data) is generalizable across societies and cultures, throughout the world.

An alternative perspective to the problem of "generalization" is neglected by this approach and is particularly appropriate for historical studies: it uses the term "generalization" to denote *causes* rather than *outcomes*.[7] In contrast to the search for generalizations about outcomes, which allows the possibility of multiple starting points, a search for generalizations about causes allows for multiple outcomes to result from a more limited set of causes. This one-to-many correspondence between cause and effect seems an appropriate theoretical approach to research where the importance of historical specificity and uniqueness is repeatedly demonstrated.[8]

Changes in the world economy, for example, affected land availability in colonial Andover, Massachusetts (Greven, 1970), in 18th through 20th century Estonia (Parming, 1979), on the American frontier in the 19th century (Easterlin, 1976), and in antebellum and post-Civil War Alabama (Barney, 1979). In each case, the availability of land declined but the consequences for the family and kinship structure varied. In Andover, movement of sons from immediate proximity and control of parents resulted; in Estonia, fertility reduction occurred; on the American frontier, a combination of these two outcomes took place; and, in Alabama, similar consequences were rapidly intercepted by the Civil War that has, ever since, eclipsed declining land availability as a prior cause of family and kinship change in the American South.

---

[6] Hempel's (1959) discussion of "the covering law model" is relevant to this issue. See Murphey (1973, chap. 3), for a review of the extensive critical debate which Hempel's position evoked.

[7] The part of the discussion which emphasizes the search for causes rather than outcomes was developed jointly with James E. Smith. See Smith and Laslett (1979).

[8] The error terms in, for example, structural equation models, allow multiple outcomes—but only as residual error, not as integral parts of a theory.

To search for causes that transcend spatiotemporal boundaries would remove the theoretical approach to the study of history from the traditional distinction between sociology and history. Traditional history has been dominated by descriptive research, with unique events—such as wars, depressions, and plagues—often assumed to be obvious causes that cannot be generalized across time or space. Sociologists, on the other hand, have traditionally followed the assumptions of the physical sciences requiring that there always be a one-to-one correspondence between cause and effect; by failing to allow for multiple outcomes from single causes, sociologists have, too often, disqualified themselves as accurate observers of the broad and varied sweep of history.

In order to develop a general, historically grounded theory of social change, a goal that is not necessarily shared by all social scientists—even among the most distinguished (see Roth, 1979:181, on Fernand Braudel)—studies that are concerned with this question need to search for (and test) explicit causal explanations for the varied outcomes that may be empirically observed. It is the ability to provide the causal inferences that link historically specific phenomena with general historical processes that would make truly explanatory the theory of which they are a part.

At least three types of causal explanations for historical change can be identified: (1) naturalistic, (2) individual, and (3) social. A naturalistic explanation of history would entail a belief that some outcome, such as the universality of the nuclear family, was inherent in—and an inevitable result of—characteristics of the human species. If this were the case, then the basic explanation for the organization of the family would be found in nature rather than in the social realm. Explanations of this sort can, of course, be found; life cycle variation in household and family structure in past times may provide one example.

Berkner (1972) found, in an analysis of household structure in an 18th century Austrian manor, that the proportion of extended and nuclear families varied by the age of the household head and suggested that household structure may systematically vary over the family life cycle. When the household head was relatively young, the coresident family was extended by the presence of the elderly parents of the head. The death of the parents changed the structure of the household, for a time, to nuclear. As the married couple aged, their children grew, married, and had children of their own. If one child and his or her own family of procreation remained within the parental household, the family would once again become extended in structure and would remain so until the death of the parents, whereupon the cycle would again proceed to the nuclear phase. These results suggest that there was a significant effect of age on the structure of the coresident domestic group in several pre- and early industrial settings. But social, in contrast to natural, change cannot be known from life cycle variation alone.

Duncan (1975:167) makes the point in statistical terms when he says that some distributions may change because of "alterations implicit in the very nature of the social structure and its initial conditions, as growth is implicit in the structure of an immature organism." Thus, variation in the age structure of a population over time is not necessarily the result of social change since it may be explained by natural processes of maturation. But if changes in the *relationship* between age and household structure are observed historically, then an explanation for them would be needed, and Duncan suggests why it is unlikely that such an explanation would be found in nature alone. He (Duncan, 1975:167) says, "The structures we hope to describe in our model are not fixed in the human gene pool nor dictated by the laws of physics. They can and do vary."

The second explanation—that social change is basically due to the actions of individual women and men in history—has had many proponents among social scientists. According to Roth (1979:195–206), Weber, for instance, considered this question in discussing the several possible levels of historical analysis. His moral concern over the importance of the individual as social and political actor in "swimming against the stream" of history, however, led him to think of history as "the causal analysis of individual phenomena."

Weber was not alone, either in his moral or political concerns or in his insistence that the individual was the most significant unit for both creating and understanding history. (See Stedman Jones, 1973, and Lukes, 1968, for further discussions of this question.) Gellner (1973:14), I think, provides an important response to this emphasis upon the individual in so much historical writing when he says,

> Perhaps, in the end there is agreement to this extent: (human) history *is* about chaps—and nothing else. But perhaps this should be written: History is *about* chaps. It does not follow that its explanations are always in terms of chaps. Socieites are what people do, but social scientists are not biographers *en grande série*.

Thus, although social change may result from what "chaps" do, it does not automatically follow that explanations of these actions, or their consequences, must consider individual factors only.

One theoretical basis for the third—the social—explanation of history already exists within the social science literature, although it has received relatively meager empirical attention in quantitatively oriented research: Marx's materialist theory of history. In brief, this theory (Marx, 1972:4) states that:

> In the social production of their life, men enter into definite relations that are indispensable and independent of their will, relations of production which correspond to a definite stage of development of their material, productive forces.

The theory further suggests that social change can be understood in terms of the contradictions between the social relations and social forces of production that develop over historical time.

Thus, an explicit theory of the organization and change of societies, which rests on social relations, not on natural or individual causes, has been proposed. The following section of this paper will apply this formulation to a general understanding of changes in the family over historical time and then to a specific example from 19th century Los Angeles. In the process of considering this empirical example, Duncan's (1975) proposed solution to some of the statistical problems involved in the empirical study of social change will be applied.

A Materialist Theory of the Family in History

There are two basic requirements for the survival of any society that can be said to have a history: production and reproduction. Both are required for the maintenance of life from day to day, and for survival across generations. While both production and reproduction are necessary for human survival, they do not necessarily have equal force as causes of social organization or social change. The formulation presented here assumes that the immediacy of daily needs has causal priority over longer-term intergenerational needs. If this is the case, then, the social organization of production, by defining the conditions which affect access to the material goods necessary for life on a day-to-day basis, should have causal priority over the social organization of reproduction.

But reproduction is also necessary if survival is to occur beyond a single life span; the social organization of reproduction should, therefore, have a direct effect on the development of any social system over time. Thus, the family, as the major social institution responsible, historically, for reproduction, while operating within limits set by the social organization of production, should not be wholly determined by it; the family should have an independent effect on history. The degree to which this statement can be empirically validated would provide a test of the causal priorities contained within the model.

As the term is used here, "reproduction" refers to the activities, institutions and relationships necessary for the maintenance and renewal of life. While the specific content of reproductive activities and relationships may vary historically, they invariably include the sexual and psychological dimensions associated with procreation, and the nurturing activities associated with care, protection, and socialization of children. Given its historical responsibility for these activities, the family may be seen as an institution with its own unique goals; it need not, however, be completely defined by these goals. Thus, variation in the family's range of activities may, in some historical periods, extend beyond the sphere of procreation and socialization to include production itself and, at other times, may exclude certain aspects of socialization and nurturance, such as formal education or the care of the elderly. When the former occurs, the study of production and reproduction are, institutionally, the same; they both occur within the family. When the latter is the case, the study of reproduction may require investigation of institutions outside the family.[9]

This brief theoretical statement is, obviously, highly general and abstract. Presenting its content in more concrete terms is beyond the scope of, or reason for its inclusion in, this paper. (See B. Laslett, 1979, for a more detailed statement.) Certain features of the formulation, however, can be identified even in the abbreviated form presented. In the first place, causal priority is explicit in the theory: the social organization of production, by structuring access to the material goods necessary for daily life, explains *why* other aspects of social life and social organization, including the family, have some of their particular characteristics. But the theory does not imply that the social organization of production *determines* family structure or behavior. As an institution with its own responsibility—that of reproduction—the family will attempt to fulfill its own goals and, by so doing, will have an independent effect on history, given the necessity of reproduction for intergenerational survival of the society as a whole.

In addition to making explicit which social factors have causal priority *and* the limitations of those factors as causes, a second implication of the formulation can be identified, even in the brief and general form in which it has been presented. If the social organization of production has primary explanatory power in relation to the family, then—when the social organization of production changes—the family should also change. The social organization of production, however, is a concept that refers to a pattern of relationships in the society as a whole, not only to individual units within it. Thus, a third implication of the formulation can be identified—one which suggests that different levels of theoretical analysis may be necessary for understanding the family, or reproduction in general, under varying historical conditions.

Historically, the development of capitalism has meant that the division of labor in the process of production has become organized on a world scale (Wallerstein, 1974). Units of reproduction, however, are considerably smaller. Whether thought of in terms of the co-resident domestic group, of the extended kinship system, or even of the nation state (which provides some material resources for reproduction in the modern world), the units of analysis most appropriate for investigating *repro*duction are considerably more limited in scope than is the case for *pro*duction.

The general practice in most recent historical family research, however, is to relate some socioeconomic characteristic of the individual with some feature of her or his family organization or behavior. If the organization of production is most adequately understood as a feature of the society as a whole, however, such an analytic procedure cannot adequately test the proposed relationships between production and reproduction, or even between the economy and the family. Thus, under some historical conditions, such as those that describe the organization of production under capitalism, investigating such a relationship may require that attention be paid to different *conceptual* levels of analysis: the social organization of production as *a whole* as well as the behavior and attributes of individual units within it.

Marx's theory is specific about those

---

[9] For additional discussions of the relationship between production and reproduction which provide a somewhat different focus, see Meillassoux, 1972; 1973.

aspects of the social organization of production that are theoretically important. He distinguished different modes of production (Marx, 1964; 1967) and identified class relations and the productive forces as key causal factors in explaining empirically observed phenomena, in both the past and the present.[10] To test such a theory in relation to changes in the family under capitalism, it is necessary to attend to both the specific features identified by Marx as key to understanding the social organization of production and the units of reproduction which historically have been more localized and individual. This issue will be discussed further in the context of an empirical example of changes in household structure in Los Angeles, California, between 1850 and 1870 (B. Laslett, 1977).

Household Structure and Social Change in 19th Century Los Angeles

Within the twenty years between 1850 and 1870, Los Angeles developed from an isolated, self-sustaining, and largely barter economy into a dependent, money economy (Fogelson, 1967). Although the economy was largely agricultural in both years, the nature of economic activity had changed: farming had become commercialized and was increasingly organized for purposes of exchange, rather than for purposes of subsistence. From a Marxist point of view, these changes would have resulted in changing class relations and changes in the forces of production which, together, describe the organization of production in the society as a whole. It is from this perspective that the empirical findings of changes in the family will be considered.[11]

In 1850, 29% of the households in the city of Los Angeles were nuclear in structure; by 1870, this proportion had increased to 46%. This change, however, appears to be due to changes in the distribution of laborers in the two time periods. The proportion of laborers had increased from 18% to 30%, and being a laborer was significantly related to heading this kind of domestic unit in both years. But, from another point of view, no social change in relation to the laborers' families had occurred since the structural coefficients, which describe the relationships, at the two points in time, between being a laborer and heading a nuclear family household, were invariant. This reasoning, however, does not explain the decline in the proportion of extended family households between 1850 and 1870 (from 16% to 7%).

Although the distribution of farmers remained nearly constant in the two time periods (19% in 1850, and 21% in 1870), the structural coefficients, which describe the relationships between being a farmer and heading an extended family household, declined in magnitude. If this change in structural coefficients is to be explained in *social* terms, Duncan's (1975) discussion would suggest that it is in changes in social relationships that the answer will be found.

But a question may also be raised about why the proportion of laborers had increased between 1850 and 1870. An answer would tell us nothing about changes in the structure of laborers' families during this time period, since the association between being a laborer and heading a nuclear family household was invariant over the twenty-year period. It might, however, tell us something about changes that were occurring in the social organization of production; and any theory which can explain *both* changing distributions and changing relationships is clearly more powerful than one which can explain one type of variation but not the other.

Two questions of interpretation are raised, then, by the empirical findings discussed: (1) Why did the structural coefficients estimating the relationships between being a farmer and heading an extended family household change over time? and (2) Why did the proportion of laborers increase? Both of these questions involve conceptual, and not merely empirical, issues. They require, at the very outset, the statement of a *theory of history* which, as already argued, must inevitably include a *causal* component. Both the theory and its causal element need clear explication.

---

[10] It is this specification that differentiates Marxist theory from other formulations which may also focus on relationships between parts of the social system (Smelser, 1959).

[11] Detailed documentation of the brief description of changes in Los Angeles cannot be presented here. It will be available in the author's research on the family in Los Angeles between 1850 and 1900. For early results from this study, see B. Laslett, 1975; 1977; 1978.

Consistent with the materialist approach to the history of the family that has already been presented, a response to the first question—why the relationship between being a farmer and heading an extended family household changed over time—would be sought within transformations in the social organization of production as a whole. Since the occupational structure may tell us something about this aspect of social organization, an answer to the second question—why the proportion of laborers increased—may provide an additional test of the theoretical approach used to answer the first question.

From a Marxist perspective, one consequence of the economic changes that occurred in Los Angeles between 1850 and 1870—the increasing commercialization of agriculture, the gradual transformation of the economy from production for subsistence to production for exchange—was that the class structure of the society as a whole was being transformed. Producers were increasingly becoming wage workers, and the means of production were increasingly being used for purposes of accumulation rather than for purposes of consumption. But how do these changes allow us to understand the relationship between social organization and household structure? Four separate opportunities to answer this question are available in the empirical example being discussed.

Laborers, in both 1850 and 1870, were more likely to be dependent on an exchange of labor power for the material goods necessary to maintain a family, and in both years they were more likely to head nuclear family households. Changes in the social organization of production did not affect these relationships because, in both cases, laborers' access to the means of production was the same.

Farmers, in contrast, were less likely than laborers to be wage workers in either year. But the relationships between their *individual class position* and the type of household they headed did change. Why? For farmers, the development of capitalism—which involved specialized production for exchange rather than diversified production for consumption—meant that in 1870 they were more like laborers than they had been in 1850; they, too, had to engage in market exchange—of agricultural products, although not of their own labor power—in order to obtain the material resources necessary for reproduction. The social meaning of individual class characteristics had changed because the social organization of production as a whole had been transformed.

Beyond suggesting why the commercialization of agriculture explains the observed change in the relationship between being a farmer and household structure, the theory can also explain why the proportion of laborers increased between 1850 and 1870. Although agricultural activity dominated the local economy in both years, the proportion of the population who were land owners declined, from 53% to 38%, between the two decades (B. Laslett, 1977). The separation of producers from the means of their own subsistence is a central feature of capitalism as a mode of production and, according to Marx, a basic cause of the dynamics of change within the system as a whole. Thus, the increasing proportion of laborers would also appear to be explained by the development of capitalism in Los Angeles.

Whether or not Marxist theory can, in the final analysis, adequately explain this or other changes in household structure—or other features of family life, in places and time periods different from Los Angeles in the 19th century—must await the results of further empirical research. Such research, however, must be explicitly informed by a causal theory in which particular aspects of social organization are identified as significant for understanding historical change.[12] Research designs and measurement procedures which address these specific features of social relationships are necessary for testing the theory's validity. To test such a theory, techniques must be developed which can adequately estimate changes in the social relations and social forces of productive activities over time, and not merely changes in the distribution of socioeconomic characteristics of individuals within a given society. Thus, Marxist theory, like any other, raises methodological as well as theoretical and empirical problems.[13] But, unless relevant aspects of this (or an alternative) theory are made explicit, no amount of attention to isolated measurement or statistical problems will provide the basis for advancing our understanding of social change.

---

[12] The general theory presented briefly in this paper also provides a basis for explaining how the needs of the family—or reproduction—contribute to social change. (For an elaboration of these ideas, see B. Laslett, 1979.) They will be explored empirically in the larger study of Los Angeles in the second half of the 19th century, of which the empirical example discussed in the text is one part.

The adequacy of research designs for historically grounded studies of social change are also affected by the theoretical model which they are attempting to test. It is only when an empirical investigation is comparative over time that the structural coefficients would be available that could indicate whether or not social change had occurred. It is therefore striking how much historical family research does *not* have temporal variation beyond the individual life cycle built into it (see, for instance, B. Laslett, 1975; Hareven, 1978).

It is not sufficient, however, for historical research to encompass *any* temporal period. A study that wishes to measure the effects of social change must be certain that the theoretically relevant changes actually occurred in the span of years covered by a particular study. Research on Los Angeles that stopped in 1870, for instance, would not be able to measure many of the effects of capitalism on the family since its major developments did not occur until after the mid-1880s. Thus, some historical knowledge is necessary for designing research which can adequately assess the effects of those changes that are identified as theoretically important. While this requirement should not be troublesome to professional historians, it cannot necessarily be taken for granted among sociologists. Just as social historians have had to become familiar with statistical methodology that is routinely taught to sociologists, so sociologists who wish to engage in historical research must develop their sensitivity to, and empirical knowledge of, different historical eras and events.

Conclusion

A discussion has been presented that suggests the need for explicit attention to the basic assumptions which underlie different theories of history since they affect the empirical research that is done. Little advance in the study of social change will be achieved if research is informed primarily by implicit theory or by the technical problems relevant to analyzing quantitative historical data. This is not because technical matters are unimportant, but because different theories of history pose different empirical and methodological problems. Technical decisions are dependent on, not independent of, theory. It is important that discourse in quantitative social history goes beyond methodology.

Unless the relationships between theory and methods are explored—which requires that theories become explicit and their causal components be specified—our empirical knowledge about past times may improve at a rapid pace without necessarily providing better explanations for social change. This paper has discussed several issues relating to theories of social change and the family, and the methodological problems posed by the wish to empirically test those theories' validity with quantitative historical data. The argument which it presents, however, has implications beyond the historical study of the family—implications for the types of generalizations appropriate to research on contemporary, as well as on historical, subjects.

---

[13] One particularly important statistical problem which Marxist theory poses is how to measure the effect of temporal change when the need to minimize measurement error requires that the relevant variable be operationalized in qualitatively different ways in different time periods. If the social institutions of reproduction or production have changed over time from, for instance, a broader to a narrower kinship unit in the case of the former, or from the family to the firm in the case of the latter, then how can the quantitative effects of changes in the same functional units be estimated if different operationalizations must be used to measure them reliably, over time? To require that all terms that refer to the same phenomenon, or function, be measured in the same way in a time series analysis, regardless of the social realities of the historical period to which they refer, presents a statistical barrier to the quantitative study of social changes that refers to qualitative transformations in social relationships. Given the ingenuity of statistically oriented social scientists, however, I have no doubt that a solution to the problem, once identified, can be found.

If, as in 19th century Los Angeles, the relationship between being a farmer and heading an extended family household is not invariant over time but is affected by the social organization of production within which farming occurs, and if social organization as a whole has undergone qualitative transformations, over time, then no generalization about the outcomes of the relationship between farming and household structure can be universally valid. If relationships between variables which describe individual characteristics are affected by changes in the social organization of production as a whole, then generalizations which ignore its key features—the social relations and social forces of production—will be unable to accurately interpret the findings that historical research makes available. And if qualitatively different forms of social organization have existed over time, then sociology can no longer justify its ahistorical propensity by referring to the goal of generating empirically validated general theory. Sociological theory, to be empirically valid, must be historically specific. This is, perhaps, the most important implication for contemporary sociology to be drawn from historical research.

Most of the substantive questions raised by recent sociohistorical scholarship still require empirical answers. For advances in this direction to be made, however—in studies of the past, of the present, or of the processes by which the one was transformed into the other—the theoretical models which inform such research need to be made explicit. Only when this occurs can the rigorous methodological requirements necessary for testing and developing scientific theories of historical change be achieved. Research on past times, like research on contemporary questions, can provide empirical answers only to those questions that are asked, and theoretical work is necessary for asking better questions.

REFERENCES

Anderson, Michael
1976 "Marriage patterns in Victorian Britain: an analysis based on registration district data for England and Wales, 1861." Journal of Family History 1:55–78.
1971 Family Structure in Nineteenth Century Lancashire. Cambridge, England: Cambridge University Press.

Barney, William L.
1979 "Patterns of crisis: Alabama white families and social change." Sociology and Social Research 63:524–43.

Berkner, Lutz K.
1975 "The use and misuse of census data for the historical analysis of family structure." Journal of Interdisciplinary History 6:721–38.
1972 "The stem family and the developmental cycle of the peasant household: an eighteenth-century Austrian example." American Historical Review 77:398–418.

Blalock, Hubert M., Jr.
1964 Causal Inferences in Non-Experimental Research. Chapel Hill, N.C.: University of North Carolina Press.

Duncan, Otis Dudley
1975 Introduction to Structural Equation Models. New York: Academic Press.

Easterlin, Richard
1976 "Population change and farm settlement in the northern university states." Journal of Economic History 36:45–75.

Fogelson, Robert M.
1967 The Fragmented Metropolis: Los Angeles, 1850–1930. Cambridge, Mass.: Harvard University Press.

Friedmann, Harriet
1978 "Simple commodity production and wage labour in the American plains." Journal of Peasant Studies 6:71–100.

Furstenberg, Frank F., Jr., Douglas Strong, and Albert G. Crawford
1979 "What happened when the census was redone: an analysis of the recount of 1870 in Philadelphia." Sociology and Social Research 63:475–505.

Furstenberg, Frank F., Jr., Theordore Hershberg, and John Modell
1975 "The origins of the female headed black family: the impact of the urban experience." Journal of Interdisciplinary History 6:211–33.

Gellner, Ernest
1973 Cause and Meaning in the Social Sciences. London: Routledge and Kegan Paul.

Goldin, Claudia
1979 "Household and market production of families in a late nineteenth century American city." Forthcoming in Explorations in Economic History.

Forthcoming "Family strategies and the family economy in the late nineteenth-century: the role of secondary workers." To be published in a volume edited by Theodore Hershberg.

Goode, William J.
1963 World Revolution and Family Patterns. New York: Free Press.

Goody, Jack
1972 "The evolution of the family." Pp. 103–24 in Peter Laslett (ed.), Household and Family in Past Time. Cambridge University Press.

Greven, Philip J., Jr.
1970 Four Generations: Population, Land and Family in Colonial Andover, Massachusetts. Ithaca, N.Y.: Cornell University Press.

Hareven, Tamara K.
1975a "A review essay of Household and Family in Past Time." History and Theory 14:242–251.
1975b "The laborers of Manchester, New Hampshire, 1912–1922: the role of family and ethnicity in adjustment to industrial life." Labor History 16:249–65.
1978 (ed.) Transitions: The Family and the Life Course in Historical Perspective. New York: Academic Press.

Hareven, Tamara K., and Maris A. Vinovksis (eds.)
1978 Family and Population in Nineteenth-Century America. Princeton: Princeton University Press.

Hempel, Carl G.
1959 "The function of general laws in history." Pp. 344–56 in Patrick Gardiner (ed.), Theories of History. Glencoe, Illinois: Free Press.

Humphries, Jane
1977 "The working class family, women's liberation and class struggle: the case of nineteenth century British history." Review of Radical Political Economics 9:25–41.

Jencks, Christopher, Marshall Smith, Henry Acland, Mary Jo Bane, David Cohen, Herbert Gintis, Barbara Heyns, and Stephen Michelson
1972 Inequality. New York: Basic Books.

Judt, Tony
1979 "A clown in regal purple: social history and the historians." History Workshop 7:66–94.

Katz, Michael B.
1975 The People of Hamilton, Canada West: Family and Class in a Mid-Nineteenth Century City. Cambridge, Mass.: Harvard University Press.

Knodel, John, and Mary Jo Maynes
1976 "Urban and rural marriage patterns in imperial Germany." Journal of Family History 1:129–68.

Laslett, Barbara
1979 "Production, reproduction and social change: theory of the family in history." A paper presented at the annual meeting of the American Sociological Association, Boston, Mass.
1978 "Household structure and the social organization of production: Los Angeles, California in 1850." A paper presented at the IXth World Congress of Sociology, Uppsala, Sweden.
1977 "Social change and the family: Los Angeles, California 1850–1870." American Sociological Review 42:268–91.
1975 "Household structure on an American frontier: Los Angeles, California 1850." American Journal of Sociology 81:109–28.

Laslett, Peter
1972 (ed.) Household and Family in Past Time. Cambridge, England: Cambridge University Press.

Levine, David
1977 Family Formation in an Age of Nascent Capitalism. New York: Academic Press.

Lukes, Steven
1968 "Methodological individualism reconsidered." British Journal of Sociology 19:119–29.

Marx, Karl
1972 "A contribution to the critique of political economy." Pp. 3–6 in Robert C. Tucker (ed.), The Marx-Engels Reader. New York: W.W. Norton.
1967 Capital. Volume 1: A Critical Analysis of Capitalist Production. New York: International Publishers.
1964 Pre-Capitalist Economic Formations. New York: International Publishers.

Meillassoux, Claude
1972 "From reproduction to production: a Marxist approach to economic anthropology." Economy and Society 1:93–105.
1973 "The social organization of the peasantry: the economic basis of kinship." Journal of Peasant Studies 1:81–90.

Middleton, Christopher
1979 "The sexual division of labour in feudal England." New Left Review 113–114:147–68.

Mitterauer, M. and R. Sieder
1979 "The developmental process of domestic groups: problems of reconstruction and possibilities of interpretation." Journal of Family History 4:257–84.

Murphey, Murray G.
1973 Our Knowledge of the Historical Past. Indianapolis: Bobbs-Merrill.

Parming, Tonu
1979 "Long-term trends in family structure in a soviet republic." Sociology and Social Research 63:443–75.

Roth, Guenter
1979 "Duration and rationalization: Fernand Braudel and Max Weber." Pp. 166–93 (chap. 5) in G. Roth and Wolfgang Schluchter, Max Weber's Vision of History: Ethics and Methods. Berkeley: University of California Press. "Weber's Vision of History." Pp. 195–206 (Epilogue) in same volume.

Stedman Jones, Gareth
1973 "History: the poverty of empiricism." Pp.

96–115 in Robin Blackburn (ed.), Ideology in Social Science. New York: Random House, Vintage.
1976 "From historical sociology to theoretical history." British Journal of Sociology 27:295–305.

Smelser, Neil J.
1959 Social Change in the Industrial Revolution. Chicago: University of Chicago Press.

Smith, James E. and Barbara Laslett
1979 "Introduction." Sociology and Social Research 63:425–31.

Thernstrom, Stephan
1973 The Other Bostonians. Cambridge, Mass.: Harvard University Press.

Tilly, Charles (ed.)
1978 Historical Studies of Changing Fertility. Princeton: Princeton University Press.
1979 "Proletarianization: theory and research." A paper presented at the 1979 meeting of the American Sociological Association, Boston, Mass.

Tilly, Louise A. and Joan W. Scott
1978 Women, Work and Family. New York: Holt, Rinehart and Winston.

van de Walle, Etienne
1976 "Household dynamics in a Belgian village, 1846–1866." Journal of Family History 1:80–94.

Verdon, Michael
1979 "The stem family: toward a general theory." Journal of Interdisciplinary History 10:87–105.

Wallerstein, Immanuel
1974 The Modern World System: Capitalist Agriculture and the Origins of the European World-Economy in the Sixteenth Century. New York: Academic Press.

Wrigley, E. Anthony (ed.)
1972 Nineteenth Century Society: Essays in the Use of Quantitative Methods for the Study of Social Data. Cambridge: Cambridge University Press.
1973 Identifying People in the Past. London: Edward Arnold.

FIREARMS OWNERSHIP FOR SPORT AND PROTECTION: TWO DIVERGENT MODELS*

ALAN J. LIZOTTE
Emory University

DAVID J. BORDUA
University of Illinois, Urbana-Champaign

American Sociological Review 1980, Vol. 45 (April):229–244

A rationale for investigating subcultures of firearms ownership is developed. Two gun-owning populations are investigated by using survey data for the State of Illinois, those who own guns for sport and those who own guns for protection. Models of individual level gun ownership for sport and for protection are constructed and tested. Sporting gun ownership appears to be subcultural. Sporting gun ownership can be predicted by using family socialization variables and indicators of contact among members of the subculture, independent of situational variables. Protective ownership has none of the trappings of a subculture. It does not respond to family socialization and indicators of contact with other people who own guns for protection. Further, there is no indication of a subculture of violence among protective gun owners. Violent attitudes and behavior do not predict protective gun ownership. In fact, a situational variable (county violent crime) was the only predictor of gun ownership for protection. Further, gun ownership for protection and gun ownership for sport were found to be independent events, with no joint probability of occurrence. This suggests that the impetus for a subculture of protective ownership could not be a logical extension of a subculture of firearms ownership for sport.

Introduction

Previous research on firearms ownership displays several shortcomings. Studying firearms ownership as a social problem from the subculture of violence perspective and certain methodological errors in previous research have limited our knowledge of the area. First, firearms ownership is often studied as a social problem, rather than as a behavior pattern. In this vein Newton and Zimring (1968) study *Firearms and Violence in American Life*; Wright and Marston (1975) investigate "The Ownership of the Means of Destruction: Weapons in the United States"; and Kukla (1973) considers *Gun Control*. This literature devotes little or no attention to the sporting use of firearms or to gun ownership for protection in response to crime.

Second, many who study firearms ownership focus on the "subculture of violence" thesis of Wolfgang and Ferracuti (1967). For example, Hackney (1969), Gastil (1971) and Reed (1972) argue that the high incidence of homicide in the South and high rates of southern gun ownership are due to a subculture of violence in that region. They ignore gun ownership for sport and for defense. Others have argued that there could be several uses of firearms. We (Bordua and Lizotte, 1979) suggest that a sporting culture is not only dominant in the gun world, but it is also unrelated to a subculture of violence. We argue that one should study the interrelationships between the legitimate gun culture, a subculture of violence, and ownership for protection.

Third, when gun ownership for protection is studied, analyses are fraught with conceptual and methodological errors. Only a few studies (Bordua and Lizotte, 1979; O'Connor and Lizotte, 1978; Kleck, 1979) have used multivariate methods to ferret out complex relations between different aspects of firearms ownership for protection. Firearms ownership for protection could be a complex phenomenon. Attitudes towards violence, toward blacks, experiences with violence, crime and perceptions of crime, the effectiveness of the criminal justice system and the fear of crime might relate to gun ownership for protection. Poor data, weak

---

* Direct all communications to: Alan J. Lizotte; Department of Sociology; Emory University; Atlanta, GA 30322.

We are indebted to William Form, Kenneth Land, James F. O'Connor, Cynthia Rexroat, and Larry Cohen for comment and criticism on earlier drafts of this paper. We would also like to thank an anonymous reviewer for helpful stylistic and theoretical comments. This research was partially supported by a grant from the Illinois Law Enforcement Commission.

conceptualization of the theory and inadequate methodologies have all compounded the problem.

This research will attempt to address these methodological and conceptual problems in studying patterns of firearms ownership. Particular attention will be devoted to the fear of crime and in turn to protective behavior as a response to that fear. But the sporting use of firearms and its interplay with crime and protection will also be considered.

Sociological Theories of Firearms Ownership

Sociological explanations of firearms ownership are of two basic types. Either they lean heavily toward a subculture of violence explanation, which can be subsumed under a rather specific literature on cultural theories of deviance, or they oppose the subcultural models by introducing "situational" or structural considerations to show that the cultural effects are spurious. Few studies combine cultural and situational factors to predict firearms ownership. Still fewer extricate their models from the rather ideological "deviance" perspective on gun ownership.

Recent proponents of the subculture of violence thesis place heavy emphasis on the function of firearms in the violent subculture. Gastil (1971) gives culture a secondary position to gun ownership in causing interpersonal violence. But Wolfgang (1958) states that high homicide rates are intrinsically related to culture and not necessarily to weaponry.[1]

Hackney (1969) and Reed (1972) also posit that high rates of interpersonal violence in the South are indicative of a southern subculture. However, their theory is closer to Wolfgang's original formulation. They suggest that "this differential in gun ownership is the result of a violent turn of mind rather than the cause of the violence" (Hackney, 1969:518). Thus, Hackney suggests that guns do not cause crime, but that violent individuals buy guns and use them to perpetuate crime.

The approach assumes that cultural factors drive people to own guns and use them violently. Indeed, implicit in the theory is the assumption that all gun owners are potentially violent and that gun ownership is an indicator of this potential. Loftin and Hill (1974), O'Connor and Lizotte (1978), and Bordua and Lizotte (1979) focus on several major inadequacies of the Gastil-Hackney-Reed thesis, while Erlanger (1975) casts doubt on Wolfgang's original formulation of the thesis.

Etzioni and Remp (1973:141) suggest that any realistic study of firearms ownership "requires a detailed examination of the varying ways in which firearms are presently being used in the U.S." More specifically, Cohen (1955:72) states that "a complete theory of subcultural differentiation would state more precisely the conditions under which subcultures emerge or fail to emerge, and would state operations for predicting the content of subcultural solutions." In the same vein Loftin and Hill (1974:724) "show that a more definite assessment of the role of culture and situational variables on interpersonal violence will require specifying a theoretical model which would allow for a full range of cultural and structural variables." In the following section we develop models of firearms ownership for sport and protection. In doing this, we attempt to consider a wide range of cultural and structural variables.

There are three basic reasons—sporting, protection, and criminal—for owning guns. Since this research studies patterns of legal firearms ownership, it will study sporting and protection patterns of ownership.

There are three possible subcultures which this study will consider. The first two are subcultures of sport and protection. The third is a subculture of violence which is centered around protective gun ownership. If gun owning subcultures do exist they arise from situational and structural conditions. These conditions vary with reasons for ownership. That is, owning firearms for sport will result from a different set of situations than owning firearms for protection. Furthermore, cultural support mechanisms operate to legitimize and justify different types of ownership for the individual. Thus, people call upon cultural legitimations as normal responses to situations they face.

---

[1] In their later work Wolfgang and Ferracuti (1967) change the tack to place more emphasis on firearms in the subculture of violence. We prefer to rely more heavily on the earlier empirical work of Wolfgang for substantive support to this argument.

We will accept the existence of a subculture when three conditions are met. First, some subgroup of people must manifest a behavior, norm, or attitude which is different from those of the larger society (Wolfgang and Ferracuti, 1967:105). For example, a member of a sporting gun subculture must, at least, own a gun for sport. Similarly, members of a protective subculture must own guns for protection. Members of a subculture of violence centered on protective ownership must own a gun for protection and exhibit violent attitudes and/or behaviors which would typify this group.

Second, in order for a subculture to exist, there must be some mechanism which will produce an intergenerational transmittance of the behavior, norm or attitude. For example, Gastil (1971), Hackney (1969), and Reed (1972) all place heavy emphasis on the family as the mechanism of socialization into a subculture of violence. The socialization requirement means that a subculture must be tenacious enough to transcend the situational concerns of one generation, and for the behavior to be passed on to the next. Without this capacity, the behavior, norm, or attitude would be ephemeral and not indicative of a subculture (see O'Connor and Lizotte, 1978).

Third, for a subculture to exist there should be some indication of contact among members of the group. For example, Cohen (1955:104) argues that members of a subculture should "gravitate toward one another . . . to establish new norms." As Cohen suggests, this contact among members is necessary not only to keep the behavior, norm, or attitude alive, but also to develop new norms and subcultural responses.

For a subculture to exist, each of these three conditions must hold in the context of situational controls. For example, Wolfgang and Ferracuti (1967:105) state that

> a given conduct, norm, or set of values must function to govern the conduct in a variety of situations in order to classify that norm or value set as a subculturally expected or required response and not merely as a statistical modal reaction.

Hence, in order to test for the existence of subcultures, one must equalize the effects of situation across hypothesized subcultural groups to evaluate differences in conduct or other cultural indicators. That is, a subculture will exist in the formal sense when our three conditions are met, and when they are independent of situation.

Sport Ownership Theory and Model

At the county level we (Bordua and Lizotte, 1979) find inferred socialization factors and situational concerns to be most important in predicting sporting culture. A similar argument is made here at the individual level. Recall that there are three conditions which must be met to support a finding of a subculture of sporting gun ownership. They are: sporting ownership, socialization, and contact among members of the subculture. Bakal (1966), Newton and Zimring (1968), and Bordua and Lizotte (1979) all argue that military service socializes men into gun ownership. They assume that men become familiar with firearms in the military. These men then continue with the orderly use of firearms when they return to civilian life. But not all military personnel carry firearms. Indeed, many are given no small-arms training at all. Hence, a veteran who carried a firearm in the military should be more likely to own a gun as a civilian than a veteran who did not carry a gun. Similarly, those in a combat zone would be better trained in small-arms use than those who were not. Finally, soldiers in combat should be more familiar with firearms than others. Hence, a scale will be developed which reflects involvement with firearms in the military. This scale should predict the legal, culturally ordered use of firearms for sport.

The civilian counterpart of this socialization into gun use is the individual's early training into the proper use of firearms. Therefore, individuals who acquired their first gun early in life should be more likely to have been trained to use a gun. Further, this training should translate into firearms ownership for sport in later life. Similarly, if one's parents owned a gun, socialization should operate which would affect that person's ownership in later life.

Other socialization factors could operate. Situations and an opportunity structure should combine to create a favorable social climate in which the sporting use of firearms can thrive. Here socialization into ownership and ownership itself are quite aboveboard, socially sanctioned and even encouraged. In this vein the sporting use of firearms should be something of a family affair. Hence, if the respondent's parents owned a gun, the respondent should be more likely to own one. Similarly, if the respondent owned a gun at an early age, that person should have a higher probability of continued use in later life.

There are three indicators of contact among members of a subculture of sporting gun use. First, given the social acceptability and even desirability of firearms ownership in this group, other members of the respondent's household might own guns for sport, hunt together, and belong to shooting clubs. Second, communication among members of the subculture could be carried out through more abstract forms. As we point out (Bordua and Lizotte, 1979), those individuals who subscribe to sporting oriented magazines are expected to be more likely to own guns for sport. Magazine subscription would not only indicate communication of subcultural ideas and values, but it would also indicate involvement in the intricacies of the culture beyond shooting and hunting. Third, high hunting rates in counties imply that a subculture of sport could be active, and that there is significant contact among members of the subculture in the county. That is, counties with high hunting rates could supply the cultural backdrop for individuals in the county to be involved in a subculture of sport.

Independently of the subculture, a certain opportunity structure could exist which would support sporting firearms ownership. Since hunting is primarily a rural phenomenon, rural people should be more likely to own guns for sport. Also, those who grew up in a rural environment are expected to carry their background with them, and to be more likely to own guns regardless of the size of place in which they currently live.

Additionally, men should be more likely than women to own guns for sport. A negative relation between education and gun ownership for sport should exist. However, there should be a positive relation between income and sporting ownership. Highly educated individuals are probably more liberal and, therefore, support gun control, while those with high incomes find it easy (monetarily) to buy a gun.

Protection Ownership Theory and Model

Independently of other concerns, individuals could own guns for protection either because they belong to a subculture of protection, or because they are members of a subculture of violence in which protective firearms ownership plays a central role. If a subculture of protection exists, we would expect those who own guns for protection to have contact with other members of the subculture. That is, they should have friends who also own guns for protection. We would also expect a subculture of protection to have a mechanism for transmitting protective gun owning from generation to generation. One possible mode of socialization would be the family; another might operate indirectly through a subculture of sport. For example, if no socialization into protective gun ownership is found, and a subculture of sporting gun ownership exists, it could indicate considerable overlap between sporting and protective ownership among individuals. This overlap would imply that there is an indirect effect of socialization on protective gun ownership through sporting gun ownership. Of course, these relations should be independent of the current situation.

A subculture of violence which centers on protective ownership should meet the conditions stated for a subculture of protection. In addition members of the subculture of violence should exhibit violent attitudes and behavior.

Of course many other factors, independent of either subcultural response, could predict gun ownership for protection. The structure of the model to be tested is as follows: Fear of crime (see McIntre, 1967 and Furstenberg, 1971) could be the primary motive for home defense, and, hence, for firearms ownership for protection (Feagin, 1970). There are six basic factors which could motivate fear of crime in an individual. They are: crime, experiences with violence and victimization, racial attitudes and violent attitudes, the perceived crime rate, the perceived effectiveness of the police and courts, and the proximity of blacks. Also, social characteristics of individuals such as income, race, and sex could operate along with situational and attitudinal variables.

The basic relations are as follows: The crime rate in a county could affect the individual's perceptions of that county's crime rate, perceptions of the effectiveness of the police and courts, and the individual's personal experiences with violence or victimization. Additionally, the crime rate might be a good predictor of the respondent's knowledge of others who own a gun for protection, home defense measures taken by the respondent, and gun ownership for protection on the part of the respondent. People need not fear crime in order to own a gun for protection. A milieu of crime might make the individual less fearful but more willing to take defensive measures. Alternatively, some people might *not* be fearful *because* they own guns for protection.

Specifically, when the crime rate in a county is high, the respondent could know other people who own guns for protection. Respondents in high crime counties might perceive the courts and police as ineffective. Where crime is high, individuals should perceive a high crime rate. Finally, high crime should predict the respondent's experiences with violence and victimization. With these effects and the effects of social characteristics controlled, only a trivial relation between actual crime and the fear of crime should be found.

The crime rate is not as important as the perceived effectiveness of the criminal justice system in determining fear of crime (Reiss and Bordua, 1967:28). The same argument, independent of other situational and attitudinal factors, is made for relations between the crime rate, home defense, and, ultimately, owning a gun for protection. The policy implications are intriguing. Even in the context of a high crime rate, large-scale reductions in gun ownership for protection could be brought about by convincing individuals that the criminal justice system can and will protect them.

Individuals could see a trade-off between the ability of the police and courts to protect them and defensive behavior to protect themselves (Feagin, 1970). Even when the objective reality of crime is put aside, those people who see the police and courts as being ineffective in protecting them from crime might take measures to protect themselves (Reiss and Bordua, 1967).

Owning a gun for protection could be a consequence of several other factors. It could be the logical extension of a general home defense orientation. One mode of behavior for individuals who are vulnerable to crime is to increase their personal security, which leads them to a general home defense orientation and the acquisition of a gun. Buying special locks for one's home, obtaining a guard dog, installing burglar alarms, and the like, are of the very same variety of protective response as buying a gun. Similarly, being female, living alone, having a low income, and living in a large city are all factors which relate to the individual's defense posture with respect to crime.

We (Bordua and Lizotte, 1979) find some evidence at the county level to suggest that individuals use the proximity of a young black population as an indicator of crime. If this is true, the proximity of blacks to one's neighborhood could lead to certain racial attitudes that would encourage a home defense orientation in general and owning a gun for protection from blacks in specific. The relations between the proximity of blacks, the fear of crime, and defensive behavior could hold for both blacks and whites. Regardless of their race, people associate black neighborhoods with crime. This would lead to fear and defensive behavior (Furstenberg, 1971).

Data and Methods

The data for this study consist of an initial sample of 764 telephone interviews of heads of household in the state of Illinois. The data were collected in the spring of 1977 by the Survey Research Laboratory (SRL) at the University of Illinois. In downstate Illinois the sample of heads of household was randomly selected from telephone directories. Random digit dialing was used in Chicago.

The overriding methodological problem in the analysis is related to the dichotomous dependent variables, firearms ownership for sport and firearms ownership for protection (Lizotte, 1979). When analyzing the variance in a dichotomous variable using ordinary least squares (OLS), several assumptions are violated (Theil, 1971; Kmenta, 1971; Nerlove and Press, 1973; Palmer and Carlson, 1976). Many alternative methods for analyzing dummy dependent variables have been suggested. These include weighted least squares, Probit, Logit, Logistic models for categorical data, and Modified Logistic Response Models.

While being directly analogous to the Goodman system, the Nerlove-Press (1973) modified logistic response model has certain advantages. It allows one to consider the effect of continuous, as well as polytomous, exogenous variables. It also permits easy computation of MLE conditional probability estimates for several qualitative polytomous endogenous variables along with the direct effects of either continuous or polytomous exogenous variables.

Of the techniques discussed, the Nerlove-Press technique is clearly best suited to this study; OLS, WLS, and WLS with grouping simply do not adequately deal with the problems of heteroscedasticity *and* the nonnormal error term encountered in dummy variable analysis. Traditional Logit and Probit require large samples to asymptotically approach MLE. Since this analysis is limited to 764 cases, it would not seem wise to use these techniques. Additionally, the continuous nature of the Probit estimates makes interpretation difficult. WLS with grouping and Logit are also undesirable because they require grouping data on x variables, which has the effect of constricting the number of variables or categories of variables in small sample sizes. The Goodman log-linear system (Goodman, 1972a; 1972b) requires all variables to be polytomous, which destroys the true nature of continuous data. The Nerlove-Press model suffers from none of these faults. Further, the equation estimated by the Nerlove-Press system,

$$p(Y) = \frac{1}{1 + e^{-(a + bx_1 + \dots + bx_G)}}, \qquad (1)$$

provides a direct OLS analogy.

The variables were coded in the following way. Exogenous variables common to both models are shown in Table 1. Variable names are listed in the left-hand portion of the table, while descriptions of those variables are shown at the right. For example, the table shows that sex is coded 0, for female respondents, and 1, for male respondents. Sex, race, age, income, education and size are variables which appear in both the sporting and protection models. Their coding is straightforward with one exception. The Nerlove-Press program finds it difficult to converge on an estimate of the asymptotic variance-covariance matrix when exogenous variables are allowed to take on values above 9.99. To facilitate computational ease, variables with values above 9.99 have been scaled accordingly. For example, age could take on values up to 99 years. Dividing age by ten yields a maximum value of 9.9. This preserves the continuous nature of the variable while allowing for computational ease. Education has been divided by ten, and size has been divided by one million.

Variables particular to the model of sporting gun ownership are shown in the second part of Table 1. Only the coding of those variables which need more elaboration than given in the table will be discussed. Region 16 is an indicator of childhood residence in the southern part of the United States. Age at first gun is the re-

Table 1. Description of Variables Common to Both the Sporting and Defensive Models of Gun Ownership, and Variables in the Sporting Model

Variable Name	Description of Variables
Both Models	
Sex	0=Female, 1=Male $\bar{x}$ = .43, S.D. = .50
Race	0=Nonwhite, 1=White $\bar{x}$ = .88, S.D. = .32
Age	years/10 $\bar{x}$ = 4.48, S.D. = 1.70
Income	1<\$5,000; 2=\$5,001–\$10,000; 3=\$10,001–\$15,000; 4=\$15,001–\$20,000; 5=\$20,001–\$25,000; 6=\$25,001–\$30,000; 7>\$30,000 $\bar{x}$ = 3.69, S.D. = 1.77
Education	years through 16; 17=some graduate school, 18=graduate degree; years/10 $\bar{x}$ = 1.29, S.D. = .27
Size	size of place; population/1,000,000 $\bar{x}$ = .80, S.D. = 1.36
Sport Models	
Size 16	size of place when 16; 1=farm, 2=country, 3=suburb, 4=<50,000, 5=50,001–250,000, 6>250,000 $\bar{x}$ = 3.89, S.D. = 1.78
Region 16	0=nonsouth, 1=south, $\bar{x}$ = .045, S.D. = .207
County Hunting.	hunting licenses per population in county of residence 1975 $\bar{x}$ = .005, S.D. = .007
Parents Owned	parents owned a gun; 0=no, 1=yes; If don't know, value is no $\bar{x}$ = .536, S.D. = .499
Age at First Gun	Years/10, if never owned, value is current age $\bar{x}$ = 3.72, S.D. = 1.94
Trained	Trained to use a gun; 0=no, 1=yes $\bar{x}$ = .450, S.D. = .498
Veteran	military gun use; 0=not veteran—not currently in military, 1=veteran or in military, 2=required to carry a firearm, 3=in combat zone, 4= in combat; respondent or respondent's husband $\bar{x}$ = 1.01, S.D. = 1.45
Hunt	members of the family hunt or shoot together; 0=no, 1=yes $\bar{x}$ = .094, S.D. = .292
Sportmags	Subscribes to *Sports Afield, Field and Stream, Outdoor Life,* or any other magazine especially directed to firearms' owners; number of different magazines $\bar{x}$ = .205, S.D. = .604
Gun for Sport	0=no, 1=yes when final endogenous; −1=no, 1=yes when conditioning endogenous $\bar{x}$ = .179, S.D. = .384

spondent's age when the first gun was owned. If the respondent has never owned a gun, current age is coded. The variable is divided by ten.

Veteran reflects military gun use. Those who have never been in the military get 0 on the variable. All respondents were asked about military service. If the respondent was a married woman and had not been in the military, the questions were asked about her husband. This assumes that married men who had not been in the military are unlikely to have wives who were in the service. It also assumes that veterans' wives are similar to veterans on related variables.

The endogenous variable in the sporting model is gun ownership for sport. It is coded 0, for no sporting gun, and 1, for owning a sporting gun. Respondents were asked how many rifles, shotguns, and pistols they owned. Then each was asked, "For what purpose do you own the firearm(s)?" Respondents were asked this question repeatedly until no additional reason was volunteered. Hence, a respondent could answer both protection and sport for only one gun. To allow for symmetrical relations in the joint probability model, gun for sport is coded −1 for no gun and 1 for gun ownership for sport when it is a conditioning endogenous variable in the gun ownership for protection equation.

Table 2 contains descriptions of variables appearing only in the protection model. CCrime is the score of the respondent's county of residence on a violent crime factor variable (see Bordua et al., 1979). The factor summarizes rates of armed robbery, aggravated assault, and murder for the period 1972 to 1976.

Copcor indicates the respondent's perception of the ineffectiveness of the police and courts in dealing with crime. It is an additive index of two items. Table 2 shows the construction of the index. Scores on the index were divided by ten. Vioexp, Victim, Fear, Racist, Homedef, Vatt1, and Vatt2 were all constructed as additive indices.

The variables Fear and Racist are scaled to yield values under 9.99.

About 18% of the respondents own a gun, whether it be a long gun or a handgun for sport, while about 6.2% own a gun for

Table 2. Description of Variables in the Gun Ownership for Protection Model

Variable Name	Description
Live Alone	0=no, 1=yes $\bar{x}$ = .167, S.D. = .373.
CCrime	Score on violent crime factor for respondent's county of residence $\bar{x}$=.281, S.D.=2.62.
Proximity of Blacks	Blacks in neighborhood; 0=no, 1=yes, nonwhites coded as 1, don't know are assigned 0 $\bar{x}$=.733, S.D.=.827.
Friend	Know men and/or women who own guns for protection; 0=no, 1=yes, don't know assigned to 0 $\bar{x}$=.427 S.D.=.495.
Copcor	Police and courts ineffective; two items combined; people must be prepared to defend their homes or can police do it: 1=police defend, 5=defend self; how important is increasing police patrols in reducing crime: 1=not at all important, 2=not very important, 3=somewhat important, 4=very important; (defend+police patrol)/10, $\bar{x}$=.743, S.D.=.226.
Vioexp	Violent experiences; four variables combined; struck anyone in anger 0=no, 1=yes, had to carry a gun for protection 0=no, 1=yes, had to use a gun for protection 0=no, 1=yes, ever felt a serious physical threat from another person 0=no, 1=yes; (struck+carry, gun+use, gun+physical threat) $\bar{x}$=.647, S.D.=.838.
Victim	Victimized in crime; sum of five variables: robbery 0=no, 1=yes, theft 0=no, 1=yes, breaking and entering 0=no, 1=yes, assault and battery with a fire arm 0=no, 1=yes, assault and battery (fists) 0=no, 1=yes, $\bar{x}$=.996, S.D.=.131
Perceived Crime	Perceived crime rate in county relative to other counties; 5=much higher, 4=slightly higher, 3=about the same, 2=slightly lower, 1=much lower, don't know assigned to about the same $\bar{x}$=.316, S.D.=1.23.
Fear	Fear of crime; sum of three items/10; afraid to walk alone at night within a mile of home 0=no, 5=yes, afraid to be at home at night, 0=no, 5=yes, how important is crime to you 1=not at all important, 2=not very important, 4=somewhat important, 5=very important, don't know is assigned to 3, low not fearful, high indicates fear $\bar{x}$=.765, S.D.=.375.
Vatt1	Violent attitudes index one; sum of five items—approve of a man punching a stranger if the stranger was in a protest march 0=no, 1=yes, was drunk bumped into wife 0=no, 1=yes, hit man's child after child damaged car 0=no, 1=yes, approve of policeman hitting man if man said vulgar and obscene things to policeman 0=no, 1=yes, was a suspect in murder case 0=no, 1=yes, high scores indicate violent attitudes $\bar{x}$=.975, S.D.=1.00
Vatt2	Violent attitudes index two; sum of four items—approve of man punching a stranger if the stranger was beating up a woman 0=no, 1=yes, had broken into man's home 0=no, 1=yes, approve of policeman hitting man if man was attempting to escape from custody 0=no, 1=yes, attacking the policeman with fists 0=no, 1=yes, high scores indicate violent attitudes $\bar{x}$=3.26, S.D.=1.05.
Racist	Racial attitudes; sum of two items/10: laws against black-white marriages okay 1=no, 5=yes, black shouldn't push where they are not wanted 1=strongly disagree, 2=disagree, 4=agree, 5=strongly agree, don't know coded as 3, high scores indicate antiblack attitudes $\bar{x}$=.491, S.D.=.258.
Homedef	Home defense measures, sum of three items: taken special measures such as special locks, burglar alarms, guard dog? 0=no, 1=yes, security was important in choosing home 0=no, 1=yes, keep doors locked in the daytime 0=no, 1=yes, high scores indicate more defense measures $\bar{x}$=1.54, S.D.=.995.
Gun for Protection	0=no, 1=yes when final endogenous −1=no, 1=yes, when conditioning endogenous $\bar{x}$=.062, S.D.=.241.

protection. It is important to note that the gun variables do not measure household gun ownership. They measure the respondent's gun ownership. It is difficult to attach attitudes and motivations to households possessing guns. Attaching the respondent's attitudes or motivations for ownership to the household results in reification and overgeneralization. On the other hand, individuals' motivations for ownership and attitudes can be tied to individuals' behavior (gun ownership) without undue inference.

Analysis and Results: The Sporting Model

In the sporting model, exogenous variables with respect to gun ownership are: Age at first gun, Veteran, Sportmags, Trained to use a gun, and Hunt. Age at first gun, Veteran and Sportmags are estimated using OLS. The remaining equations are estimated using the Nerlove-Press maximum-likelihood logistic response procedure.

Figure 1 shows the final estimated path model for gun ownership for sport. Insig-

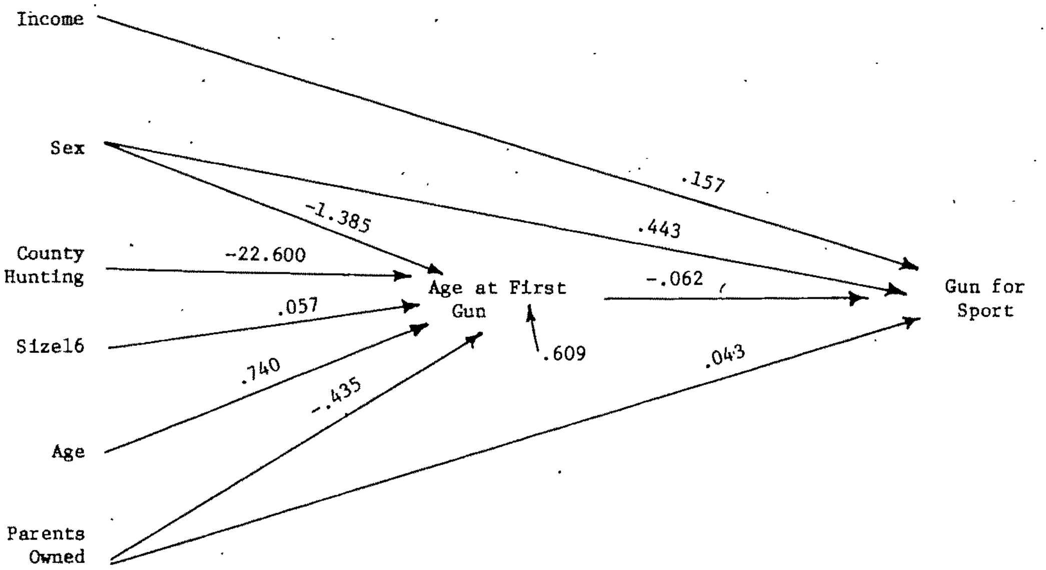


Figure 1. A Path Model of Firearms Ownership for Sport, OLS and Logistic Estimation Procedures Are Used Where Appropriate

nificant variables have been dropped from the model. Income, Sex, Parent's gun ownership, and Age at first gun are the only significant determinants of gun ownership for sport. Sex, Age, County hunting, Size 16, and Parent's gun ownership also have indirect effects on sporting gun ownership. Sex, Parent's gun ownership and County hunting all have positive indirect effects on gun ownership for sport through age at first gun. That is, males, respondents with gun-owning parents, and respondents who live in high hunting counties all tend to be sporting gun owners. Respondents who lived in small towns when 16 years old tend to have acquired their first gun at an early age. This translates into a high probability of gun ownership for sport in later life.

Table 3 shows OLS and Logistic parameter estimates for models of age at first gun and gun ownership for sport. Relations which were not significant at the .05 level of a one-tailed test have been dropped from the model. Equations were reestimated without insignificant relations. Either normal or asymptotic standard errors are in parentheses. The row in the table labeled λ is actually the likelihood ratio statistic:

$$\lambda = -2(\mathrm{LOG}(L(0)) - \mathrm{LOG}(L(\mathrm{OPT}))). \quad (2)$$

The statistic is distributed as chi-square with D.F. degrees of freedom. Significant values of λ indicate that the obtained likelihood function (L(OPT)) is significantly different from zero (L(0)). Hence, the equation is significantly better than an equation with zeros for parameter estimates. All equations reported for both the sporting and protection models are significant.

Sex, Size 16, Age, County hunting, and Parent's gun ownership are all good predictors of age at first gun. Males are younger when acquiring their first gun. The larger the city in which one grew up, the older the age at first gun. Where the county hunting rate is high, age at first gun is low. Finally, if the respondent's parents owned a gun, the respondent is likely to have acquired the first gun at an early age. In short, small town males whose parents owned a gun and who now live in high hunting counties are most likely to have acquired a gun at an early age. The equation explains about 63% of the variance in age at first gun.

Gun ownership for sport is the result of financial status, sex, and the early socialization into gun use. Men are more likely to have acquired a gun at an early age. It is not surprising that they are also more likely than women to own guns for sport.

Table 3. Ordinary Least-Squares Equation Predicting Age at First Gun, Standard Errors Are in Parentheses, and Logistic Equation Predicting Gun Ownership for Sport, Asymptotic Standard Errors Are in Parentheses

	OLS		Logistic
	Age at First Gun		Gun Ownership for Sport
Constant	1.1315		−1.390
	(.20561)		(.2204)
Sex	−1.3851		.4430
	(.09089)		(.1065)
Parents Owned	−.43468		.0425*
	(.09530)		(.0308)
Size 16	.05652		
	(.02726)		
Age	.74049		
	(.02675)		
County Hunting	−22.6001		
	(6.8249)		
Income			.1569
			(.0373)
Age at First Gun			−.0615
			(.0207)
R^2	.63	$-2 \log \lambda$	371.068
			D.F. 5
			sig. yes

* Significant at the .1 level one-tailed.

Guns and the sporting activity associated with them are expensive. Higher income respondents find it easier to own a gun for sport. The county hunting rate has dropped out of the picture. Individuals respond more to parent's gun ownership and age at first gun. Respondents tend to own guns when their parents owned guns and when they were young first-time owners. These findings suggest that, apart from the expense of gun ownership for sport, ownership is largely a function of early socialization into gun use.

Parent's ownership is central to relations in the sporting model. This measure of socialization into sporting has a direct effect on age at first gun. It should be noted that the county hunting rate has no direct effects on measures of individual contact among subcultural members such as family gun use and sporting magazines. Its effects are limited to socialization (age at first gun).

This suggests some support for the existence of a subculture of sporting gun ownership. That is, each of the three conditions indicating a subculture of sporting ownership have been met. Significant numbers of people exhibit a behavior which is different from the larger culture—they own guns for sport.[2] Second, we have located the mechanism of socialization into the subculture. The intergenerational transmittance is carried out via family socialization through parent's gun ownership and age at first gun. Third, there are indirect effects of county hunting activity on sporting gun ownership. This is a mild indication of cultural contact in the respondent's area of residence. The subculture appears to be largely, but not exclusively, male. Further, other situational and demographic variables do not appear to be particularly strong in predicting firearms ownership for sport. Neither are they effective in neutralizing cultural variables.

Not all of our cultural variables were effective in predicting firearms ownership for sport. There is no evidence suggesting individuals are socialized into a sporting subculture in the military. Additionally, two indicators of contact among members of the subculture (family hunting and magazine subscriptions) were not effective in predicting sporting gun ownership when parent's ownership and age at first gun were controlled.

---

[2] We do not mean to imply that small numbers of people own guns. While about 20% of respondents over age 18 own a gun, about 50% of families own at least one gun.

Analysis and Results: The Protection Model

We now turn to estimating the model of firearms ownership for protection. Table 4 shows OLS equations for variables in the model which are endogenous with respect to gun ownership for protection. Relations which are insignificant at the .05 level have been eliminated. Equations were reestimated without insignificant variables. The table includes the Logistic equation for gun ownership for protection.

Violent crime in the county is the only significant predictor of owning a gun for protection. Similarly, county crime is the only predictor of friend's owning a gun for protection. This is a good deal simpler than the relations hypothesized. Apparently, in this study, people's defensive ownership of guns is totally a function of violent crime in the area. It is not an extension of a general home defense orientation or a product of any of the other variables in the model, such as violent attitudes or racism.

To this point, there is no evidence of a subculture of defensive gun ownership. While we can locate a group of protective gun owners, there is no indication of contact among them. That is, having friends who own guns for protection does not predict the respondent's protective ownership. Additionally, there is no evidence of a subculture of violence centered on protective ownership. Violent attitudes (Vatt1 and Vatt2) and violent experiences Vioexp) do not predict gun ownership for protection.

Table 4. Ordinary Least-Squares Equations Predicting Copcor, Victim, Perceived Crime, Fear and Homedef, Standard Errors Are in Parentheses, and a Logistic Equation Predicting Gun Ownership for Protection, Asymptotic Standard Errors Are in Parentheses

	Copcor	Victim	PCrime	Fear	Homedef	Gun Ownership for Protection	
Intercept	.9539 (.0470)	1.816 (.1921)	2.483 (.0707)	.7379 (.0812)	1.544 (.2222)	−1.723 (.1341)	
Sex		.4972 (.0937)		−.2853 (.0165)			
Income				−.0190 (.0074)			
Education	−.1189 (.0310)			−.0985 (.0489)	−.3381 (.1218)		
Race	−.0569 (.0259)	−.5681 (.1453)			−.2685 (.1218)		
Age		−.1456 (.0279)					
Live Alone	−.0450 (.0224)						
Size				.0338 (.0095)			
Proximity of Blacks		.1605 (.0568)	.1248 (.0519)	.0533 (.0154)			
County Crime			.1675 (.0164)		.0572 (.0157)	.1409 (.04323)	
Victim			.1202 (.0325)	.0219 (.0098)	.0348 (.0259)		
Copcor				.1926 (.0548)			
Perceived Crime				.0366 (.0105)			
Vioexp					.5730		
Fear					(.0915)		
R^2	.032	.123	.181	.311	.212	−2 Logλ	661.208
						D.F.	2
						Sig.	yes

A question remains about the effects of family socialization on protective ownership. Two possibilities exist. First, family socialization variables could have direct effects on protective gun ownership. Second, family socialization variables could have indirect effects on protective ownership through sporting ownership. If the latter is the case, a subculture of protection could be accepted on the grounds that both socialization and contact among members occurs through membership in the subculture of sporting gun use. Of course, no such argument could be made for the existence of a subculture of violence centered on protective gun ownership, since violent attitudes and violent experiences do not predict protective ownership.

In essence, we want to evaluate whether or not there is a joint probability of occurrence between firearms ownership for sport and firearms ownership for protection. One way to attack this problem is along the lines suggested by Nerlove and Press (1973). That is, by applying maximum-likelihood methods to the univariate dichotomous logistic response function in symmetric form to approximate full information, maximum-likelihood estimates of the joint probability function.

Table 5 shows the equations for gun for sport and gun for protection estimated in this way. All variables which were significant in either equation individually are included in the conditional estimation equations. For symmetry, the conditioning endogenous variable is coded -1, for no gun, and 1, for gun. For example, the gun for protection equation answers the ques-

Table 5. Logistic Maximum-Likelihood Equations of Joint Probabilities (Conditional Estimators) for Owning a Gun for Sport and Owning a Gun for Protection. Asymptotic Standard Errors Are in Parentheses

Predictor	Gun for Protection	Gun for Sport
Constant	−1.796	−1.458
	(.2702)	(.1917)
Sex	.1866	.5117
	(.1264)	(.1116)
Income	.0367	.1506
	(.0282)	(.0357)
Parents Owned	.0038	.0396
	(.0228)	(.0267)
Age at First Gun	−.0274	−.0329
	(.0230)	(.0231)
County Crime	.0057	−.0328
	(.0217)	(.0226)
Sport	.0147	——
	(.0245)	
Protection	——	.0100
		(.0243)
$-2 \log \lambda$	657.840	366.094
D.F.	7	7

could not argue that contact among members of a protective subculture and socialization into the subculture are provided by a subculture of sport. Additionally, Table 5 shows that family socialization variables do not have direct effects on gun ownership for protection. Hence, protective ownership meets none of the conditions necessary for a subculture of protective ownership to exist. In fact protective ownership responds only to a situational variable—the county crime rate. The conditional model is abandoned for the simple independent equations of Tables 3 and 4.[3]

---

[3] There is a final matter of concern. One might be tempted to argue that some of the respondents in the

Other Findings in the Protection Model

While only one variable was significant in predicting protective firearms ownership, some consideration should be given to the pattern of relations among the remaining variables in the model.

The perceived ineffectiveness of the police and courts (Copcor) is determined by three variables. They are: whether or not the respondent lives alone, the respondent's education, and race. Blacks and those with low education see the police and courts as being ineffective in dealing with crime. Those who live alone tend to think that the police and courts are effective in dealing with crime.

The respondent's perception of the level of crime is not solely a function of the acutal crime rate in the county. Perceived crime also responds to the proximity of blacks and to victimization. The data suggest that the presence of blacks is used by people to indicate the level of crime. Similarly, respondents who have been victimized perceive more crime in their county of residence.

The data suggest that the fear of crime is founded in reality. It responds to eight variables. They are Sex, Income, Education, Size, Proximity of blacks, Victimization, Copcor, and Perceived crime. Women are more likely to be afraid of crime than men. Those with low income and education are also more likely to fear crime. City life leads to a fear of crime, as does having blacks in one's neighborhood. Apart from this, those who have been victimized, those who see the police and courts as ineffective and those who perceived high crime in their county are more afraid of crime.

People defend their homes when crime becomes a threat. Education, Race, Size, County crime, Victim, and the Fear of crime are all significant determinants of home defense. Poorly educated people and blacks tend to have a strong home defense. Those who live in larger cities tend to be more concerned with home defense. Similarly, the higher the county violent crime rate, the greater the home defense. If the respondent has been victimized, home defense increases. The greater the fear of crime, the greater the home defense. Figure 2 shows the estimated path model. Vioexp, Vatt1, Vatt2, and Racist had no significant direct or indirect effects on home defense and gun for protection. Therefore, they have been dropped from the model.

Figure 2 shows that there are negative direct and indirect effects of education on home defense measures. These indirect effects pass through Copcor and Fear. That is, highly educated people see the police and courts as effective, and, hence, they are less afraid of crime and less likely to take home defense measures. Presumably, those with high income can afford to live in neighborhoods where they have no reason to fear crime. Hence, they find no need for serious home defense measures.

The same general logic is true for race. Blacks think that the police and courts are ineffective in dealing with crime, and blacks are more likely to be victimized in crime. This leads to a fear of crime, and, subsequently, to a home defense orientation.

Because women fear crime they are more likely to take home defense measures. On the other hand, men are more likely to have been victimized and, therefore, to take home defense measures. The indirect effect for women is $-.163$, while for men it is .016. Adding the indirect effects of sex on home defense,

$$.016 - .163 = -.147 \tag{3}$$

shows that women are more likely to take home defense measures.[4]

The young are more likely to have been victimized, and, therefore, to take defensive measures. The proximity of blacks has indirect effects on home defense

---

criminal, the .953 estimate is probably high. One gun criminal lying about gun use probably would not seriously affect the parameter estimates in the models.

[4] Since the modified logistic response model differs in functional form from OLS, extreme caution should be used in evaluating indirect effects between logistic and OLS equations. In this example, both are OLS equations. However, when estimation is mixed, the precise nature of indirect effects depends on the predicted values (P(x)) for both equations, and not solely on the value of coefficients.

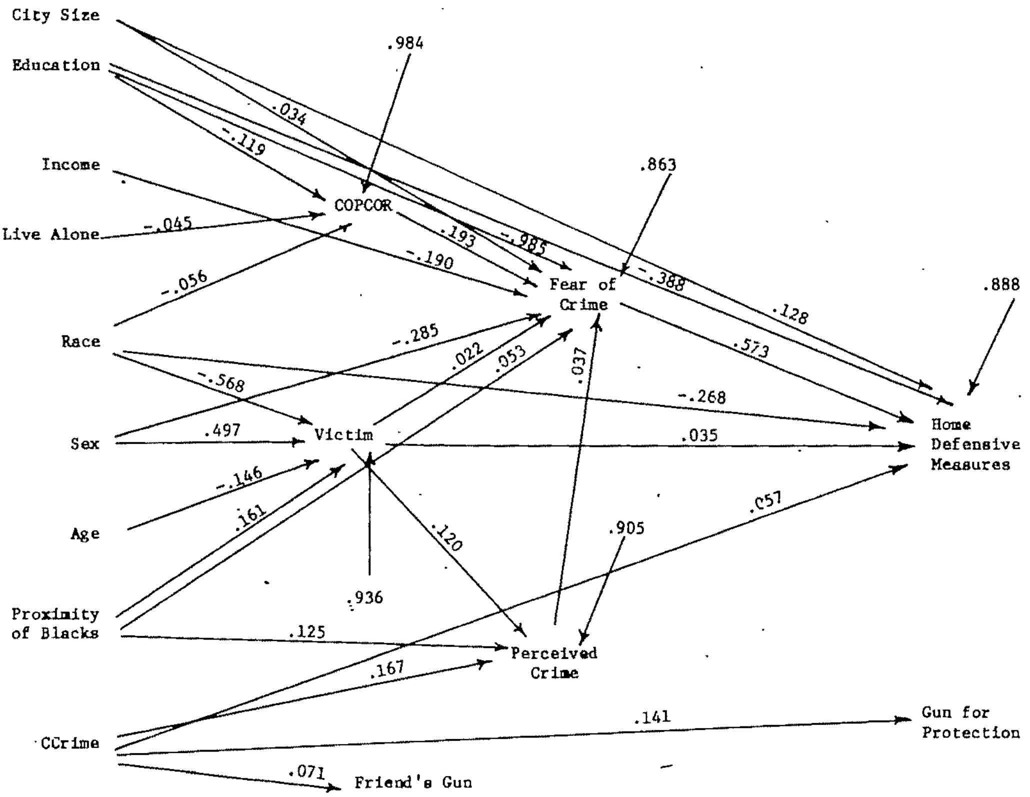


Figure 2. A Path Model of Firearms Ownership for Protection, OLS and Logistic Estimation Procedures Are Used Where Appropriate

through victimization, perceived crime, and the fear of crime.

Having blacks in one's neighborhood leads to victimization and high perceived crime. This results in the fear of crime and a home defense orientation.

Summary

There is no doubt that socialization plays a strong role in the sporting use of guns. Parent's gun ownership and age at first gun were both strong predictors of owning a sporting gun. Further, one indicator of contact among members of the subculture did relate to gun ownership for sport. That is, the county hunting rate has indirect effects through age at first gun on firearms ownership for sport. These findings support the notion of a subculture of sporting gun use.

Not all of the subcultural indicators were significant predictors of firearms ownership for sport. Several studies have found a strong relation between veteran status and firearms ownership (see Newton and Zimring, 1968). This relation was proven spurious. Veterans are no more likely than others to own guns because of their military training. Similarly, neither family hunting nor sporting magazine subscription are good measures of contact among individuals owning firearms for sport.

Gun ownership for protection is a more straightforward phenomenon than hypothesized. Gun ownership for protection is only related to situation. That is, violent crime in the county of residence is the only significant predictor of gun ownership for protection. Firearms ownership for protection had none of the trappings of a subculture. We found no evidence of socialization into the subculture, and, hence, no evidence that the behavior is transmitted intergenerationally. Similarly, we found no evidence of contact among indivudals who own guns for protection. Additionally, we found no indication of overlap between a subculture of sporting gun use and ownership for protection.

Since violent attitudes and violent experiences did not predict gun ownership for protection, we must also reject the notion of a subculture of violence centered around protective ownership. We do not mean to imply that this is conclusive evidence that protective gun-owning subcultures do not exist. Other measures of socialization into the subcultures, of contact among members of the subculture, and of violent attitudes and experiences may be good predictors. However, this study provides no such support. A subculture of protective ownership was not found to be independent of situation.

Nor do we mean to imply that a myriad of situational and demographic variables were capable of predicting protective gun ownership. Neither an unrealistic fear of crime nor the perceived effectiveness of the courts and police affected protective ownership. This implies that convincing people that they are safe and that the courts and police will protect them would not reduce gun ownership for protection. Protective gun ownership was not found to be a logical extension of a home defense orientation. There is no sex difference in the probability of ownership for protection. Unlike sporting ownership, women are as likely as men to own a gun for protection. Racist attitudes did not predict owning a gun for protection. Similarly, age, size of place, income, living alone, and the proximity of blacks were all insignificant in predicting gun ownership for protection. Victimization did not play a role in owning a gun for protection. Additionally, the perceived crime rate is of little consequence. The only predictor of owning a gun for protection is living in the context of high crime. The policy implication is obvious. In order to reduce the demand for protective guns we must reduce the crime rate.

REFERENCES

Bakal, Carl
1966 The Right to Bear Arms. New York: McGraw-Hill.

Bordua, David J. and Alan J. Lizotte
1979 "A subcultural model of legal firearms ownership in Illinois." Law and Public Policy Quarterly 2:147–75.

Bordua, David J., Alan J. Lizotte, Gary Kleck, with Van Cagle
1979 Patterns of Firearms Ownership and Regulation in Illinois. A report to the Illinois Law Enforcement Commission.

Cohen, Albert K.
1955 Delinquent Boys: The Culture of Gangs. Glencoe: Free Press. Reprinted in David O. Arnold (ed.), Subcultures. Berkeley: Glendessary Press.

Erlanger, Howard S.
1975 "Is there a 'subculture of violence' in the South?" Journal of Criminal Law and Criminology 66:483–90.

Etzioni, Amitai and Richard Remp
1973 Technology Shortcuts to Social Change. New York: Russell Sage Foundation.

Feagin, Joe R.
1970 "Home defense and the police: black and white perspectives." American Behavioral Scientist 13:797–814.

Furstenberg, Frank F.
1971 "Public reactions to crime in the streets." The American Scholar 40:601–10.

Gastil, Raymond
1971 "Homicide and a regional culture of violence." American Sociological Review 36:412–27.

Goodman, Leo A.
1972a "A modified multiple regression approach to the analysis of dichotomous variables." American Sociological Review 37:28–46.
1972b "A general model for the analysis of surveys." American Journal of Sociology 72:1035–86.

Hackney, Sheldon
1969 "Southern violence." Pp. 505–27 in Hugh D. Graham and Ted R. Gurr (eds.), The History of Violence in America. New York: Praeger.

Kleck, Gary
1979 "Capital punishment, gun ownership, and homicide." American Journal of Sociology 84:882–910.

Kmenta, Jan
1971 The Elements of Econometrics. New York: Macmillan.

Kukla, Robert J.
1973 Gun Control. New York: Stackpole Books.

Lizotte, Alan J.
1979 "Methodology for the individual level analysis." In David J. Bordua, Alan J. Lizotte, and Gary Kleck, with Van Cagle (eds.), Patterns of Firearms Ownership and Regulation in Illinois. A report to the Illinois Law Enforcement Commission, Appendix B.

Loftin, Colin and Robert H. Hill
1974 "Regional subculture and homicide: an examination of the Gastil-Hackney thesis." American Sociological Review 39:714–24.

McIntre, J.
1967 "Public attitudes toward crime and law enforcement." The Annals of the American Academy of Political and Social Sciences 374:34–46.

Nerlove, Marc and S. James Press
1973 Univariate and Multivariate Log-Linear and Logistic Models. Santa Monica: Rand.
Newton, George D. and Franklin E. Zimring
1968 Firearms and Violence in American Life. Washington, D.C.: U.S. Government Printing Office.
O'Connor, James F. and Alan J. Lizotte
1978 "The 'southern subculture of violence' thesis and patterns of gun ownership." Social Problems 25:420–9.
Palmer, Jan and Paul Carlson
1976 "Problems with the use of regression analysis in prediction studies." Journal of Research in Crime and Delinquency 13:64–81.
Reed, John Sheldon
1972 The Enduring South. Lexington, Mass.: D. C. Heath.
Reiss, Albert and David J. Bordua
1967 "Environment and organization: a perspective on the police." Pp. 25–55 in David J. Bordua (ed.), The Police: Six Sociological Essays. New York: Wiley.
State of Illinois
1976 Crime in Illinois. Crime Studies Section, Bureau of Identification, Department of Law Enforcement.
Theil, Henri
1971 Principles of Econometrics. New York: Wiley.
Wolfgang, Marvin E.
1958 Patterns in Criminal Homicide. Philadelphia: University of Pennsylvania Press.
Wolfgang, Marvin E. and Franco Ferracuti
1967 The Subculture of Violence. New York: Tavistock.
Wright, James D. and Linda L. Marston
1975 "The ownership of the means of destruction: weapons in the United States." Social Problems 23:93–107.

THE TACIT CONVENTIONS OF THE MODERNITY SCHOOL: AN ANALYSIS OF KEY ASSUMPTIONS*

WILLIAM C. RAU
State University of New York, Albany

American Sociological Review 1980, Vol. 45 (April):244–260

This paper examines a number of conventions which guide current research into psychological modernity. The key assumption is unidimensionality, an assumption which underlies recent investigations into the convergent and discriminant validity of various modernity scales. I show that this assumption takes for granted what needs to be proven and, therefore, violates the null hypothesis axiom. Using data from a five nation study of individual modernity, I statistically examine this assumption and demonstrate its inappropriateness to modernity research. I close with an interpretation of patterns of modernity in the five nations. These patterns have been derived from factor solutions that meet statistical tests of dimensionality. The results suggest that existing modernity data should be reanalyzed.

> You may . . . find much of the thinking of an individual, a school, or even a generation, dominated and determined by one or another turn of reasoning, trick of logic, methodological assumption, which if explicit would amount to a large and important and perhaps highly debatable proposition in logic or metaphysics. (Arthur O. Lovejoy, 1936:10)

INTRODUCTION

Every paradigm is rooted in a number of tacit conventions and shared convictions that constitute unarticulated boundaries to research within a scientific community. So it is with the modernity school. Generally not recognized or explicitly formulated, these conventions often tell us more about the nature and developmental stage of a theory than would an examination of either the logic or evidentiary basis to the theory. These assumptions can act as barriers to the use of methods required to render a theory testable. As a result, they can assume the additional disfunction of masking anomalies between evidence and theory. Finally, their habituation in re-

---

* Direct all communications to: William C. Rau; Department of Sociology; State University of New York; Albany, NY 12222.

I am grateful to Bernard Karsh, Frederick Fliegel, William Form, Ken Southwood, Reeve Vanneman, and to Jere Cohen, Mariah Evans, Joseph Kahl, Wilbert E. Moore, Allan Schnaiberg and an anonymous reviewer for commentary on several versions of this paper. I would like to thank Ledyard R. Tucker for bringing the parallel analysis criterion to my attention.

search practice can obstruct the resolution of anomalies once they occur.

This state of affairs characterizes modernity studies. The purpose of this paper, therefore, is to raise several unexamined assumptions of this school to the level of explicit propositions. I then demonstrate that the leading assumption in modernity research violates one of the basic principles of scientific jurisprudence and, moreover, is unnecessary in research. Further, this assumption cannot withstand statistical examination. The assumption is unidimensionality. The main purpose here is to reveal the inappropriateness of this cardinal assumption and the problems which flow from it. I do this first by examining how unidimensionality handicaps research into individual modernity. I, then, marshal evidence which shows that modernity studies must move to a multidimensional perspective if the field is to remain viable and dynamic.

Some of the assumptions of the contemporary modernity school can be seen in the work of Cohen and Till (1977) and Armer and Schnaiberg (1972; 1975; 1977). This is ironical since the latter are among the more articulate critics of the assumptions of unidimensionality and unilinearity in modernization theory. Even so, their classic review of modernity scales operates within a unidimensional framework. They are, therefore, confined within the limitations of the style of research they criticize. The major problem is that they reveal serious inadequacies in modernity scales but are unable to suggest solutions to the resulting impasse.

One of the primary objectives of both investigations is to determine whether modernity scales meet the convergent/discriminant validation test (see Campbell and Fiske, 1959). Unfortunately, because this is an advanced validation procedure, the resulting evidence misdirects attention from fundamental deficiencies in the construction of the scales which go into the test. These investigations assume that the study of modernity has advanced beyond an exploratory research stage; hence, antecedent and more primitive questions are overlooked.

The central question can be stated simply. Is individual modernity unidimensional or multidimensional? A number of scholars, such as Portes (1973) and Schnaiberg (1970b), suggest the latter, a position which receives statistical support in this paper. Under such circumstances, the varied operationalizations of individual modernity (as reflected in the classes and types of items in modernity batteries) cannot be assumed to measure only one set of values. Yet the conventional method of creating scales from such batteries uses item analysis as a "blind culling procedure" (Lumsden, 1978:23). As Schnaiberg (1976a; 1976b) and Armer (1977) note recently, the resulting scales cannot be expected to be either valid or equivalent. In retrospect, they used the convergent/discriminant validation technique when every scale in the convergent class was probably invalid from the start. This suggests that we return to the logically antecedent problem of scale construction.

The principles of scale construction require that one should first consider the construct and content validity of items going into a scale before conducting statistical tests of reliability and validity. However, as Nunnally (1967:79–88) and Lord and Novick (1968:350–1) point out, the logically and temporally prior process of conceptual validation is generally not considered, or is explored after a scale is constructed. (Analogous problems have been encountered in the natural sciences. See Kuhn, 1961). Instead, a number of scholars working in a common area develop different measures for the same thing, conduct research, and publish results. Findings accumulate and contradictions begin to surface. This precipitates questioning on the validity and conceptual framework of the various measures being used. Practitioners are then forced to deal explicitly with problems of construct and content validity. This sequence of events describes the current state of modernity studies. Consider the question of convergent and discriminant validity in Armer and Schnaiberg (1972).

Validation Procedures

In terms of modernity theory, convergent validity assumes that if

$$M_1 \cap M_2, \text{ then } M_1^* \cong M_2^*,$$

where M_1 and M_2 represent conceptual articulations of the traditional/modern dichotomy, and where M_1^* and M_2^* represent a sample of measures of each conceptual elaboration. If the two scales are conceptually equivalent, then, empirically, they should behave in a similar fashion with scores of individuals correlating highly. Discriminant validity assumes that if

$$M_1, M_2 \not\subset A, \text{ then } M_1^*, M_2^* \neq A^*,$$

where M and A represent conceptually distinct value domains, and M* and A*, a sample of measures for each concept.

Thus, the test that Armer and Schnaiberg, 1972, conduct, and that Cohen and Till, 1977, replicate with modification is that

$$rM^*_1M^*_2 > rM^*_1A^*, rM^*_2A^*.$$

Put simply, the correlation among the modernity scales should be substantially higher than the correlation of modernity scales with the measure of another concept. This is a sound testing strategy that can be considered for any additive construct.[1] A prior question, however, is whether the assumptions of the test have been met. Specifically, are the scales used in both reports suited to tests of convergent and discriminant validity? The answer is no.

CONCEPTUAL EQUIVALENCE AND DISSIMILARITY

Consider the modernity scales. These scales are extracted from modernity batteries developed by Smith and Inkeles (1966), Kahl (1968), Schnaiberg (1970a), and Armer (1970). In what sense are the sets of measures in these batteries conceptually equivalent? On either a conceptual or empirical level, are they sampling from the same value universe? Witness, in Table 1, the value sets or subscales contained in these batteries.

The degree of overlap for the number of items, the number of value subsets, and thematic content of value subsets is low. This table also illustrates that the reservations of Cohen and Till (1977:374–5) regarding the inclusion of the Schnaiberg Emancipation Scale in validation tests are well taken.[2] In addition, the Kahl Modernity I Scale is quite different from the Smith and Inkeles and Armer indices. Even if we discard subscales 13 to 17 (from Stratification of Life Chances on), the equivalence of the Kahl, Smith and Inkeles, and Armer modernity batteries is more apparent than real. Smith and Inkeles's Political Identification/Activism Subscale, for example, has no direct equivalent on the other batteries. Similarly, Kahl's Individualism Subscale shares an affinity with the Receptivity to Change Subscales only on a highly abstract, conceptual level.

Finally, the degree of equivalence of

---

[1] Not all test theorists think so. Lumsden (1976), for example, feels that this procedure is too difficult in practice and unlikely to yield reasonable dividends. In his (Lumsden, 1976:270) words, "the multi-trait multi-method procedure seems to require that a test have an identical twin of the opposite sex." Moreover,

> where great care has been taken to test construction, where much is known a priori about traits, methods, and relationships between them, the validation procedure will more and more resemble construct validation and the explicit setting up of the multi-trait multi-method matrix will be rare.

This is no doubt correct, insofar as it goes. From the perspective of comparative measurement, however, this technique may assume new functions. One of the central problems in cross-national study concerns the relation between sets of culture common and culture specific measures of the same concept (see Triandis et al., 1971; Rau, 1979:169–201). This problem goes to the heart of the disagreement between Inkeles (1969) and Stephenson (1968;1969). Here, the multitrait, multimethod procedure provides a way to deal with the questions raised in this exchange. It gives us a systematic approach to investigating linkages between the particular and general aspects of cultural forms. At this level of analysis we are not looking for identical twins, but family relationships.

[2] In addition, Schnaiberg's battery consists of behavior items rather than attitudinal statements. As presented elsewhere (Rau, 1979:29–60, 256–76), and as Armer and Isaacs (1978) have demonstrated, modern values—at least as portrayed by an overall modernity index—are not sufficient for the prediction of behavior. On the other hand, the underrepresentation of items dealing with women's emanicipation in most modernity studies means that Schnaiberg's battery may prove to be helpful in an exploratory analysis of the nature and number of latent traits which should be given attention in modernity studies.

Table 1. A Comparison of the Subscale Composition of Modernity Batteries

No.	Kahl (1968) Modernity I (N)	Smith & Inkeles (1966) OM–6 (N)	Armer (1970 Individual Modernity (N)	Schnaiberg (1970a) Emancipation (N)
1	Activism (7)	Efficacy (8)	Efficacy/Futurism/ Empiricism (11)	——
2	Occu. Primacy (4)	Ed./Occu. Aspiration (4)	——	——
3	Risk Taking/ Individualism (9)	Change Orientation (1)	Change Receptivity (4)	——
4	——	Openness to New Exp. (4)	Openness to Ideas (4)	——
5	——	Political Identification (6)	——	——
6	Religiosity (4)	Religiosity (5)	Secularism (5)	Religious Behavior (2)
7	Family Integration (3)	Kin Obligation (1)	Family Independence (3)	Extended Family Ties (1)
8	——	——	Ethnic Equality(4)	——
9	Family Modernism (5)	Women's Rights/ Family Planning (5)	Women's Equality (3)	Nuclear Family Behavior (14)
10	Mass Media/Info. (6)	Mass Media (2)	——	——
11	Trust (6)	——	Interpersonal Trust (3)	——
12	——	Consumption Aspir. (1)	——	Consumption Index (1)
13	Life Chance Strat. (3)	——	——	——
14	Community Strat. (3)	——	——	——
15	Anti Big Company (3)	——	——	——
16	Pro Manual Work (3)	——	——	——
17	Urban Life Pref. (2)	——	——	——

many of the subscales is often a function of the same concept labels rather than content equivalence of subscale items. A case-in-point concerns one of the more comparable subscales, namely, the religiosity subscales in the first three modernity batteries. The items within each subscale are as follows:

Kahl

(Help from God/hard work by people) most important for the country's future.
(More religious/less religious) than father.
I consider myself (very/not very) religious.
In the last two months, I went to church (often/not often).

Armer

Should give alms to the poor because of (fear of God/compassion).
Should follow (religious men/sacred books/ own conscience).
(Need not be/should be) respectful of other religions.
A boy (cannot/can) be truly good without any religion.
Heaven is for (own religion only/good people of other religions).

Smith and Inkeles

Who do you admire most? monk/both/ factory owner.
Who has done more for this country? monk/ both/factory worker.
Most important in caring for sick person: prayer/both/medical care.
Can a man be good without religion? yes/no.
A man should give to the poor because of: fear of God/generosity.

Each set of items probably taps the same religiosity/secularism domain and is likely to produce statistically equivalent scales. Nonetheless, without empirical confirmation that these items covary together, a skeptic could argue that Kahl's scale is a religious participation scale, Armer's, a religious tolerance scale, and Smith and Inkeles's, a religious esteem scale.[3]

---

[3] A more important difficulty is that these scales do not discriminate among types of religiosity and secularism, or among the ethical precepts of the great religious traditions. A devout Hindu, Muslim, or Protestant could receive the same scores on these

Due to these considerations the conceptual and statistical equivalence of modernity indices is a question that deserves detailed and systematic attention. One can argue that existing modernity scales are not conceptually equivalent. Present methods of scale construction, in conjunction with the unsystematic diversity of value sets and attitudinal statements in the various batteries, cannot be expected to produce scales which are either equivalent or valid. If these scales are not conceptually equivalent, then a test of convergent/discriminant validity is illogical.

Subset equivalence of modernity indices. There are, however, several thematic cores across these modernity batteries. The unique virtue of the Armer-Schnaiberg Uptown data set is that it allows factor analysis within and across these batteries. It will, therefore, prove invaluable in the validation of the attitudinal content and number of value themes appropriate to the modernity concept.

I have conducted a somewhat analogous investigation into the structural characteristics of the Peshkin modernity battery used in the Karsh five nation study of value modernization (see Rau, 1979:169–201). This analysis establishes efficacy-activism as a pancultural value theme for the nation samples in this study. We have, therefore, provided support to the observations of Kahl (1968) and Inkeles and Smith (1974) on the centrality of personal efficacy and activism in modern value systems. In terms of their interpretation, efficacy establishes that an individual can and should control and direct his own destiny, and stresses the role of certain values and behaviors in achieving chosen ends. Activism establishes that an individual feels that men in general can and should harness and direct natural and social forces. The two are mutually reinforcing and very close conceptually and psychologically.

The measures used to establish discriminant validity are anomia, alienation, and SES. Here the value orientations are substantially more restricted than in the case of overall modernity. Moreover, SES is a structural index rather than a measure of values. Thus, the tests of discriminant validity are based on a subset to set comparison. If

$$A \not\subset | M, \text{ then } A^* \neq M^*,$$

where A is the value subset that is conceptually distinct from all the values incorporated in the set M.

Anomia/alienation and efficacy/activism. A difficulty arises, however, when we look at anomia and alienation and then consider their putative distinctness in relation to the modernity concept. As mentioned, the core value set in modern value systems appears to be efficacy/activism. Anomia and alienation, in turn, represent measures of personal powerlessness, normlessness, the lack of an internalized and integrated value system and, in general, a sense of personal futility and societal fatalism. Thus, on a conceptual level anomia/alienation are mirror-opposites, inverted equivalents, to personal efficacy and activism.

To illustrate this point, items from Srole's (1956) Anomia, Middleton's (1963) Alienation, and Kahl's (1968) Modernity Battery are comingled in Table 2. At first glance, it is difficult to identify the source of each item, and even more difficult to establish a rationale for the conceptual distinctness of the separate groups of items. It strains credulity to assume that a panel of lay judges could make the necessary distinctions, and even with such virtuosity one would find it hard to demonstrate a convincing logic of exclusiveness. In short, the primary difference between anomia/alienation and personal efficacy/activism is the direction in which one

---

scales. Hence, the meaning of scores can change from one cultural and religious context to another. The comparative usefulness of such measures is quite limited.

Another quandary concerns the relation between mundane values and religious belief. We cannot assume a consistent relationship across components in modern value systems and the beliefs of different religions. It is, thus, unfortunate that Charles Morris's (1955; 1956) work on the cross-national analysis of ethical systems appears to have been overlooked by modernity scholars. A simplified version of his Ways of Life questionnaire may provide us with the type of instrument needed to separate out different ethical points of view and their relation to religiosity/secularism and the various modern value orientations.

Table 2. Srole's Anomia and Middleton's Alienation Items Intermingled with Key Items from Kahl's Attitudinal Battery

Number	Statement
1	It doesn't make much difference if the people elect one or another candidate, for nothing will change.
2	There is not much I can do about most of the important problems that we face today.
3	There's little use in writing to public officials because often they aren't really interested in the problems of the average man.
4	Nowadays a person has to live pretty much for today and let tomorrow take care of itself.
5	With things as they are today, an intelligent person ought to think only about the present, without worry about what is going to happen tomorrow.
6	Things have become so complicated in the world today that I really don't understand what is going on.
7	The secret of happiness is not expecting too much out of life, and being content with what comes your way.
8	In spite of what some people say, the lot of the average man is getting worse, not better.
9	People in a big city are cold and impersonal; it is hard to make new friends.
10	These days a person doesn't really know whom he can count on.
11	I often feel lonely.
12	Most people will repay your kindness with ingratitude.

scores responses. Since the same sort of items masquerade under different exterior labels, the high negative correlations between anomia/alienation and measures of modernity are primarily a reflection of the difference in the way the two classes of scales are scored.

In other words, anomia and alienation, as negative expressions of efficacy and activism, lie in inverted form at the very center of the various modern value syndromes. Anomia, alienation, efficacy, and activism items all represent expressions of the same underlying trait. Thus, in this instance:

$$A \subset M1, M2.$$

This means that the middle term which links these semiequivalent modernity scales is represented as the discriminant validation index. Hence, the common empirical core which unites these rather different indices is being used as the distinct concept measure.[4]

SES and modernity: discriminant or predictive validity? The use of SES as a measure of discriminant validity is also questionable. SES is, in large part, a measure of privileged access to institutions, namely, the extent to which individuals have access to, have been processed through, or participate in complex, rationalized organizations, such as institutions of higher learning or bureaucratized and highly capitalized work organizations. Now, modernity theory clearly states that such institutionalized milieux have strong modernizing effects (see Form, 1976; Kahl, 1968; Inkeles, 1960; 1975; 1976; Inkeles and Smith, 1974; Stinchcombe, 1974). For example, Kahl (1968:45–52) observes that SES is one of the more powerful predictors of modernity, and that the results of his study indicate that "a modern perspective diffuses through society via the social-class hierarchy." Additionally, Inkeles and Smith (e.g., 1974:133–43) have established that the institutional components of social class indicators (for example, educational attainment) influence the development of modern values independently of the other correlates of social class. A theory of psychological modernization that incorporates both institutional and social learning perspectives cannot also argue that modernity is a class-free concept. Since class is closely related to both institutional privilege and differences in socialization practices and educational attainment, some association is inevitable. From this perspective SES must be seen as a measure of predictive, not discriminant, validity. The positive correlations between SES and modernity indicate that the latter does result in theoretically predicted and meaningful differences among social groups.

---

[4] On logical and psychological grounds it is difficult to assume that an individual could be both alienated and activist in his values. Instrumental activism represents the negation of psychological expressions of alienation. Hence, measures of the two concepts should emerge as polar ends framing the same continuum or latent trait. If so, it is proper to represent these in a fashion somewhat analogous to Parsons's construction of the pattern variables. An actor can be either alienated or activist in his values, but he cannot be both at the same time. Thus, in terms of their empirical constitution, measures of these linked conceptual dichotomies should cluster on opposite ends of the same factor.

The use of SES in a test of discriminant validity mixes a real concern regarding spuriousness with the requirements of scale validation. Most modernity theorists assume that this value system will contribute to the total explained variance in a causal model. Thus, while social class variates will be instrumental in the inculcation of modern values, these values, in turn, will condition outcomes further along in a causal chain. It is, therefore, necessary to establish that the relation between values and other social phenomena is not a spurious function of social class. But the intermediate location of values in any causal chain does not rule out a strong relation between class and values. Thus, the comparison of social class and modernity in a test of discriminant validity is illogical.

Dimensionality and Scale Construction

Since the importance of dimensionality is not immediately obvious, its relation to scale construction needs emphasis. When constructed properly, a scale measures a single underlying trait or psychological process. The items in the scale will, therefore, covary more highly with each other than with measures of other traits. Under such circumstances a scale will possess unidimensional properties and will tend toward factorial simplicity. Hence, the importance of dimensionality. If a scale is factorially complex, then independent variation across items in the scale cannot be ruled out. Such scales may measure more than one property or latent trait. This goes against the very principle of measurement since the latter

> implies that one characteristic at a time is being quantified. The scores on an attitude scale are most meaningful when it is known that only one continuum is involved. Only then can it be claimed that two individuals with the same score or rank order are quantitatively and, within limits, qualitatively similar in their attitude toward the given issue. As an example, suppose a test of liberalism consists of two general sorts of items, one concerned with economic, the other with religious issues. Two individuals could thus arrive at the same numerical score by two quite different routes. Now, it may be true that economic and religious liberalism are correlated but, unless highly correlated, the meanings of scores based on such a composite is questionable. (McNemar, 1946:298)

In such situations, results derived from factorially complex scales—designated conglomerate measures by psychometricians—are likely to contain major errors.

Now, a strange inconsistency. Many modernity theorists assert that psychological modernity is multifaceted, complex, and multidimensional. Why, then, the resort to "blind culling procedures," like item analysis, to select scale items from batteries which contain measures for as many as 24 different value themes? As stated, the basic characteristic of any multidimensional phenomenon is independent variation across the properties which constitute the phenomenon. The reason we compartmentalize a multidimensional entity into its constituent traits is to avoid the problem of scale aggregation. If we bury separate properties or latent traits within one index, then independent patterns of variation are concealed or "washed out" in the aggregate index. As a result, we can distort and obfuscate potentially important distinctions required in the precise specification of psychological and social processes.

In the final analysis, the unidimensional scalability of overall modernity is an empirical question that is answerable with fair precision by means of a number of statistical tests. Yet, present investigations into modernity scales have not considered such tests even though this problem bears directly on the issue of convergent and discriminant validation. This is particularly apropos to comparative studies since the potential for the errors associated with conglomerate scales is likely to be magnified when different national and cultural areas are under study. As such, scale aggregation can conceal both national differences and similarities. One outcome can be scores which are epiphenomena of masking statistical techniques rather than human patterns of valuation.

The Modernity Debate and Unidimensionality

Dimensionality is important because of the aforementioned problems. It also opens inspection of a number of questionable research practices. In terms of methodological conventions, three subsidiary issues are tied to this assumption: (1) the general reliance on item analysis to develop scales; (2) the relation between reliability coefficients and itemetric scales; (3) the lack of a statistical test of dimensionality due to the reversal of null and alternative hypotheses in modernity research.

Item analysis. Item analysis has been used to select those items that have acceptable correlations (e.g., where r > .20) with a composite index formed from the sum of all of the items in a battery. The items above the cutting point are used to construct scales and calculate reliability coefficients.

The limiting factor in modernity research is that item analysis was intended for the empirical validation of scales. It does not speak to the theoretical or psychological integrity of the domain sample in the analysis. It assumes that sample items have been predetermined to be valid measures of one target domain or latent trait. These items, in their entirety, should give the boundaries of the construct.

A crucial issue which Armer and Schnaiberg raise concerns the psychological integrity of overall modernity constructs, namely, whether modern value orientations can be represented by a single index. Unfortunately, item analysis cannot provide statistical verification of such a construct. Even if such a construct does exist we do not know the types of modern values which define this construct. Lacking this information, we cannot classify specific measures or value sets into an overall modernity category. Hence, several difficulties crop up when items of unknown construct validity are chosen by this technique. First, it is virtually impossible not to establish a number of item-to-scale correlations of acceptable magnitude. Item analysis, Guttman (1971:343) observes, is widely used because "it is virtually guaranteed to succeed for the kinds of data concerned." As a result, item analysis "does not test any hypothesis of scalability. It assumes that scalability exists and that its task is merely to cull out inappropriate items." Once one establishes a sample of items to represent a homogeneous value universe, item analysis becomes an efficient tool for selecting effective scale items. Used appropriately, it can produce scales that are parsimonious, discriminating, and reliable. It cannot, however, establish unidimensionality or conceptual homogeneity.

The reason, and second limitation, is that item analysis does not consider the structure of intercorrelations among measures, thereby begging the problem of dimensionality. The fact that a number of attributes covary with a scale does not mean that they also covary with each other. For example, the fact that activism and secularism items are selected for an itemetric scale only means that the chosen items covary with the aggregate scale. Their relation to each other is unknown and problematic. Unless there is an accumulated fund of knowledge regarding the content and construct validity and statistical characteristics of a psychological domain, one cannot rule out substantial independent variation across measures selected by item analysis. This is particularly apropos to exploratory research ventures such as comparative value analysis. To wit, all value scales should contain statistically interrelated items. This can be established when items in a conceptual class factor together in a factor solution with negligible residuals.

Reliability coefficients. Reliability coefficients do not resolve the aforementioned problems since heterogeneous, factorially complex scales can still produce acceptable coefficients (Cronbach, 1951; Cattell, 1964; Lumsden, 1961; 1976; Nunnally, 1967). Conventional reliability coefficients only establish the strength of the average intercorrelation of the scale items. They assume that low intercorrelations are randomly dispersed among items and that unidimensionality characterizes the structure of intercorrelations. As a result, reliability coefficients, such as the Kuder-Richardson Formula 20, can yield "a gross underestimate of reliability unless the test is unidimensional" (Lumsden, 1976:266). In sum, the fundamental assumption of classical reliability theory is that one general factor dominates the items in a scale (Cronbach, 1951; Cattell, 1964; Cattell and Tsujioka, 1964; Cotton et al., 1957; La Forge, 1965; Taylor, 1977).

Neither item analysis nor reliability coefficients can establish that all items in a scale systematically covary with each other. In this sense, both reliability coefficients and item analysis rely on the same assumptions. Both assume scalability and unidimensionality without verification of either assumption. A more conservative and rigorous approach is first to demonstrate scalability by establishing that the items going into a scale hold a statistical relationship to each other—for example, through a sequential unfolding process or through mutual covariation. On an item-to-item level of analysis one can convincingly establish that a scale has ostensive statistical properties. The nature of this problem is perhaps best illustrated by an example.

At a preliminary stage of investigation, reliable, item analysis scales were developed for the nation samples in the Karsh (1971) project. Each of these scales incorporated both activism and secularism items. Exploratory factor analysis, however, established that items in these value sets loaded on different factors in four of the five nation samples. As a result, we could not assume that the two subsets varied together. Additional analysis confirmed this suspicion. Table 3 presents a case in point.

These results establish that the American and Japanese samples receive nearly identical scores on overall modernity indices. For both samples, however, factor analysis establishes that the two major components in these indices can be represented as separate traits, and therefore as activism and secularism scales. The scores on these scales are quite different. The United States sample is substantially higher on the activism scale and dramatically lower on the secularism scale. As a result, the two nations locate in completely different spaces in a two dimensional matrix.

Table 3. Mean Scores on Overall Modernity, Activism, and Secularism Scales for the Japanese and United States Samples: Scales Adjusted to a 0–100 Range

Nation Sample	Overall Modernity	Activism	Secularism
Japan	59	57	58
United States	57	67	31

In this case, overall modernity indices are conglomerate measures with a serious problem of scale aggregation. Because of such possibilities, psychometricians have insisted that a scale measure one, and only one, clearly defined trait unless there is a well-established rationale for a multiple trait scale. The problem is that the interpretability of scores becomes quite hazardous for such scales.

Thus, if the structure of covariance of modernity batteries is best represented by a multiple factor solution, then the construction of overall modernity scales is highly questionable. It would suggest that subscales collapsed into a one-dimensional index be represented as components or vectors within a multidimensional framework. It would also suggest that current modernity indices are conglomerate measures and, therefore, of dubious utility. With this in mind we turn to the cardinal assumption of the modernity school.

Null and Alternative Hypotheses in Modernity Research

Why has unidimensionality been given the status of an axiom when it should be considered a testable proposition? "The dimensionality of data," Guttman (1971:343) states, "is an empirical phenomenon and not to be determined by fiat." Yet, this is exactly what has happened in modernity studies.

This tacit convention reverses the logic of hypothesis testing. It posits unidimensionality as the null hypothesis and multidimensionality as the alternative hypothesis. For example, Inkeles and Smith (1974:84) frame the question in terms of "whether individual modernity [is] a general syndrome rather than mere congeries of discrete elements." This is both a false dichotomy and an example of the reversal of null and alternative hypotheses. The proper question is not whether modernity is a general syndrome or nothing at all. Rather, the questions are: What are the major value themes which receive expression in a population? What relationships exist among these value themes? and What method of statistical representation is required to portray the patterns of variation within and across these themes?

Given our knowledge of the multiplicity and mutability of values, and of the variety of human undertakings in contemporary societies, scientific prudence dictates that we establish the multidimensionality of values as the null hypothesis. A historical analogue to the present dilemma in modernity studies can be seen in the initial attempts of psychologists, such as Charles Spearman, to represent intelligence by means of a single factor. In fact, Spearman's factorial procedures did not permit the representation of group factors or the multiple factor representation of intelligence (e.g., Thurstone, 1931; 1947; Brody and Brody, 1976). On the basis of his theoretical predisposition and statistical approach, Spearman was both unwilling and incapable of exploring the possibility of a multidimensional structure to intelligence. "This is a most unusual status to give to a revolutionary hypothesis," Guttman (1971:344) notes. He continues: "The whole idea of null versus alternative hypothesis . . . is to have the new kind of idea be the alternative. . . ." Thus, new ideas are not given plausible status without solid, cumulative evidence. Since "intelligence has been recognized by most people to comprise a very complex phenomena [, it] is a new idea to hypothesize a small number of factors." In short, the principle of verification requires that the null hypothesis state that the statistical representation of intelligence requires a large number of factors, "and only the alternative should be a small number, with the one-dimensional case being the extreme alternative."

The null hypothesis, I entertain, is that modernity is multidimensional and nation specific. I assume that the dimensionality, structure of variance, and patterns of meaning expressed in factor matrices will differ across nations and economic and cultural areas. Acceptance of either an overall modernity syndrome or pancultural value themes is, therefore, contingent upon the use of dimensionality criterion tests and factor comparison procedures. The importance of dimensionality estimation is that the tests now available can indicate when a one-dimensional index is statistically acceptable. Recent developments in factor analysis clarify this point.

Tests of Factorial Dimensionality

A common misconception views factor analysts as statistical luddites who smash meaningful constructs into an unwieldy and disorderly mélange of subcomponents. This misconstrues the mathematical characteristics of factor analysis. When used properly, this class of techniques can help to identify the common pattern or patterns of covariation in a correlation matrix. The resulting dimensionality is a function, not of factor analysis, but of empirical patterns in the data under scrutiny. If a set of attitudinal measures is characterized by unidimensionality, then one, or, at most, a few factors should represent major sources of common variance, or should produce a reconstituted correlation matrix that approximates the original correlation matrix. Where there are several factors, a general modernity syndrome (and the concomitant predominance of statistical unidimensionality) can be established when the first factor captures items from most of the variable subsets and monopolizes common variance.

One of the primary objectives of factor analytic techniques concerns the identification and measurement of properties or traits which, due to their abstractness or complexity, are not directly observable. By virtue of this fact a fair to large number of surface variables are needed to explicate fully and isolate such a trait or traits. Thus, a useful factor solution (1) provides a pattern matrix with conceptually interpretable constellations of variables, and (2) extracts the major sources of common variance in a correlation matrix.

A central problem until recently has been the lack of statistical estimation of the second issue. As a result, a number of judgmental procedures, such as the screen test, have been used to estimate dimensionality. The trouble with such procedures is that inexperience and subjective bias can affect their use. Consequently, under- and overfactoring have been commonly encountered in exploratory studies. In the first instance, the interpretability of a factor matrix can change as one extracts additional factors. In overfactoring, random noise is introduced into the pattern matrix and can produce factors which are epiphenomena of chance variation.

This problem has been greatly minimized with the recent development of several statistical tests of dimensionality. Two deserve attention. The first is given by the goodness-of-fit test in maximum-likelihood factor analysis (Jöreskog, 1967; Jöreskog and Lawley, 1968). The second is provided by the parallel analysis criterion which has been transformed into a practical research tool by Montanelli and Humphreys, 1976 (see, also, Horn, 1965; Linn, 1968; Humphreys and Ilgen, 1969; Humphreys and Montanelli, 1975).

Maximum-likelihood factor analysis determines goodness-of-fit by examining the residual covariance matrix remaining after a factor solution. An acceptable goodness-of-fit (e.g., $p > .01$) is established when the residual eigenvalues approach equality (Gorsuch, 1974:139–41; Mulaik, 1972:161–7). This indicates that the factor solution approximates the patterns of covariation estimated to exist in a population covariance matrix.

A difficulty with this test, however, is that it is sensitive to differences in sample size.[5] As a result, the likelihood ratio can underestimate dimensionality in small samples and, conversely, overestimate in large samples (Browne, 1968; Burt, 1973; Harris and Harris, 1971; Montanelli, 1974; Humphreys and Montanelli, 1975). In such situations, the likelihood ratio should be used in conjunction with a Tucker reliability coefficient (Tucker and Lewis, 1973) which compensates for the effects of sample size.

The parallel analysis criterion represents a somewhat more reliable and considerably less expensive technique. It is, therefore, well suited to exploratory research. Here we proceed by inserting squared multiple correlations in the diagonal of the correlation matrix and factoring by means of the principal axes method. The process is then repeated on a second correlation matrix which is constituted from normally distributed random numbers, where the order of the actual and random data matrices is the same. The latent roots obtained from the actual and random data are then compared. The number of factors required to extract the sources of common variance is given at the point where the actual eigenvalue equals or drops below the random eigenvalue.

This technique has been rendered usable through the development of regression equations for predicting random eigenvalues (Montanelli and Humphreys, 1976). The estimated eigenvalues are virtually identical to those derived from monte carlo experiments. Thus, practitioners are spared the task of constructing random correlation matrices.

If the fit between data and the common factor model is adequate, this technique will yield a reliable estimate of factorial dimensionality. On the other hand, if the correlation matrix is ill-conditioned, it is possible to overestimate common factors.

---

[5] In addition, the most useful application of the maximum-likelihood estimation procedure is in confirmatory or hypothesis-testing factor designs. These models, however, presuppose a structural theory for the target domain in such a way that the parameters of the hypothesized model can be specified (e.g., Burt, 1973; Jöreskog, 1974; Mukherjee, 1973; Mulaik, 1975). Without such a theory, the use of confirmatory factor or structural covariance analysis is likely to be governed by ad hoc and post hoc considerations.

For the present study, in any case, the maximum-likelihood goodness-of-fit procedure yields dimensionality estimates which are either identical or very close to results derived from the parallel analysis criterion. The results are as follows:

Nation	*No. Factors*	χ^2	*Probability*	*Tucker Reliability*
Ghana	6	1,252	.05	.89
India	7	1,348	.00	.83
Brazil	5	1,293	.07	.90
Japan	7	1,218	.02	.88
USA	7	1,236	.01	.90

Similarly, if extant and theoretically important traits are overlooked in the battery, then dimensionality can be underestimated. As such, this technique cannot be used blindly, particularly in exploratory research ventures.

A Five Nation Analysis of Dimensionality

We can demonstrate dimensionality estimation by examining the dimensional properties of the Peshkin battery used in the Karsh study. The data were derived from field interviews administered to groups of farmers and industrial workers in Ghana, India, Brazil, Japan, and the United States.

The dimensionality established by the parallel analysis criterion is given in Table 4. As can be seen, four to five factors must be extracted from each sample correlation matrix to represent at least one-half of the common variance. Additionally, the proportion of variance across descending factors is such that at least two major factors appear in each matrix with the possible exception of India. Since a number of value themes, such as universalism/particularism, do not receive adequate representation in the present battery, it is likely that the reanalysis of other studies will isolate other value orientations with comparative significance. In sum, the factorial dimensionality and the structure of variance in the present study rule out unidimensional modernity constructs.

Table 4 also points to a problem. The required dimensionality calls for a rather large number of factors for both the Indian and Japanese samples. As mentioned, the accuracy of this criterion test depends on a reasonable fit between the common factor model and a set of data. As data depart from the model the criterion test will overestimate dimensionality.

Departures from the common factor model characterize the data for India and Japan. Closer inspection reveals that the communality estimates for the correlation matrices of these samples are below desirable levels. The mean communality estimates for the nation samples are as follows:

Table 4. Parallel Analysis Criterion Test: Factorial Dimensionality Estimation for Five Nations

	Nation Percent Common Variance—Actual Eigenvalue—Random														
Factor	Ghana			India			Brazil			Japan			United States		
Number	% Var.	Actual	Random	% Var.	Actual	Random	% Var.	Actual	Random	% Var.	Actual	Random	% Var.	Actual	Random
1	21.3	6.55	1.95	29.6	6.54	1.42	21.8	6.21	1.42	21.9	4.90	1.59	26.3	8.04	1.89
2	15.7	4.82	1.85	9.7	2.15	1.33	12.8	3.64	1.85	15.0	3.36	1.48	17.0	5.20	1.77
3	7.8	2.40	1.71	8.4	1.87	1.23	6.9	1.97	1.73	7.0	1.57	1.38	5.9	1.81	1.66
4	6.4	1.95	1.58	7.0	1.54	1.14	5.8	1.65	1.59	6.5	1.47	1.27	5.3	1.61	1.53
5	6.2	1.89	1.51	6.3	1.38	1.08	5.4	1.54	1.52	6.0	1.34	1.22	4.9	1.50	1.46
6	5.9	1.81	1.41	5.5	1.21	1.01		1.36	1.43	5.4	1.21	1.14		1.32	1.36
7		1.33	1.33	5.1	1.12	.95				5.2	1.17	1.07			
8				4.3	.96	.91				4.9	1.10	1.02			
9				4.1	.91	.85				4.3	.97	.96			
10					.81	.81				4.1	.93	.91			
11											.84	.89			

Ghana	.52
India	.40
Brazil	.52
Japan	.41
United States	.56

In factor analytic practice, an average communality estimate of below .50 is taken as a sign of departure from one of the assumptions of the common factor model. This means that a fair number of items do not share much common variance with other battery items. In other words, intercorrelations among items tend to run consistently on the low side.

Such an outcome can stem from a number of difficulties: (1) The items in the battery may not be tapping existing latent traits, hence, a problem of either construct or content validity. (2) The underlying distribution of data may depart substantially from the assumptions of linearity or multinormality. Like all regression models, factor analysis tends to be robust. But large departures from assumptions can lead to the sort of problems encountered here. (3) A sample may not possess sources of common variance. The individuals studied may share little in common or too much in common. The first situation will result in low intercorrelations, that is, data lacking an internal structure. The second may result in a distribution which is not multinormal or linear, or data with little variation.

In the present study, the following possibilities are likely: In India the research design forced us across several different cultural areas. I suspect that the problem here is due to low intercorrelations which reflect the cultural diversity of this sample. Japan, on the other hand, may represent the opposite situation. The Japanese sample has proved to be homogeneous in the extreme with regard to quantitative differences among the value patterns found (Rau, 1979:230–55). In the case of Japan, it is possible that the data distribution may depart meaningfully from the assumptions of normality or linearity. It is also possible that the attitudinal statements may not adequately tap Japanese values.

The major point is to underscore once again the early developmental stage of comparative value analysis. I have also shown how factor analysis can help to uncover major problem areas in value studies, problems which are not exposed by present statistical techniques. Moreover, factor analysis can help investigate content and construct validity, and can assist in the development of a structural theory of modernity.

This much, however, is certain. The present results indicate consistently a need to develop indices for value themes which depict independent sources of variation. The major problem is to define the central value traits contained within modern value systems. Such an undertaking will ultimately lead to scales with acceptable levels of content and construct validity.

Table 5 presents a preliminary approach to this problem. Here, we begin the conceptual interpretation of factor matrices through overview of the subscale representation on various factors. By itself, this shorthand method of substantive factor interpretation can obscure meaningful differences in factors with items from similar conceptual classes. The content variety among modernity subscales is sufficiently great that this constitutes a hazardous procedure. For example, the content of a number of the Peshkin achievement and universalism items concerns sex role stereotyping. Other status ascription items focus on locality, age, education, and so forth. If the gender items load together, one can view this as a women's emancipation factor, or, depending on the other items on the factor, a universalistic value syndrome. Such situations require a content or profile analysis of items on the factor. The present level of analysis is but a prelude to a detailed interpretation of factors in a companion paper (Rau, 1979:169–201).

With this in mind, one can observe a number of obvious patterns in Table 5. One demonstrative pattern is seen on the first factor in the Brazilian sample. This factor possesses the characteristics of a general modernity syndrome with a substantial number of items from nearly all the value subscales in the Peshkin battery. In relation to the salience of subscales in the other factor matrices, only the need-

Table 5. Percentage Subscale Items in the Peshkin Modernity Battery with Loadings of .30 for the First and Second Factors across the Five Nations*

	First Factor					Second Factor				
Subscale (N)	Ghana	India	Brazil	Japan	USA	Ghana	India	Brazil	Japan	USA
Universalism/ Particularism (8)	38	38	63	13	50	38	00	00	00	13
Ascription/ Achievement (8)	38	00	50	63	50	25	25	00	00	25
n-Achievement (12)	42	50	17	50	75	17	08	08	00	17
Closed/Open (5)	40	20	60	20	60	20	20	20	20	00
Dependent/ Independent (4)	00	00	25	25	75	50	25	00	00	00
Alienation/ Trust (4)	00	00	25	50	00	00	00	25	25	00
Religiosity/ Secularism (8)	50	00	63	13	00	00	63	38	38	100
Localite/ Cosmopolite (6)	50	25	17	00	17	00	00	33	00	50

* These results are derived from the Varimax, Principal Axes solutions used in Table 6.4. For reasons cited in the text, I have departed from the parallel analysis criterion for India and Japan. Hence, the present analysis is based on the extraction of five factors for Brazil and the United States, six factors for Ghana and Japan, and seven factors for India. The sixth factor, rather than the second, is listed for Japan since this is where the secularism items appear.

achievement subscale is underrepresented. It is possible that the meager representation of need-achievement in this sample may be an indirect function of this nation's religious tradition. The patterns in this factor matrix suggest that a modern Brazilian is also likely to be secular. Nonetheless, the fatalistic propensity of South American Catholicism may place a shadowy stamp on modern values through a suppression of a stress on achievement. Clearly, this is a speculative interpretation of the relation of religion and modernity in Brazil. I offer it as an example of the pressing need to consider more carefully both the nature and relationship among values and belief, and as one more indication of the desirability of measuring national manifestations of the great religious traditions.

The United States sample represents another pattern. Here, need-achievement and other activism items are strongly represented on the first factor. This factor could be labeled a general activism factor. In addition, the secularism items cluster on the second factor. The separation of the activism and secularism domains in the United States suggests either a compatibility or neutrality between religion and modernity in this sample. Note also the broad affinity between the first Brazilian and American factors with the important exceptions that the latter is completely asecular and has a heavy emphasis on achievement items.

The first Japanese factor is also asecular. In relation to Brazil and the United States, there is a conspicuous paucity of measures of value universalism. In addition, the Japanese factor reveals a rather balanced stress on both with the evaluative and affective elements of an activist value orientation which further distinguishes it from Brazil.

Finally, the Ghanaian sample may constitute a special case. First, a number of negative loadings are not represented in Table 5. This suggests that modernity in Ghana is syncretic and that Ghanaians may blend both traditional and modern value elements into an appositional value system (Jahoda, 1970; Le Vine, 1973). Does this typify Ghana's culture and level of development, or a general solution to the problem of modernity in some developing countries? Further research and analysis should focus on this issue. Also, the major distinction between the first and second factors in Ghana consists in the absence of secularism and cosmopolite items on the second factor. Otherwise, the two factors seem to represent differing combinations of general modernity themes.

On the basis of the subscale composition of factors, the overall patterns are as follows: First, secularism items factor separately from activism items in the United States, Japan, and India. Thus, the general practice of including both types of items in modernity scales is highly suspect. It also accentuates a concern in the debate under discussion. Comparative scholars must devote more attention to the content and construct validity of value scales, and need to more carefully delineate the nature, consistency, and number of modern and traditional value domains. Second, the composition of subscales on the first factor varies across the five samples. As such, the extent of cross-cultural equivalence among factor matrices is an open question, as is the larger question regarding the pancultural features of modern value systems. Due to the general nature of the present analysis nothing conclusive will be stated here. The question of pancultural value themes is addressed elsewhere (Rau, 1979:169–201) by means of statistical comparison of factors, and a content and conceptual interpretation of items on correlated factors. The present analysis, however, should reveal the nature of the problem.

CONCLUSION

This paper's objective is to raise questions that lack systematic discussion in literature on modernity scales. It represents a continuation of the debate of Armer-Schnaiberg, 1977 and Cohen-Till, 1977. Their investigations examine the concept of value modernity and measures associated with the concept. The basic question is whether overall modernity scales constitute valid constructs. Armer and Schnaiberg reject measures of the concept while Cohen and Till suggest that valid modernity constructs can be developed if the concept itself is more carefully and precisely specified.

The present report supports the major observations of both pairs of authors. First, this analysis suggests that the concept of overall modernity be rejected. While it is possible that overall modernity may be supportable in some cultural settings, the results from this five nation study indicate that the value themes subsumed within this umbrella concept should be represented as differentiable components of multidimensional value systems.

Second, the possibility of valid measures of modern and traditional values is still maintained. A central theme here is that the development of valid measures will require a multidimensional approach. A corollary theme is that many of the difficulties encountered in the comparative study of values stem from the unverified assumption of the unidimensionality of overall modernity and measures of this concept. The balance of this paper substantiates this viewpoint by examining how this assumption structures and circumscribes studies in the area, and by suggesting a new direction to working through problems in comparative value analysis. Hopefully, this discussion will provide an impetus for rethinking current research strategies, suggest more fruitful approaches to statistical analysis, and open discussion of an agenda for attacking mutual problems.

REFERENCES

Armer, Michael

1970 "Formal education and psychological malaise in an African society." Sociology of Education 43:143–58.

1977 "Education and social change: an examination of the modernity thesis." Studies in Comparative International Development 12:86–99.

Armer, Michael and Larry Isaac

1978 "Determinants and behavioral consequences of psychological modernity: empirical evidence." American Sociological Review 42:378–82.

Armer, Michael and Allan Schnaiberg

1972 "Measuring individual modernity: a near myth." American Sociological Review 37:301–16.

1975 "Individual modernity, alienation, and socioeconomic status: a replication in Costa Rica." Studies in Comparative International Development 10:35–47.

1977 "Reply to Cohen and Till." American Sociological Review 42:378–82.

Brody, Erness Bright and Nathan Brody

1976 Intelligence: Nature, Determinants, and Consequences. New York: Academic Press.

Browne, M. W.

1968 "A comparison of factor analytic techniques." Psychometrika 33:267–334.

Burt, Ronald S.

1973 "Confirmatory factor-analytic structures and the theory construction process." Sociological Methods and Research 2:131–90.

Campbell, D. T. and D. W. Fiske
1959 "Convergent and discriminant validation by the multitrait-multimethod matrix." Psychological Bulletin 56:81–105.

Cattell, Raymond B.
1964 "Validity and reliability: a proposed more basic set of concepts." Journal of Educational Psychology 55:1–22.

Cattell, Raymond B. and Bien Tsiyioka
1964 "The importance of factor-trueness and validity, versus homogeneity and orthogonality, in test scales." Educational and Psychological Measurement 24:3–30.

Cohen, Jere and Amnon Till
1977 "Another look at modernity scales: reanalysis of the convergent and discriminant validities of the Armer, Kahl, Smith and Inkeles, and Schnaiberg scales (Comment on Armer and Schnaiberg, ASR June, 1972)." American Sociological Review 42:373–8.

Cotton, John W., Donald T. Campbell, and R. Daniel Malone
1957 "The relationship between factorial composition of test items and measures of test reliability." Psychometrika 22:347–57.

Cronbach, Lee J.
1951 "Coefficient alpha and the internal structure of tests." Psychometrika 16:297–334.

Form, William H.
1976 Blue-Collar Stratification: Autoworkers in Four Countries. Princeton: Princeton University Press.

Gorsuch, Richard L.
1974 Factor Analysis. Philadelphia: Saunders.

Guttman, Louis
1971 "Measurement as structural theory." Psychometrika 36:327–47.

Harris, Margaret L. and Chester W. Harris
1971 "A factor analytic interpretation strategy." Educational and Psychological Measurement 31:589–605.

Horn, John L.
1965 "A rationale and technique for estimating the number of factors in factor analysis." Psychometrika 30:179–85.

Humphreys, Lloyd G. and Daniel R. Ilgen
1969 "Note on a criterion for the number of common factors." Educational and Psychological Measurement 27:571–8.

Humphreys, Lloyd G., D. R. Ilgen, D. McGrath, and R. Montanelli
1969 "Capitalization on chance in rotation of factors." Educational and Psychological Measurement 27:259–71.

Humphreys, Lloyd G. and Richard G. Montanelli
1975 "An investigation of the parallel analysis criterion for determining the number of common factors." Multivariate Behavioral Research 10:193–206.

Inkeles, Alex
1960 "Industrial man: the relation of status to experience, perception, and value." American Journal of Sociology 64:1–31.
1969 "Comments on John Stephenson's 'Is everyone going modern?'," American Journal of Sociology 75:146–51.
1975 "Emerging social structure in the world." World Politics 27:467–95.
1976 "Understanding and misunderstanding individual modernity." Pp. 103–30 in Lewis A. Coser and Otto N. Larsen (eds.), The Uses of Controversy in Sociology. New York: Free Press.

Inkeles, Alex and David H. Smith
1974 Becoming Modern: Individual Change in Six Developing Countries. Cambridge: Harvard University Press.

Jahoda, Gustav
1970 "Supernatural beliefs and changing cognitive structures among Ghanaian university students." Journal of Cross-Cultural Psychology 1:115–30.

Jöreskog, Karl G.
1967 "Some contributions to maximum likelihood factor analysis." Psychometrika 32:443–82.
1974 "Analyzing psychological data by structural analysis of covariance matrices." Pp. 1–49 in D. H. Krantz, R. C. Atkinson, R. D. Luce, and P. Suppes (eds.), Contemporary Developments in Mathematical Psychology, Vol. II. San Francisco: W. H. Freeman.

Jöreskog, Karl G. and D. N. Lawley
1968 "New methods in maximum likelihood factor analysis." British Journal of Mathematical and Statistical Psychology 21:85–96.

Kahl, Joseph A.
1968 The Measurement of Modernism: A Study of Values in Brazil and Mexico. Austin: University of Texas Press.

Karsh, Bernard
1971 "The cross-national study of technology and culture." Unpublished paper.

Kuhn, Thomas S.
1961 "The function of measurement in modern physical science." Pp. 31–63 in Harry Woolf (ed.), Quantification: A History of the Meaning of Measurement in the Natural and Social Sciences. New York: Bobbs-Merrill.

La Forge, Rolfe
1965 "Components of reliability." Psychometrika 30:187–95.

Levine, Mark S.
1977 Canonical Analysis and Factor Comparison. Sage University Paper Series on Quantitative Applications in the Social Sciences. Beverly Hills: Sage.

Le Vine, Robert
1973 "Patterns of personality in Africa." Ethos 1:123–52.

Linn, R. L. A.
1968 "A monte carlo approach to the number of factors problem." Psychometrika 33:37–72.

Lord, Frederic M. and Melvin R. Novick
1968 Statistical Theories of Mental Test Scores. Reading, Mass.: Addison-Wesley.

Lovejoy, Arthur O.
1936 The Great Chain of Being: A Study in the History of an Idea. Cambridge: Harvard University Press.

Lumsden, James
1961 "The construction of unidimensional tests." Psychological Bulletin 58:122–31.
1976 "Test theory." Annual Review of Psychology 27:251–80.
1978 "Tests are perfectly reliable." British Journal of Mathematical and Statistical Psychology 31:19–26.

McNemar, Quinn
1946 "Opinion-attitude methodology." Psychological Bulletin 43:289–374.

Middleton, Russell
1963 "Alienation, race and education." American Sociological Review 28:973–7.

Montanelli, Richard G.
1974 "The goodness of fit of the maximum-likelihood estimation procedure in factor analysis." Educational and Psychological Measurement 34:547–62.

Montanelli, Richard G. and Lloyd G. Humphreys
1976 "Latent roots of random data correlation matrices with squared multiple correlations on the diagonal: a monte carlo study." Psychometrika 41:341–48.

Morris, Charles
1956 Varieties of Human Value. Chicago: University of Chicago Press.

Morris, Charles and Lyle V. Jones
1955 "Value scales and dimensions." Journal of Abnormal and Social Psychology 51:523–35.

Mukherjee, Bishwa Nath
1973 "Analysis of covariance structures and exploratory factor analysis." British Journal of Mathematical and Statistical Psychology 26:125–54.

Mulaik, Stanley A.
1972 The Foundations of Factor Analysis. New York: McGraw-Hill.
1975 "The confirmatory factor analysis." Pp. 170–207 in Daniel J. Amick and Herbert J. Walberg (eds.), Introductory Multivariate Analysis. Berkeley: McCutcheon.

Nunnally, Jum C.
1967 Psychometric Theory. New York: McGraw-Hill.

Portes, Alejandro
1973 "The factorial structure of modernity: empirical replications and a critique." American Journal of Sociology 79:15–44.

Przeworski, Adam and Henry Teune
1970 The Logic of Comparative Social Inquiry. New York: Wiley.

Rau, William C.
1979 "Industrial man in the global village: a five nation test of value convergence." Ph.D. dissertation, Dept. of Sociology, University of Illinois, Urbana, Ill.

Rummel, R. J.
1970 Applied Factor Analysis. Evanston: Northwestern University Press.

Schnaiberg, Allan
1970a "Rural-urban residence and modernism: a study of Ankara Province, Turkey." Demography 7:71–85.
1970b "Measuring modernism: theoretical and empirical explorations." American Journal of Sociology 76:399–425.
1976a "A review of 'Becoming Modern' by Inkeles and Smith." Journal of Political and Military Sociology 4:174–6.
1976b "Remarks on the review symposium on 'Becoming Modern'." School Review 84:296–300.

Smith, David H. and Alex Inkeles
1966 "The OM Scale: a comparative socio-psychological measure of individual modernity." Sociometry 29:353–77.

Srole, Leo
1956 "Social integration: certain corollaries." American Sociological Review 21:709–16.

Stephenson, John B.
1968 "Is everyone going modern? a critique and suggestion for measuring modernism." American Journal of Sociology 74:265–75.
1969 "Reply to Inkeles." American Journal of Sociology 75:151–6.

Stinchcombe, Arthur L.
1974 Creating Efficient Industrial Administrations. New York: Academic Press.

Taylor, James B.
1977 "Item homogeneity, scale reliability, and the self-concept hypothesis." Educational and Psychological Measurement 37:349–61.

Thurstone, L. L.
1931 "Multiple factor analysis." Psychological Review 38:406–27.
1947 Multiple Factor Analysis: A Development and Expansion of "The Vectors of the Mind." Chicago: University of Chicago Press.

Triandis, Harry C., Vasso Vassiliou, George Vassiliou, Yasumasa Tanaka, and H.V. Shanmugan
1972 The Analysis of Subjective Culture. New York: Wiley-Interscience.

Tucker, Ledyard and Charles Lewis
1973 "A reliability coefficient for maximum likelihood factor analysis." Psychometrika 38:1–10.

THE TRANSITION TO ADULTHOOD AS A CAREER CONTINGENCY*

DENNIS P. HOGAN
The University of Chicago

American Sociological Review 1980, Vol. 45 (April):261–276

Individuals in American society tend to agree that it is normatively appropriate behavior to order the events marking the transition to adulthood so that formal schooling is completed first, and so that both school completion and beginning of first job occur prior to marriage. This paper reports on the testing of the hypothesis that men who order their transition events in a nonnormative fashion experience reduced occupational status and earnings returns in their later careers. The study is based on the responses of 18,370 ever-married white males aged 20 to 65, who were employed in the experienced civilian labor force, and were interviewed in the 1973 Occupational Changes in a Generation II survey. While no difference in occupational attainments are observed, men who experience a disorderly transition to adulthood also experience lower earnings returns to their education, as well as substantial deficits in their total earnings.

INTRODUCTION

Prior to World War II, American males enjoyed considerable latitude in the ages at which they could choose to complete school, begin working, and first marry. This latitude is evidenced in the fairly wide dispersion in the ages at which men experienced these transition events. Beginning during the 1940s, and especially during the postwar period, the typical age at which males made these transitions became more concentrated on a few modal ages within a relatively narrow age range. Both average age at school completion and age at the beginning of first full-time job after the completion of schooling increased moderately due to increases in average years of school completed. Age at first marriage declined precipitously during the early postwar period (Winsborough, 1978; Modell et al., 1976; 1978).

The age patterns of these events indicate the "social times" in the life course of individuals during which the events typically occur.

> The notion of social time is based on the meanings of age, and refers to the ordering of events and social roles by age-linked expectations, sanctions, and options. The variable meanings of age represent social constructions which take the form of age norms and sanctions, [and] social timetables for the occurrence and arrangements of events. . . . (Elder, 1978:25–6).

What evidence is available indicates that individuals in American society express broad agreement regarding the appropriate ages for various transition events, and that this normative concept extends to specify not only the ideal age for each event, but also suitable age ranges—outside of which an individual is either "early" or "late" for the transition in question (Neugarten et al., 1965; Elder, 1974; Modell, 1979). The post-war changes in transition behavior imply that the old, accepted norms regarding the appropriate social times for these transition events may themselves have changed. Historical changes in normative beliefs about timing schedules are difficult to document, but what research has been done suggests that changes have occurred (see, for example, Modell [1979], for evidence about postwar changes in age-at-marriage norms.

---

* Direct all communications to: Dennis P. Hogan; Population Research Center; Department of Sociology; The University of Chicago; 1126 East 59th Street; Chicago, IL 60637.

An earlier version of this paper was presented at the annual meeting of the Rural Sociological Society in Burlington, Vermont, August 23–26, 1979. This research was supported by a Spencer Research Grant from the Division of Social Sciences, The University of Chicago; and by the Population Research Center. Computational work was capably carried out by Michele Pazul, and the paper was typed by Faye Gilbert and Susan Sherry. I thank Clifford Clogg, Glen Elder, David Featherman, and Rachel Rosenfeld for their comments. Any opinions, findings, conclusions, or recommendations are those of the author and do not necessarily reflect the views of any agency supporting this work.

The normative schedules regarding transition events not only specify the appropriate age for each event, but also prescribe an appropriate sequence of those events (Elder, 1974; 1978). For example, it is normatively preferred to delay marriage until schooling is completed and the couple can be self-supporting (Modell, 1979; Neugarten et al., 1965). The passage to adulthood is achieved in a socially prescribed way when a man first finishes his formal schooling, next becomes financially independent through employment at a full-time job, and finally forms his own family of procreation through marriage.

The actual timing of first marriage among American men is closely associated with age at school completion, with men showing a pronounced tendency to delay marriage until after the completion of schooling (Hogan, 1978a; 1978b; Winsborough, 1978). The timing of the early life-cycle events through which American boys pass in their transition to adulthood appears to be governed by an underlying tendency to order those events in a normatively preferred order (Hogan, 1978b). Men, therefore, can be characterized by their degrees of conformity to this hypothesized normative-ordering pattern (Hogan, 1978b).

As typical ages for school completion, first job, and first marriage have become more concentrated within narrow bands of modal ages, and, as the average age for each event has become more similar, the period of overall transition time to adulthood has narrowed, resulting in a more concentrated pattern of status changes (Elder, 1978; Winsborough, 1978; Modell et al., 1976). It has consequently become increasingly difficult for men to achieve the events marking the transition to adulthood in a normatively prescribed order (Hogan, 1978b).

Indeed, the ability of men to order their life-course events in a normative fashion varies by the unique history of the birth cohort into which they are born and, in particular, by the military service and educational experiences characteristic of their cohort. In general, time-consuming activities during the period of early adulthood—such as the achievement of advanced levels of education (undergraduate and/or graduate and professional training), or service in the military—result in postponements or preponements of school completion, first job, or first marriage that are disruptive of the usual sequence of life-cycle transitions (Hogan, 1978b; Mason, 1970).

Military service and higher education are particularly disruptive of the typical sequence of events among men born during the 1920s. Such men carried the heaviest burden of manpower supply during World War II and, thus, had little discretion in the timing of their military service. As a consequence, many more of these men married prior to the completion of schooling and/or beginning of work than did men born into different times (Hogan, 1978b).

Such a disorderly sequence of life-cycle events may be viewed as disrupting the harmony which usually exists between a man's own life style and the institutional structure that provides the social context in which he lives. It has been hypothesized that the consequences of such disharmonies with his social environment may persist over a man's life course (Elder, 1978; Hogan, 1978b). Previous research has demonstrated that men who deviate from the normative ordering of events experience higher rates of marital instability than men who make the transition to adulthood in a normative fashion (Hogan, 1978b). This paper (by examining the consequences of ordering pattern for occupational and earnings attainments in the later career) reports additional tests of the hypothesis that the variable order of events in the life course is an important life-cycle contingency.

Previous studies of the effects of the timing of life-cycle transition events on occupational status and earnings attainments have considered the timing of each event separately. Thus, age at school completion is usually divided into its component parts (age at beginning of school, level of educational attainment, years of educational disruption, and years of age-grade retardation or acceleration), and the consequences of these component parts are then examined. For example, Featherman and Carter (1976) report that among a cohort of men born between July 1, 1939, and June 30, 1940, those men who delayed postsecondary education after leaving high school, or who interrupted their posthigh school education, ultimately attained fewer years of education, even after controls for socioeconomic background, ability, and aspirations. Furthermore, men who were age-grade retarded in high school earned an average $2,400 less annually (when interviewed in 1972) than did men with similar background characteristics who did not experience such retardation.

It has proven difficult to disentangle the socioeconomic consequences of age at first job from the consequences of age at school completion because of their intricate interconnections (Duncan et al., 1972; Ornstein, 1976). Men who begin their first jobs relatively early have less advantageous family backgrounds and tend to remain at their first jobs for somewhat longer periods of time (Duncan, 1968; Ornstein, 1976). The "early" job takers achieve slightly lower levels of occupational attainments (2 to 3 points on the Duncan scale) compared with "late" job takers at similar levels of education. In part, however, this negative relationship of age at first job to occupational attainments may result from the earlier age at first marriage which is characteristic of early job takers (Duncan et al., 1972).

An early age at first marriage has a negative relationship to economic position in the early years of the life cycle, as does a very short interval between marriage and birth of the first child (Duncan et al., 1972; Coombs and Freedman, 1970; Coombs et al., 1970; Freedman and Coombs, 1966). A substantial component of this negative association is explained by the poorer family backgrounds and lower educational attainments which are characteristic of men who marry early or have short first-birth intervals (Duncan et al., 1972; Coombs and Freedman, 1970; Coombs et al., 1970).

We thus have evidence from a number of studies showing that the ages—at which a man finishes school, begins work at a full-time civilian job, and first marries—all act as life-cycle contingencies affecting his career occupational and earnings attainments. My objective in this paper is to demonstrate that the ordering in which these life-cycle transitions occur constitutes a career contingency of some importance in the life course. This effect of the temporal ordering of events is hypothesized to occur net of the effects of the age of occurrence of each of the component events.

Data

The data source for this study of the occupational status and earnings consequences of the temporal ordering of schooling, first job, and marriage is the 1973 Occupational Changes in a Generation II survey which was carried out in conjunction with the March demographic supplement to the Current Population Survey. In 1973, the eight-page OCG questionnaire was mailed out six months after the Marsh CPS and followed by mail, telephone, and personal call-backs. The respondents, comprising 88% of the target sample, included more than 33,500 men aged 20 to 65 in the civilian noninstitutional population. Also, blacks and persons of Spanish origin were sampled at about twice the rate of whites, and almost half the black men were interviewed personally. A complete description of the survey is found in Featherman and Hauser (1978).

In this paper, I am interested in the occupational attainments of men in the postschooling, economically active years, so the analysis is limited to men aged 25 to 65 in the experienced civilian labor force. Black men have lower occupational and earnings attainments than white men with similar background characteristics (Featherman and Hauser, 1978; Blau and Duncan, 1967), and black men less often make the transition to adulthood in a normative fashion compared with white men at similar levels of schooling (Hogan, 1978b). In order to estimate the pure effects of ordering pattern on career attainments—without contaminating the estimates by the effects of race—the analysis in this paper is restricted to white men. Finally, so as to remove the effects of nonmarriage on career attainments, the analysis is limited to men who have been married. These restrictions produce a population of analysis that includes ever-married white men aged 25 to 65 who are in the experienced civilian labor force.

Temporal Ordering Scale

The scale of temporal ordering used here was developed in Hogan (1978b). The scale is designed to measure the variation in the normativeness of the ordering pattern of transition events. The scale is based on the idea that the three events marking the passage to adulthood occur in a normative order only when a man first completes school, next starts to work, and lastly marries. A single inversion from this normative rank order occurs when a man either begins a job prior to finishing school, or marries prior to beginning work but after the completion of schooling. An extreme nonnormative ordering pattern occurs whenever a man marries prior to the completion of his education; this produces at least two inversions in the normative order.

All OCG respondents answered questions about the dates (month and year) in which they completed school, began their first full-time civilian job, and first married. In order to be classifiable on this three-category temporal ordering scale, a respondent had to provide information on at least the year during which he had completed each event.[1] These dates were used to classify the respondents into the three ordering categories. In cases where two events occurred during the same month, the ordering category closest to the normative pattern was assigned.[2]

Levels of Socioeconomic Background and Attainments

Men who completed the transition to adulthood in a normative fashion were from poorer socioeconomic backgrounds (i.e., their fathers' occupational status, fathers' education, and family income at age 16 were all lower than average; and they had a larger number of siblings) than did men who made the transition in a nonnormative fashion (Table 1).[3] While men

---

[1] In cases where the year of an event was given by the respondent but no month was indicated, a midyear (July) month of occurrence was assumed.

[2] These restrictions on the population of analysis result in the following number of sample cases representing men with each ordering pattern: 11,288 for the normative order, 3,379 for the intermediate nonnormative order, and 3,703 for men with the extreme nonnormative order. In carrying out statistical calculations for this paper, these sample cases were adjusted by a design factor of 0.75 to reflect departures from a simple random sample (see Featherman and Hauser, 1978: Appendix B). The adjusted number of sample cases is shown in Table 2. In estimating the structural equation models for this paper, a list-wise deletion procedure was followed for cases with missing data on one or more variables in the models. The number of sample cases on which the standard errors for the structural equation coefficients are based is shown in Table 1. A comparison of the characteristics of the men included in the structural equation models of this paper with the characteristics of all white men in the OCG-II survey showed that the procedures used in this paper produced no biases greater than 5% in mean of father's occupational status, proportion farm background, father's education, respondent's education, and respondent's occupational status. I therefore conclude that the sample on which this analysis is based —despite sample attrition due to missing data—is representative of ever-married white men aged 25 to 65 who were in the experienced civilian labor force.

[3] Paternal education is scaled in years completed according to the following recode of class intervals: no school, 0.0 years; elementary (1–4), 3.3 years; elementary (5–7), 6.3 years; elementary (8), 8.0 years; high school (1–3), 9.9 years; high school (4), 12.0 years; college (1–3), 13.8 years; college (4), 16.0 years; college (5 or more), 18.0 years. Number of high school (1–3), 9.9 years; high school (4), siblings is the number of brothers and sisters (not counting respondent). Farm origin is a dummy variable, with a score of 1 indicating that respondent's father was a farmer, farm manager, farm laborer, or farm foreman. Broken family is a dummy variable, with 1 indicating that the respondent was not living with both parents most of the time up to age 16. Family income at age 16 is based on the respondent's report of his family income when he was age 16, inflated to 1972 dollars using the Consumer Price Index. To allow for the differential occupational returns and earnings returns to an additional year of schooling at the grade school and high school level compared to the returns to an additional year of college, respondent's education is scaled as a spline function, following the procedures of Featherman and Hauser (1978). Persons with 0–12 completed years of graded schooling reported to the Current Population Survey are so indicated on the graded schooling variable; men with one or more years of college completed are scored 12 on the graded schooling variable. The college variable is scored 0 for those with no college, as 1 to 4 for those with 1 to 4 years of college, respectively, and as 5 for those with 5 or more years of college completed. Father's and respondent's occupation are scored according to Duncan's socioeco-

Table 1. Means and Standard Deviations of Background and Attainment Variables by Ordering Group

Variable	Normative		Intermediate Nonnormative		Extreme Nonnormative	
	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
Father's occupation	28.17	21.35	30.31	22.44	38.99	24.89
Father's education	8.29	3.73	8.66	3.81	10.07	3.87
Farm background	0.23	0.42	0.24	0.43	0.15	0.36
Broken family	0.12	0.32	0.12	0.33	0.12	0.32
Number of siblings	3.81	2.65	3.55	2.60	2.89	2.34
Family income, age 16 (1972 dollars; thousands)	11.93	6.10	12.70	6.31	14.18	6.22
Graded schooling	10.83	2.01	11.18	1.74	11.86	0.78
College	0.69	1.44	0.91	1.58	3.02	1.94
Veteran	0.57	0.49	0.63	0.48	0.64	0.48
First job	29.35	22.21	33.42	23.30	57.42	24.68
Age at completion of schooling	17.41	3.88	18.66	4.19	28.77	7.50
Age at first job	18.62	3.66	19.99	6.96	25.74	6.67
Age at first marriage	24.09	4.80	23.25	4.56	22.48	3.56
Birth cohort						
1943–47	0.14	0.35	0.16	0.37	0.17	0.37
1938–42	0.14	0.35	0.16	0.36	0.17	0.38
1933–37	0.12	0.32	0.14	0.34	0.16	0.36
1928–32	0.13	0.33	0.13	0.33	0.15	0.36
1923–27	0.13	0.34	0.13	0.34	0.15	0.36
1918–22	0.14	0.34	0.12	0.33	0.11	0.32
1913–17	0.12	0.32	0.10	0.29	0.05	0.22
1907–12	0.09	0.28	0.07	0.25	0.03	0.18
Current occupation	40.26	24.18	42.32	24.25	61.31	22.79
Weeks worked	49.43	8.02	49.63	7.66	49.80	7.86
Earnings	11537	8219	11621	8146	14584	9433
Adjusted sample cases	6962	—	2079	—	2293	—

in the intermediate nonnormative ordering group are generally intermediate in their family background characteristics, they tend, otherwise, to more closely resemble the normative group than the extreme nonnormative group.

Men with extreme nonnormative orders are characterized by better early attainments as compared with other men. They thus enjoy an average of one more year of graded schooling and 2.3 more years of college than had men with normative orders. The intermediate ordering group is also intermediate with respect to educational attainment, but more closely resembles the normative group. Men with nonnormative orders are more likely to have spent some time in the military service. (These characteristics are in line with the findings reported in Hogan, 1978b; 1976).

Men with extreme nonnormative orders married at an average age of 22.5 which is 1.6 years younger than the average marriage age of men with normative orders. In line with their higher educational attainments, the men with nonnormative or-

---

nomic index for occupations (Duncan, 1961), since occupational socioeconomic status is the major dimension along which occupational positions persist from generation to generation (Hauser and Featherman, 1977). "Father's" occupation refers, as well, to the mother or other household head where the father was absent. Number of weeks worked by the respondent during the previous year is scaled in weeks according to the following recode of class intervals: None, 0.0 weeks; 1–13, 7.0 weeks; 14–26, 20.0 weeks; 27–39, 33.0 weeks; 40–47, 43.5 weeks; 48–49, 48.5 weeks; 50–52, 52.0 weeks. Earnings are expressed in 1972 dollars. Age at completion of schooling, age at first job, and age at first marriage are scored in single years of age. Birth cohort is represented by a series of dummy variables in order to control for differences in the birth cohort compositions of the ordering groups. A variable for years of work experience is not included since it would be highly collinear with the combination of the birth cohort and age at first job variables.

dering patterns remained in school until a later age than the men with normative orders. In fact, men with extreme nonnormative orders, even though they averaged only 3.36 more years of schooling (graded schooling and college), finished school 11.4 years later than men who had normative orders. The extreme nonnormative men also began working at the first full-time civilian jobs they held after completing their highest grade of school an average of 3.0 years before completing school.[4] This contrasts with other men who began working at full-time civilian jobs an average of 1.3 years after completing school.

The later age at first job together with the higher level of schooling of men in the nonnormative ordering category suggest that the first jobs of these men represent, actually, the beginning of their careers rather than merely the start of more tenuous "first jobs." This supposition is confirmed by the average first-job status of 57.4 points reported by the extreme nonnormative men, compared with average first job statuses of 29.4 for men with normative orders and of 33.4 for men with an intermediate ordering pattern.

It is not surprising, given their better socioeconomic backgrounds and above average early attainments, that men with extreme nonnormative ordering patterns report higher-status occupations and higher earnings as of 1973, compared to men with intermediate nonnormative and normative orders. Thus, the current occupational statuses of the men who marry prior to completing school are an average 21 points higher than those of men with normative orders, and their annual earnings are about $3,000 higher.

These simple comparisons run strongly counter to the hypothesis that nonconformity with normative ordering patterns in the transition to adulthood will reduce later career attainments. The remainder of this paper reports more detailed analyses which demonstrate that this apparent advantage of men with nonnormative orders disappears when their more favorable social backgrounds and early attainments are taken into account. While the results do not confirm the hypothesis for occupational attainments, they provide strong confirmation for the hypothesis in regard to earnings attainments. The paper concludes with a discussion of various factors that may be producing these results.

The Process of Occupational Attainment

The analysis of occupational attainment processes traditionally focuses on either intergenerational occupational mobility models or on structural equation models which show occupational status attainments as a function of family background and education (Blau and Duncan, 1967; Featherman and Hauser, 1978). In order to test fully for differences among the three groups in the process of occupational attainments, I carried out both types of analyses.

A comparison of the intergenerational occupational mobility processes of the three ordering groups indicates that the minimum degree occupational mobility between fathers and sons is 41% among men with extreme nonnormative orders, but only 24% among men with other ordering patterns.[5] Most of this difference is accounted for by the very pronounced recruitment, into the salaried professions, of men who have extreme nonnormative orders. Among such men, 27.4% more are in the salaried occupations (compared to their fathers), while the shift is only 6.6% among men with intermediate orders and only 4.7% among men with normative orders. Despite these differences in minimum mobility, rates of expected and observed mobility are virtually identical.

In order to control for the effects of structural mobility, a proportional adjustment procedure was carried out, which adjusted the margins of each matrix to those of the normative ordering group (see Hauser and Featherman, 1977).

---

[4] This is the definition of first job in the OCG-II survey. An alternative definition of first job would perhaps have been preferable for this analysis, but the bias resulting from the OCG-II definition is probably slight. See Hogan (1978b; 1976) for a complete discussion of the first job variable.

[5] A 17-category major occupation group classification is used. The occupational classification is the same as that used by Blau and Duncan, 1967, and Featherman and Hauser, 1978.

Table 2. Regression Analysis of Occupational Attainment by Ordering Group

	Ordering Group					
	Normative		Intermediate Nonnormative		Extreme Nonnormative	
Variable	Regression Coefficient	Standard Error	Regression Coefficient	Standard Error	Regression Coefficient	Standard Error
Father's occupation	0.07*	0.01	0.07*	0.02	0.05*	0.02
Father's education	0.10	0.07	0.08	0.13	0.15	0.11
Farm background	−1.95*	0.60	−3.03*	1.12	−2.47*	1.09
Broken family	0.85	0.70	−0.34	1.26	0.46	1.09
Number of siblings	−0.26*	0.09	−0.12	0.18	−0.39*	0.16
Graded schooling	2.31*	0.14	1.93*	0.28	1.08*	0.47
College	4.46*	0.22	4.10*	0.37	3.01*	0.24
Veteran	1.06*	0.50	1.97*	0.92	0.95	0.81
First job	0.31*	0.01	0.34*	0.02	0.41*	0.02
Age at completion of schooling'	0.07	0.10	−0.09	0.13	−0.01	0.06
Age at first job	−0.27*	0.10	−0.01	0.06	−0.09	0.07
Age at first marriage	−0.16*	0.05	−0.20	0.11	0.20	0.12
Birth cohort						
1943–47	−7.85*	1.01	−4.82*	1.98	−4.09	2.20
1938–42	−5.18*	1.00	−3.14	1.98	−3.38	2.15
1933–37	−2.23*	1.03	−0.89	1.99	−0.56	2.16
1928–32	−1.17	1.02	0.05	2.02	−2.80	2.15
1923–27	−0.18	1.02	1.53	2.01	1.07	2.17
1918–22	0.16	1.00	1.01	2.00	1.42	2.20
1913–17	0.10	1.01	3.61	2.06	0.06	2.41
1907–12	—	—	—	—	—	—
Intercept	10.65		9.95		12.91	
R^2	.404		.415		.484	
Error of estimate	18.70		18.64		16.43	

* Regression coefficients larger than twice their standard errors.

There are no significant differences in intergenerational mobility patterns among the three groups, once differences in origin and destination marginals are taken into account.

Turning to an examination of the occupational status achievement process among the groups, we find that the superior occupational status level of those men in the extreme nonnormative group is largely accounted for by their superior educations. Direct standardization for level of education results in an average current job status of 44.0 among men with normative orders, 43.4 for men with intermediate nonnormative orders, and 45.9 for men with extreme nonnormative orders.[6] These standardized figures may be compared with the unadjusted figures of 39.4, 41.7, and 60.2, respectively. While standardization for education does not reverse the pattern, it does attenuate the results in a direction consistent with the hypothesis.

Can controls for other background and early achievement variables reverse the occupational status differentials among the groups, in line with the hypothesis that degree of normativeness should be positively associated with occupational status? To answer this question, separate structural equation models of the occupational status attainment process were estimated for each ordering group (Table 2).These models incorporate all of the family background variables commonly used as independent variables (see Duncan et al., 1972). Educational attainment is scored as a spline function to allow for the differing occupational returns to a year of graded schooling and a year of college (see Featherman and Hauser, 1978, for a discussion of this specification). Veteran status and birth cohort are both included as control variables because both characteristics of an individual are known to be associated wth his ordering pattern, and are probably related to his occupational attainment (see Fligstein, 1976). Age at the completion of schooling, beginning of first job, and first marriage are included so that any differences observed among the ordering groups are due to the ordering of the component events, and not to the individual timing of one or more of those events.

---

[6] The education distribution for the entire population of analysis was used as the standard.

The sequence of transition events does not affect occupational status returns to family background, but it does have substantial consequences for the occupational returns to education, military service, and first job status.[7] As hypothesized, men with normative orders receive higher returns to a year of graded schooling than men who marry prior to completing schooling (2.31 occupational status points versus 1.08 points), and their occupational returns to years of college are also higher (4.46 versus 3.01). As anticipated, the occupational returns to a year of graded schooling and to a year of college are intermediate for the intermediate ordering group. A disorderly pattern of events marking the transition to adulthood thus reduces the returns a man gains for each additional year of schooling. Therefore, although advanced schooling is desirable for men as a way to increase their occupational achievements, the maximum occupational returns to schooling can be received only if their schooling is completed prior to both the beginning of their first full-time civilian job and their first marriage, controlling for other variables.

A disorderly pattern of events increases the occupational status returns to first job. This confirms that the first job is more important for men who begin that job relatively late in life, after considerable schooling, because it represents the beginning of a career trajectory. For men who begin their first job at a younger age, but after the completion of schooling, the first job status is not as closely tied to later occupational attainments because it may represent a more tenuous sort of labor force (and career) attachment.

Structural equation models omitting the first-job variable show a pattern of differential occupational returns to education in a much attenuated form. This indicates that part of the reason for the lower returns to an additional year of education among men with nonnormative ordering patterns is due to the greater importance of first job as an intervening variable among such men.

In order to decompose the source of the higher overall occupational status attainments of men with nonnormative ordering patterns into components due to differences in background and to differences in process, the means of the independent variables for each of the nonnormative ordering groups were inserted into the structural equation models estimated for men with normative orders, and predicted occupational attainments were calculated. A comparison of the predicted and actual scores permits an assessment of the penalty or advantage experienced by each of the nonnormative groups as a result of differences in the process of occupational attainment (Table 3).[8]

Most of the 2.06 occupational status-point advantage of men with intermediate nonnormative orders compared to men with normative orders is due to their better social origins (1.13) and higher educations (1.58). Men with intermediate nonnormative orders suffer a 0.88 status-point disadvantage in the process of attainment, compared to men with normative orders. That is, men with the same characteristics on the independent variables as these men would have occupational status scores an average of 0.88 points higher if they experienced transition events in a normative way.

Superior social origins account for 5.60 of the 21.05 status-point advantage of men with extreme nonnormative orders compared to those with normative orders, and their greater years of graded schooling and college account for an additional 14.64

---

[7] The structural equation models for the occupational attainment process differ significantly among the ordering groups ($F = 1.56$, with 41 and 11,274 degrees of freedom; $p < .02$).

[8] This type of multiple regression standardization procedure has been widely used (see Duncan, 1968; Althauser and Wigler, 1972: Hogan and Featherman, 1977).

points of their occupational status advantage. Net of the effects of social background and education, the better first jobs of men with extreme nonnormative orders adds 1.70 points to their average status scores. The overall penalty suffered for an extreme nonnormative ordering of events is a 0.60 point deficit in expected occupational status.

This analysis of the occupational attainment process does not confirm the hypothesis that a disorderly sequencing of events marking the transition to adulthood reduces expected levels of occupational attainment. While the net returns to a year of schooling are less among such men, this is mainly a function of the greater importance of first job as an intervening variable. Overall, the net deficit suffered as a result of a disorderly sequencing of events is less than one occupational status point. Therefore, the ordering of transition events leading to adulthood does not affect occupational attainments in the later career. The models also indicate that the age of occurrence of each of the events marking the transition to adulthood have only trivial effects on occupational attainments.

Table 3. Components of Differences between Occupational and Earnings Attainments of Intermediate and Extreme Nonnormative Ordering Groups and Men with Normative Ordering of Events*

	Ordering Group	
Attainment and Components	Intermediate Nonnormative	Extreme Nonnormative
Occupational Status:		
Social origins	1.13	5.60
Education	1.58	14.64
Military service	0.08	0.06
First job	0.44	1.70
Age at events	−0.25	−0.87
Birth cohort	−0.04	0.52
Residual	−0.88	−0.60
Total difference	2.06	21.05
Earnings:		
Social origins	338	1285
Education	285	3123
Military service	24	23
First job	59	241
Age at events	−76	−467
Birth cohort	−17	268
Occupational status	−79	−33
Weeks worked	0	−65
Residual	−450	−1329
Total difference	84	3046

* The structural equations for men with a normative ordering pattern are used for the standardization.

The Process of Earnings Attainments

The next step in the research is to test the hypothesis that men with nonnormative ordering patterns suffer earnings deficits as a result of their distinctive ordering patterns. As already mentioned, the gross earnings differentials are patterned exactly opposite the hypothesized ordering, with men with extreme nonnormative orders earning $14,251 in 1972, compared to $11,550 for men with intermediate ordering patterns, and to $11,320 for men with a normative ordering pattern. But this is not surprising, given the better educations of men with deviant ordering patterns.

Comparing the earnings of men with different ordering patterns at each level of education, we find the expected differentials emerge among men who are high school graduates or better (Table 4). Among high school graduates, men with normative orders have average earnings $328 higher than men with extreme nonnormative orders. This differential increases with higher levels of education to $524 among men with one to three years of college, $988 among men with four years of college, and $5,124 among men with one or more years of graduate or professional schooling. The intermediate group has an intermediate earnings position only among men with one to three years of college or with graduate or professional training. Standardizing for education, the three ordering groups show the hypothesized pattern, with men with normative orders earning $12,407, men with intermediate nonnormative orders earning $12,018, and men with extreme nonnormative patterns earning $11,725.

The earnings advantage accruing to men with normative ordering patterns is concentrated among men who are in the professional and managerial occupations or are farmers, when standardized for educational differentials among the ordering

Table 4. Mean Earnings in 1972 by Educational Level for Ordering Group

	Ordering Group			
Educational Level	Normative	Intermediate Nonnormative	Extreme Nonnormative	Total
None	—[a]	—	—	—
Elementary 1–4	6,581	(7,359)	—	6,747
Elementary 5–7	7,603	7,621	—	7,635
Elementary 8	8,498	8,364	(8,149)	8,465
High school 1–3	9,570	9,579	9,983	9,587
High school 4	11,211	10,750	10,883	11,083
College 1–3	12,989	12,875	12,465	12,788
College 4	16,674	16,745	15,686	16,252
College 5 or more	22,361	20,047	17,237	18,659
Total	11,320	11,550	14,251	11,953
Total (standardized for education)[b]	12,407	12,018	11,725	11,953
Adjusted sample size	8,466	2,534	2,777	13,777

[a] Mean earnings are not shown for cells with fewer than 25 cases; means for cells with 25 to 49 cases are shown in parentheses.

[b] Directly standardized, using the education distribution for the total population as the standard population.

groups within each category of occupation (Table 5). This difference amounts to $1,258 among the professionals; $1,440 among the managers, officials, and proprietors; and $907 among the farmers. The men with intermediate ordering patterns have intermediate earnings patterns among professionals, and among managers, officials, and proprietors, but not among farmers. There appear to be no earnings differentials among the ordering groups for men in the lower white-collar and blue-collar occupations.

Structural equation models for the earnings attainment process can help explain the source of these differentials by indicating the total magnitude of the earnings deficit suffered by men with disorderly transitions to adulthood, and by showing what differences in process produce these earnings deficits. The earnings returns to an additional year of graded schooling and of college differ markedly by ordering pattern (Table 6).[9] Whereas men with normative orders earn $218 for each additional year of graded school and $911 for each additional year of college, men with intermediate nonnormative orders enjoy lesser returns of $140 and $786, respectively, and men with extreme nonnormative orders get a nonsignificant return to additional years of graded schooling, and only $414 for each additional year of college. A one-year delay in the completion of schooling or age at first job appears to depress the earnings of men with nonnormative orders, although the size of the loss remains small.

The better social origins of men with extreme nonnormative orderings account for $1,285 of their $3,046 earnings advantage over men with normative origins (Table 4). Their better educations provided them with an additional $3,123 in earnings. Their better first jobs and more favorable birth cohorts add about $250 (each) to their earnings advantage. But differences in the process of earnings attainments (between men with extreme nonnormative orders and those with normative orders) penalizes, by a total of $1,329, the men who marry prior to finishing school. As the detailed examination of the structural equations model indicated, this penalty is primarily due to their poorer earnings returns to graded schooling and college.

The men with intermediate nonnormative ordering patterns have gross earnings that are about equal to the earnings of men with normative orders. But this results from the counterbalancing effects of their better social origins and educations—which add $338 and $285, respectively, to

---

[9] The structural equation models for earnings attainment differ significantly among the groups ($F = 2.64$, with 45 and 11,265 degrees of freedom; $p < .01$).

Table 5. Mean Earnings in 1972 by Occupational Level For Ordering Groups

	Ordering Group			
Occupational Level	Normative	Intermediate Nonnormative	Extreme Nonnormative	Total
	Observed Means			
Professionals	16,155	15,596	15,791	15,897
Managers, officials, and proprietors	15,209	15,230	16,495	15,527
Clerical and sales	11,465	11,785	12,860	11,795
Crafts	10,574	10,434	11,164	10,608
Labor, operatives, and service	8,742	8,823	9,570	8,832
Farm	6,799	7,113	9,198	7,040
Total	11,320	11,550	14,251	11,953
	Standardized for Education[a]			
Professionals	16,495	16,338	15,237	15,897
Managers, officials, and proprietors	16,029	15,729	14,589	15,527
Clerical and sales	11,851	11,903	11,975	11,796
Crafts	10,657	10,398	10,600	10,608
Labor, operatives, and service	8,803	8,749	8,589	8,833
Farm	6,893	7,022	5,986	7,040
Total	12,232	11,979	11,780	11,953

[a] Directly standardized, using the education distribution for the total population of the same occupation group as the standard population; the education variable was categorized into four categories—less than high school, high school, some college, and college graduate.

their average earnings—and of the earnings penalty of $450 suffered as a result of differences in process. As in the cases of the extreme nonnormative men, this penalty operates primarily via reduced earnings returns to graded schooling and college.[10]

Thus, the sequential ordering pattern of life-course transition events has an important impact on the earnings attainments of men in their later careers, but does not substantially affect occupational attainments (measured by occupation group or by occupational status score). The earnings penalty for having transition events out of their appropriate sequence amounts to $450 among men who marry after completing schooling, and to $1,329 among men who marry prior to completing their schooling.

This pattern of differential earnings returns to a year of schooling cannot be attributed to a differential rate of school disruptions among the out-of-order groups. Restricting the analysis to men with no interruptions in their schooling, the earnings differential between men with normative orders and those with intermediate nonnormative orders amounts to $285, and the earnings penalty for marrying prior to completing school is $1,243. The earnings differentials by ordering group are concentrated among men with one or more years of college who are professionals, managers, officials, or proprietors. Restricting the analysis to this

---

[10] The structural equation models reported used earnings rather than the natural log of earnings as the dependent variable. My substantive interest in this paper is on the actual process of earnings returns among the ordering groups, and not on determinants of relative earnings position (as would be provided by a semilogged structural equation model for earnings attainment). In any case, a regression analysis using log-earnings as the dependent variable produced the same pattern of differential returns to education for the ordering groups. The relative earnings returns to an additional year of graded schooling, controlling for all other independent variables, are .033 for men with normative orders, .018 for men with intermediate orders, and .026 for men with extreme nonnormative orders. The comparable relative earnings returns to an additional year of college are .041, .039, and .021 for the ordering groups, respectively. When the three ordering groups are included in a single equation, the differences in intercept are in the expected direction, with an intermediate nonnormative ordering pattern reducing relative earnings position by .030, and an extreme nonnormative order reducing relative earnings position by .063.

Table 6. Regression Analysis of Earnings Attainment by Ordering Group

	Ordering Group					
	Normative		Intermediate Nonnormative		Extreme Nonnormative	
Variable	Regression Coefficient	Standard Error	Regression Coefficient	Standard Error	Regression Coefficient	Standard Error
Father's occupation	2	5	7	9	13	9
Father's education	12	28	−28	51	46	56
Farm background	−254	229	−451	430	−674	549
Broken family	−283	270	72	486	−58	550
Number of siblings	−4	36	−19	67	−160	81
Family income, age 16 (thousands)	102*	17	100*	29	72*	32
Graded schooling	218*	54	140	110	−27	233
College	911*	88	786*	145	414*	125
Veteran	108	190	529	350	383	407
First job	16*	6	34*	10	27*	10
Age at completion of schooling	−16	37	−23	50	−34	32
Age at first job	−40	39	−52*	25	−151*	34
Age at first marriage	−35	21	−165*	40	−61	59
Birth cohort						
1943–47	−1630*	390	−2414*	759	−4686*	1103
1938–42	109	385	−531	757	−2365*	1081
1933–37	844*	395	840	760	−56	1082
1928–32	1358*	389	1107	771	892	1082
1923–27	1111*	391	57	769	1581	1093
1918–22	998*	385	282	767	2092	1108
1913–17	877*	387	1414	786	2276	1209
1909–12	—	—	—	—	—	—
Occupation	77*	5	69*	8	88*	11
Weeks worked	172*	11	179*	21	215*	22
Intercept	−3446	—	314	—	1061	—
R^2	.244	—	.246	—	.245	
Error of estimate	7155	—	7110	—	8235	

* Regression coefficients larger than twice their standard errors.

population, the structural equation models show a $514 earnings deficit for men with an intermediate ordering pattern and a $1,479 deficit for men who marry prior to completing school. These differentials persist when controls are introduced for five-year job-tenure cohorts.

These findings apply to men in both their early and later careers. Separate structural equation models were estimated for each of the ordering groups for men aged 26 to 39 and 40 to 65. There were no differences in occupational attainments, overall, at any age, although a pattern of reduced occupational status returns to each year of school, similar to that for the population aged 25 to 65, are observed. The expected pattern of earnings returns, by ordering group, is observed for all groups except the older men with intermediate nonnormative orders. Thus, among younger men, the earnings returns to an additional year of college are $754 for men with normative orders, $574 for men with intermediate orders, and $428 for men with extreme nonnormative orders. Among older men, the comparable earnings returns are $1,097, $1,070, and $458, respectively. The younger men with intermediate ordering patterns experience a total deficit of $185 and those with extreme nonnormative orders have earnings $874 lower than men with normative orders. Among older men, those with intermediate ordering patterns experience no overall earnings deficit, but men who marry prior to finishing school have earnings $1,558 less than would otherwise be expected.

Summary and Conclusions

Throughout the twentieth century, American society has become more distinctly age-graded, with certain chronological ages being recognized as appropriate for a variety of social roles. This historical tendency toward sharper age-grade distinctions has become especially pronounced since World War II. Whereas males making the transition from youth to adulthood in the prewar period faced a relatively loosely defined set of ages for the completion of schooling, the beginning of work, and marrying, a substantial narrowing of the variation in the usual age-range for each of these events occurred in the postwar period. Furthermore, the average age at school completion and at the beginning of first full-time civilian job following school increased somewhat in the postwar period, while the average age at first marriage declined substantially. As a result, it became increasingly difficult (for those young men making the transition to adulthood during the postwar years) to adhere to the normative timetable for each transition event while, simultaneously, maintaining the sequence of transition events that is normatively preferred. This is reflected in a higher proportion of men making the transition to adulthood in a nonnormative fashion during the postwar period.

It is especially difficult for men who attend college, graduate, or professional school to maintain a normative ordering of events, because an extended postponement of marriage until after the completion of schooling will make them "late" marriers, in relation to other members of their cohort. Such a postponement could, if strictly adhered to, even result in the loss of a desirable marriage partner who might decide to marry someone else rather than wait and, then, herself become "late" on the normal marriage schedule of her cohort. As a result of the joint effects of the norms regarding the desirability of a college education and marriage at the "right" age, some men marry prior to completing their schooling, thereby violating the normative proscription that schooling should be completed prior to marriage.

While this violation of the ordering norm may be preferable to dropping out of school (to the detriment of career socioeconomic attainments) or to postponing marriage until a later age than usual (and risking the loss of a suitable marriage partner), it does not occur with impunity. Men who marry prior to the completion of schooling receive much lower earnings returns to a year of graded school and a year of college than do men who adhere to the ordering norm. Overall, this deficit amounts to over $1,329 per year. The penalty is most substantial among men who are college graduates, especially those with one or more years of graduate or professional school. The earnings deficit is also most pronounced among men who are in the higher-status white-collar occupations. Men with intermediate nonnormative orders experience a somewhat smaller—but nonetheless substantial—earnings deficit of $450.

The analysis presented here indicates that a deviant ordering of transition events causes deficits in earnings, but not in occupational status attainments. Why are occupational attainments unaffected by the ordering of transition events? What is the mechanism that produces lower rates of earnings returns for men with deviant ordering patterns? There are a number of possible answers to these questions. While the data set used in this analysis is appropriate for testing the consequences of transition-event ordering patterns, it does not readily lend itself to the test of these alternative hypotheses. The hypotheses I mention will, therefore, be speculative; if they are found sufficiently compelling, they may provide a basis for future research.

The first possibility is that men who marry prior to the completion of school, and, to a lesser extent, men who marry before beginning their first job (but after completing school) may be less geographically mobile than single men, and this depresses their earnings. This hypothesis is based on two related ideas: (1) that men who are married are less geographically mobile than single men, and (2) that geographic mobility is an important mechanism by which men place themselves in more favorable job markets where higher wages are available (see Eldridge and Thomas, 1964; Blau and Duncan, 1967; Featherman and Hauser, 1978). The potential earnings gains from migration have historically been the greatest for highly trained men in the higher-status occupations (Featherman and Hauser, 1978), possibly accounting for the reason that earnings deficits are associated with deviant ordering patterns among only the better-educated, higher-status white-collar occupation men.

A second possibility is that men who are working and/or married while in school may spend less time on their academic studies than do men who are single while in college, graduate, or professional school. Such men possibly would obtain a smaller intellectual return from each additional year of schooling. Men with nonnormative transition orders therefore would have lower educational attainments (measured by achievement scores) than would men with normative transition orders who have the same number of years of schooling. To the extent that educational attainments promote on-the-job productivity, men with nonnormative orders would be less productive on the job. This might be reflected in lower wages, salaries, commissions, and profits accruing, overall, to such men. If employers hire men on the basis of years of schooling (or highest degree completed), they would not discriminate against married men in hiring them for their first jobs following school completion. In fact, they may prefer such men because of the presumption that they will prove to be more "stable" employees than single men. Also, at the higher-degree levels, many of these men will be self-employed and their ordering patterns would not affect their occupations. The lower productivity of the men with nonnormative orders would become apparent only after they begin work, and would then result only in lowered earnings (not in effect upon occupation choice). While this explanation is certainly plausible, it is complex, and will prove very difficult to test empirically.

Meanwhile, skeptics may wish to offer the alternative "beer-guzzler" hypothesis, which states that single students spend more time partying and less time doing "serious" studying than do students who are married and/or working.

A third possible explanation of the findings of this analysis is that men who marry prior to completing school and/or beginning work prefer to choose family achievements over career achievements. This type of choice, if made repeatedly over the life cycle, would ultimately depress career achievements below the level attained by men at similar levels of education who choose to emphasize career over family concerns. Bailyn (1976) and Bailyn and Schein (1976) have drawn attention to such an "accommodative" pattern in which the demands of work are subordinated to those of the family. Their study of 1,300 M.I.T. alumni suggests that such an accommodative pattern may act as a career detriment, particularly in technical and entrepreneurial occupations. While the accommodative solution of the conflict between family roles and work roles may depress achievements in a given occupation (in particular, earnings attainments), and may hinder upward occupational mobility beyond the career beginning, it may be insufficiently detrimental to cause downward career occupational mobility. In this way, ordering patterns of transition events would have a greater impact on earnings than on career attainments.

Regardless of which, if any, of these alternative explanations accounts, in the later career, for the earnings deficits that are associated with a disorderly sequencing of the transition events leading to adulthood, the research reported here has demonstrated the importance of a life-course perspective in understanding the family and career histories of American men. American men attempt to order the completion of schooling, beginning of first job, and first marriage in a normative fashion. At the same time, they attempt to adhere to normative constraints regarding the appropriate ages for each of these transition events. These normative constraints often conflict with the desire to enhance career chances through the achievement of higher levels of education. Those men who resolve this conflict by violating the norms regarding the appropriate ordering of transition events experience reduced earnings returns to additional years of graded schooling and college, and they suffer substantial earnings deficits during their later careers.

None of the alternative explanations I have offered for this finding suggests that it is the imposition of societal sanctions (resulting from the violation of the ordering norm per se) that has produced the earnings deficits of men violating those norms. Rather, they suggest that, in the educational and career achievement processes, there are structural factors that account for the earnings deficit associated with a nonnormative transition to adulthood. Thus, the scheduling of events (timing, spacing, and sequencing) by which American males make the transition from adolescence to adulthood has lasting consequences for career attainments in the later life course.

REFERENCES

Althauser, Robert P., and Michael Wigler
1972 "Standardization and component analysis." Sociological Methods and Research 1:97–135.

Bailyn, Lotte
1976 "Involvement and accommodation in technical careers: an inquiry into the relation to work at mid-career." Pp. 109–32 in J. Van Maaren (ed.), Organizational Careers. London: Wiley.

Bailyn, Lotte, and Edgar Schein
1976 "Life/career considerations as indicators of quality of employment." Pp. 151–68 in A. D. Biderman and T. F. Drury (eds.), Measuring Work Quality for Social Reporting. Beverly Hills, Calif.: Sage.

Blau, Peter M., and Otis Dudley Duncan
1967 The American Occupational Structure. New York: Wiley.

Coombs, Lolagene C., and Ronald Freedman
1970 "Pre-marital pregnancy, childspacing, and later economic achievement." Population Studies 24:389–412.

Duncan, Beverly
1968 "Trends in output and distribution of schooling." Pp. 601–74 in E. B. Sheldon and W. E. Moore (eds.), Indicators of Social Change: Concepts and Measurements. New York: Russell Sage Foundation.

Duncan, Otis Dudley
1968 "Inheritance of poverty or inheritance of race?" Pp. 85–110 in Daniel P. Moynihan (ed.), On Understanding Poverty. New York: Basic.

Duncan, Otis Dudley, David L. Featherman, and Beverly Duncan
1972 Socioeconomic Background and Achievement. New York: Seminar.

Elder, Glen H., Jr.
1974 "Age differentiation and the life course." Pp. 165–90 in Alex Inkeles (ed.), Annual Review of Sociology, 1975, Vol. 1. Palo Alto, Calif.: Annual Reviews.
1978 "Approaches to social change and the family." American Journal of Sociology 84:S1–S38.

Eldridge, Hope T., and Dorothy Swaine Thomas
1964 "Demographic analyses and interrelations." Vol. 3 in S. S. Kuznets and D. S. Thomas, (eds.), Population Redistribution and Economic Growth: United States, 1870–1950. Philadelphia: American Philosophical Society.

Featherman, David L., and T. Michael Carter
1976 "Discontinuities in schooling and the socioeconomic life cycle." Pp. 133–60 in W. H. Sewell, R. M. Hauser, and D. L. Featherman (eds.), Schooling and Achievement in American Society. New York: Academic.

Featherman, David L., and Robert M. Hauser
1978 Opportunity and Change. New York: Academic.

Fligstein, Neil D.
1976 "The effects of the G.I. Bill on educational and occupational achievement of American men, 1940–1973." Master's thesis, Department of Sociology, University of Wisconsin-Madison.

Freedman, Ronald, and Lolagene Coombs
1966 "Childspacing and family economic position." American Sociological Review 31:631–48.

Hauser, Robert M., and David L. Featherman
1977 The Process of Stratification: Trends and Analysis. New York: Academic.

Hogan, Dennis P.
1976 "The passage of American men from family of orientation to family of procreation: patterns, timing, and determinants." Ph.D. dissertation, Department of Sociology, University of Wisconsin-Madison.
1978a "The effects of demographic factors, family, background, and early job achievement on age at marriage." Demography 15:161–75.
1978b "The variable order of events in the life course." American Sociological Review 43:573–86.

Hogan, Dennis P., and David L. Featherman
1977 "Racial stratification and socioeconomic change in the American north and south." American Journal of Sociology 83:100–26.

Mason, William M.
1970 "On the socioeconomic effects of military service." Ph.D. dissertation, Department of Sociology, University of Chicago.

Modell, John
1979 "The marriage transition: normative aspects of change since World War II." Paper presented at the annual meeting of the Population Association of America, Philadelphia, in April.

Modell, John, Frank F. Furstenberg, Jr., and Theodore Hershberg
1976 "Social change and transitions to adulthood in historical perspective." Journal of Family History 1:7–32.

Modell, John, Frank F. Furstenberg, Jr., and Douglas Strong
1978 "The timing of marriage in the transition to adulthood: continuity and change, 1860–1975." American Journal of Sociology 84:S120–S150.
Neugarten, B. L., J. W. Moore, and J. C. Lowe
1965 "Age norms, age constraints, and adult socialization." American Journal of Sociology 70:710–7.
Ornstein, Michael D.
1976 Entry into the American Labor Force. New York: Academic.
Winsborough, Halliman H.
1978 "Statistical histories of the life cycle of birth cohorts: the transition from schoolboy to adult male." Pp. 231–59 in Karl E. Taeuber, L. L. Bumpass, and J. A. Sweet (eds.), Social Demography. New York: Academic.

AN OPPORTUNITY LABOR DEMAND MODEL AND MARKOVIAN LABOR SUPPLY MODELS: COMPARATIVE TESTS IN AN ORGANIZATION*

SURESH L. KONDA
SHELBY STEWMAN
Carnegie-Mellon University

American Sociological Review 1980, Vol. 45 (April):276–301

In this paper we comparatively test three models of individual mobility in an organizational internal labor market. The data are on a police work force and its managerial staff. Two of the models are Markovian; the third, a vacancy model. In each model two types of decision-makers are postulated—managers selecting individuals for promotion and workers deciding to stay or leave. Individuals are distinguished by seniority within each grade. The vacancy or labor demand model's predictive capability was consistently better than either Markovian model—for long (10 years) or short (3–5 year) term forecasts.

1. INTRODUCTION

Internal Labor Markets

In the 1950s both sociologists and economists were discussing the fragmentation of the national work force into distinct (internal) labor markets (cf. Caplow, 1954; Kerr, 1954; Ross, 1958). Recently, this perspective, which is again gaining attention in both fields (Spilerman, 1977; Stewman, 1975a; 1975b; Stolzenberg, 1975a; 1975b; Doeringer and Piore, 1971), is rounding out the large omission of institutional processes within the more prevalent perspectives of status attainment in sociology (Blau and Duncan, 1967; Duncan et al., 1972; Jencks et. al., 1972; Hauser and Featherman, 1974) and human capital in economics (Mincer, 1974; Becker, 1975).

The most comprehensive theoretical treatment of internal labor markets (ILMs) is given by Doeringer and Piore (1971). Spilerman (1977) extends their treatment by tying careers to ILMs. The theory of internal labor markets postulates that many available opportunity sets within the national pool of jobs have restricted entry ports and internal rules regarding mobility and wages. Moreover, different ILM processes operate at different levels of analyses—organizations, industries and occupations—with varying degrees of openness to the external supply of labor. Specific empirical tests demonstrating the operation of internal labor market process have been conducted by Stolzenberg (1975a; 1975b) for occupational markets and by Stewman (1975a; 1975b) for an organizational market.

---

* Direct all communications to: Shelby Stewman; School of Urban and Public Affairs; Carnegie-Mellon University; Pittsburgh, PA 15213.

This research was in part supported by a grant from the National Science Foundation (SOC 77-16240).

Individual Behavior and Job Opportunities

Since most theoretical and empirical work in both sociology and economics has not taken into account the structural effects of internal labor markets, we may raise the question of what governs individual movement within such systems. Two streams of analytical development seem relevant: (1) work on labor supply, generally involving Markovian models and (2) work on labor demand, incorporating job vacancies. Illustrative of the developments in labor supply are Tuma's (1976) semi-Markovian model, Wise's (1975) Markov chain model and Spilerman's (1977) development of the career-line construct. Both Tuma and Wise generate individual mobility parameters from a regression model of individual attributes,[1] which results in a nonstationary multivariate Markovian model of labor supply.[2] The significance of this work for the present paper lies in its attempt to further specify labor mobility beyond the simple Markov chain process. Prior significant work includes Blumen et al. (1955), Hodge (1966), McGinnis (1968), Henry et al. (1971), McFarland (1970), Spilerman (1972), Mayer (1972) and Sørenson (1975).

The second major development postulates that the nonstationarity of individual mobility arises from fluctuations in the demand side of the process. Organizational internal labor markets have been modeled from both supply and demand perspectives (see Forbes [1971] and Young and Vassiliou [1974], respectively), but from a theoretical point of view, demand models appear preferable unless some known labor supply mechanism, such as seniority, governs mobility. The foremost work on organizational demand processes is, of course, White's (1970) model of vacancy chains and Bartholomew's (1963; 1973) renewal models. Empirical tests to date strongly confirm the operation of chain reactions of vacancies in organizational job systems (White, 1970; Young and Vassiliou, 1974; Brown, 1975; Stewman, 1975a). Thus, there is strong evidence of the need for incorporating *both* labor supply and labor demand mechanisms when attempting to explain individual mobility in an organizational internal labor market.

Our purpose is to describe and comparatively test three models of individual mobility in an organization. Two of the models are Markovian; the third, a vacancy model. In each model two types of decision makers are postulated—managers selecting individuals and workers deciding to stay or leave. We know of only two other cases in which organizational labor models have been comparatively tested (Young and Vassiliou, 1974; Stewman, 1978), neither of which postulated decision processes or individual heterogeneity of work force. In short, we will make assumptions regarding individual behavior and, at least in the vacancy model, merge these assumptions with assumptions regarding the underlying structure of opportunities.

The data provide information on only one attribute—seniority in present grade—but this attribute is viewed as particularly important in affecting individual behavior in an organization. Exit rates, for example, may be expected to decline quite rapidly as seniority increases until the possibilities of retirement come into play (see Bartholomew, 1973; Young, 1965; 1971).[3] On the other hand, we know very little about how seniority interacts with hierarchical opportunity. McGinnis (1968) hypothesizes an inverted U-shaped curve for intraorganizational movement such as promotion. But, as yet, no one has empirically examined the form of these curves. Normative issues also arise when we consider relations between seniority and promotions. In a social psychological study of organizational managers in two firms, Sofer (1970) found explicit promotion expectations and "trauma" regarding stratum seniority and promotion. Certain norms were perceived about the maximum level of seniority beyond which the opportunity for promotion was negligible. In addition, Young (1971) has considered a promotion-exit linkage, which he called a "norm of fairness." He suggested that a certain level of promotion would become expected and when the organization's promotions dropped below this expected level, exit rates would increase. In subsequent analytical and empirical work, however, Young (1974) dropped this line of reasoning and pursued vacancy modeling per se. In this paper, we will consider a range of interactions between seniority and opportunity by postulating alternative models of individual mobility and by comparatively testing these models over time.

---

[1] Tuma utilized data on job attributes—occupation and industry—but since the attributes were of the individual's currently held job, not his job after the move, we may also consider current job held as an individual attribute. Whether or not there is a match between his resources and rewards in the occupied job is an important topic, but the issue remains within the sphere of labor supply. Tuma's inclusion of a variable "historical change" is an attempt to move beyond the supply orientation, however. Another semi-Markovian theorist, who focuses on geographical rather than labor mobility, is Ginsberg (1972). Furthermore, Ginsberg also treats destination attributes causally and is instructive in this regard.

[2] At the individual level Wise's model assumes stationary probabilities; however, at the aggregate level the process is nonstationary as a function of the heterogeneous distributions over time. Tuma's model, on the other hand, is also nonstationary at the individual level due to its incorporation of such variables as age and seniority.

[3] Both McFarland (1970) and Bartholomew (1973) have pointed out that decreased mobility rates may have at least two theoretical explanations: (1) population homogeneity with duration effects and (2) population heterogeneity such that persons with higher seniority also have lower loss rates (constant over time). Thus, the two conceptualizations differ in

Plan of the Paper

There are four main analytical sections to follow and a brief overview of each is provided at this point. In Section 2 we fully describe the three models and hence only a brief note on each is given here. The vacancy or demand model asserts that an individual's promotion chances over time are determined by the number of opportunities available, managerial preference by seniority (given that a vacancy is to be filled internally) and the sufficiency of labor at the desired seniority levels. The simplest Markovian model assumes a constant promotion probability at a seniority level; hence, the individual's promotion chances are determined by his seniority level and successive cohorts of individuals experience the same set of promotion probabilities. The alternative Markovian model postulates a stable organizational level process of internal flows rather than a stable individual-level process. That is, grade-level promotion probabilities are assumed to be a fixed proportion of the internal labor supply at each grade. Given this number of promotions, an individual's promotion chances are determined by managerial preference per seniority level and the sufficiency of labor at those desired levels.

In Section 3 we focus on estimates of the parameters of each model. Particular attention is given to each model's treatment of the managerial selection process and of the major differences in promotion probabilities across models; the differences in imagery of the promotion process are quite dramatic.

Section 4 presents the comparative tests of the three models over a ten-year period. The data are on a police work force and its managerial staff. One of the issues in these tests is whether seniority operates both to rank individuals within grade and to determine promotion rates or whether its effect is conditional on opportunities. A second and related issue is the specification of the underlying behavior of the process—managerial selection, exit decisions that are conditional on not being promoted, and chain reactions of vacancies across grades. We know there are distinct vacancy effects (cf. White, 1970), but, as yet, we do not know the significance of vacancy processes for individual movement. The comparative test of the demand model should begin to inform us about vacancy effects on individual behavior, as well as the interaction of vacancies with individual attributes.

Finally, in Section 5 we probe further into the relations between opportunities and individual behavior by using the demand model. The analysis also enables us to take a harder look at the underlying behavior of the process as depicted by the demand model. Inferential evidence of cohort effects and variable selection rules is found, and the implications of interdependent vacancies are shown.

---

their initial assumptions, each explaining declining mobility over time. Since our population attribute data are limited to seniority, we will treat only seniority effects here. For an attempt to separate duration and other population heterogeneity effects, see Heckman (1977).

2. ORGANIZATIONAL ILM MODELS

Opportunity Labor Demand (OLD) Model

The opportunity model is a variant of Bartholomew's (1973) renewal model (cf. Hopes, 1971 and U. K. Civil Service Department, 1974), with extensions to permit growth and allocation of new jobs per stratum, internal selection rules, and an exit decision that is conditional on promotion. The model is also consistent with White's (1970) vacancy chain model in that it includes chain reactions of vacancies. No explicit links between individual vacancies are considered, however, and therefore vacancy chains are not themselves units of analysis. In short, this model is less comprehensive in its portrayal of total vacancy behavior than White's model. The loss of this aspect of the structural representation is not viewed as significant here since our purpose is to predict movement of individuals across grade boundaries.

Divide the ILMs population into k grades. Let $n_i(s,t)$ denote the observed number of persons with seniority s in grade i at time t and $\bar{n}_i(s,t)$ the corresponding number *expected* or *derived* from the assumptions of the model.[4] Let the probability that an individual in grade i with seniority s at time t who moves to grade j in one discrete time period be $p_{ij}(s,t)$ and the associated probability of exit from grade i be $p_{io}(s,t)$. Aggregate grade sizes and probabilities are simply the proportion of seniority level s in grade i at time t.

For the simple case where (1) promotion is only to the immediately higher grade, (2) no jobs are abolished, (3) there is no lateral recruitment and (4) ILM size is constant, Bartholomew (1973) developed a discrete time, equilibrium equation for his renewal model as follows:

$$n_i(t) = \sum_s n_i(s,t) \tag{1}$$

$$p_{ij}(t) = \sum_s \alpha_i(s,t)\, p_{ij}(s,t), \tag{2}$$

and

$$p_{io}(t) = \sum_s \alpha_i(s,t)\, p_{io}(s,t) \tag{3}$$

where

$$\alpha_i(s,t) = \frac{n_i(s,t)}{n_i(t)}, \tag{4}$$

$$\bar{p}_{i,i+1}(t) = \frac{\sum_{j=i+1}^{k} \bar{n}_j(t)\, p_{jo}(t)}{\bar{n}_i(t)}. \tag{5}$$

The numerator is the *cumulative* number of persons exiting from all higher grades, thereby generating job vacancies, and the denominator is the number of persons in the origin grade or promotion pool. Given the exit rates and initial grade distribution, $\{n_i(0)\}$, the model derives the expected promotion probabilities. Note that $\bar{p}_{ii}(t) = 1 - [\bar{p}_{i,i+1}(t) + p_{io}(t)]$.

The above model takes on interesting dynamic properties once we introduce the following types of subprocesses:

(1) Growth—new jobs and their allocation per grade;

(2) Managerial Selection Rules—the decision to fill a vacancy internally or externally and the preference by seniority level if the choice is internal;

(3) Worker Exit Decisions—the decision by individual workers to stay or to leave the firm, conditional on not being promoted.

Growth. Growth is embedded in the model by taking into account new job vacancies as well as vacancies arising from individuals leaving the ILM. Moreover, we will also treat the exit probability (to be derived shortly) as a derived term. The equation becomes:

$$\bar{p}_{i,i+1}(t) = \frac{\sum_{\ell=i+1}^{k} \bar{n}_\ell(t)\, \bar{p}_{\ell o}(t) + \bar{j}_\ell(t)}{\bar{n}_i(t)} \tag{6}$$

where $\bar{j}_\ell(t)$ is the expected number of new jobs created in grade ℓ in (t,t+1). Two things are very important in Equation 6. The first is the cumulative impact or chain reaction of vacancies originating in higher grades. This chain reaction results from the initial vacancies being filled by someone inside the organization who thereby opens another vacancy. Secondly, an individual's promotion chances over time are postulated as the ratio of the number of vacancies to the number of persons available for promotion or, in other words, the ratio of demand and supply over time.

---

[4] We will use the following symbols to indicate alternative meanings: $\bar{x}$: expected value of x; $\hat{x}$: value of x estimated from observed data; x: observed or theoretical value of x.

Since new vacancies, $\bar{j}_l(t)$, are an expected or derived term, there must be some mechanism for allocating the total pool of new jobs to grades. The simplest case, which we will use here, is to assume constancy in relative grade size. Let overall growth in jobs in (t,t+1) be J(t);

then
$$\bar{j}_l(t) = J(t)\hat{\rho}_l \qquad (7)$$
where
$$\hat{\rho}_l = \frac{\sum_t n_l(t)}{\sum_t \sum_l n_l(t)}, \qquad (8)$$
the proportion of persons in the ILM holding jobs at grade j. If we take the total ILM size at time t, N(t), as observed, then

$$J(t) = N(t+1) - N(t). \qquad (9)$$

Managerial selection rules. We now add two important factors to the supply/demand nature of the model: an assumption of managerial decision-makers who play an active role in the mobility process and an assumption that the managerial selection rules for labor supply are conditional on vacancies.

The discussion of individual heterogeneity, in this instance grade-level seniority, will be presented with respect to a person having seniority s in grade i. Let $v_{i+1}(t)$ represent the total number of vacancies in grade i+1 in (t,t+1). Given a vacancy, the managerial staff in grade i+1 may choose to fill it internally or externally. Let $\ell_{i+1}(t)$ denote the probability of filling a vacancy internally (in this case, by a person from grade i). If we assume a constant managerial policy regarding this internal/external distribution, the estimate of ℓ_{i+1i} is

$$\hat{\ell}_{i+1} = \frac{\sum_t \sum_s n_{i,i+1}(s,t)}{\sum_t v_{i+1}(t)}, \qquad (10)$$

where $n_{i,i+1}(s,t)$ is the observed number of persons with seniority s in grade i at time t who are promoted to grade i+1 by time t+1.

For internal selections, we assume that the managerial preference for an individual is determined by his seniority level within the grade.[5] We further assume that this selection rule is constant as long as there is a sufficient supply of individuals with the desired seniority to meet the vacancy requirement. Hence, given that a vacancy in grade i+1 is to be filled internally, the chances of being selected for promotion for a person with seniority s in grade i is $\lambda_{i+1,i}(s)$ and

$$\hat{\lambda}_{i+1,i}(s) = \frac{\sum_t n_{i,i+1}(s,t)}{\sum_t n_{i,i+1}(t)}. \qquad (11)$$

When there is an insufficient supply at a seniority level, the above selection rule must be modified so that no vacancies remain unfilled at the end of this period. If we assume that the relative weights attached to each seniority level are invariant, we may operationalize this modification by simply renormalizing the $\hat{\lambda}_{i+1,i}(s)$ terms. That is, $\hat{\lambda}_{i+1,i}(s)$ is applied iteratively until all vacancies are filled. Such modifications of the λ's are necessary when the seniority supply is less than the internal demand[6]—

$$n_i(s,t) < v_{i+1}(t)\ \ell_{i+1}\ \lambda_{i+1,i}(s). \qquad (12)$$

The choice of filling a vacancy internally or externally modifies the chain reaction of vacancies depicted in Equation 6 since some promotions are truncated by lateral recruitment. This modification is captured in Equation 13;

---

[5] Many alternative assumptions about heterogeneous attributes of the population, in terms of decision rules for managerial selection, are possible; thus, we expect other population attributes to affect both managerial and worker decisions. Since the present data preclude examining attributes other than seniority, however, our discussion will be directed only to seniority effects. Moreover, once multiple attributes are considered, then extreme partitioning, such as in McFarland's (1970) Markovian model, requires use of multivariate modeling strategies as in the Markovian regression models of Spilerman (1972), Wise (1975) and Tuma (1976), rather than partitioning of state space as is used here.

[6] Operationally, this means checking the promotion pool for sufficient supply via Equation 12. If $n_i(s^*,t)$ is less than the desired number, then all $n_i s^*,t)$ are promoted and $\{\lambda_{ii}(s)\}$ is renormalized to fill the vacancies remaining from the inequality in Equation 12; that is, $\lambda_{ii}(s) = \dfrac{\lambda_{ii}(s)}{[1 - \lambda_{ii}(s^*)]}$, where s* denotes the specific seniority level with insufficient supply.

$$
\begin{aligned}
\bar{v}_i(t) &= \sum_s \bar{n}_i(s,t)\ \bar{p}_{io}(s,t) \div \bar{j}_i(t) \qquad \text{if } i = k \\
&= \hat{\ell}_{i+1}\ \bar{v}_{i+1}(t) + \\
&\quad \sum_s \bar{n}_i(s,t)\ \bar{p}_{io}(s,t) + \bar{j}_i(t) \qquad (13) \\
&\qquad\qquad \text{if } i = 1, \ldots, k-1 .
\end{aligned}
$$

In sum, let m denote an individual in the ILM. Then, the probability of m being promoted, $\bar{p}_{i,i+1}(m)$, is

$$
\Pr\left[\begin{array}{l} \text{m is in} \\ \text{grade } i+1 \text{ at} \\ \text{time } t+1 \end{array}\middle|\begin{array}{l} \text{m is in} \\ \text{grade } i \text{ at} \\ \text{time } t\ , \end{array}\ \begin{array}{l} \text{m has} \\ \text{seniority } s \\ \text{in grade } i\ , \end{array}\ \begin{array}{l} \text{there is a} \\ \text{vacancy available} \\ \text{in grade } i+1\ , \end{array}\ \begin{array}{l} \text{the vacancy} \\ \text{is to be filled} \\ \text{internally} \end{array}\right]
$$

$$
= \frac{\bar{v}_{i+1}(t)\ \hat{\ell}_{i+1}\ {}^{-}\lambda_{i+1,i}(s)}{\bar{n}_i(s,t)} . \qquad (14)
$$

Worker exit decisions. We treat exit decisions as conditional on both the seniority distribution and promotion rates. We assume a two-step decision process in which managerial promotion occurs first and then exit behavior, contingent on managerial decisions, is determined. Sequencing the process in this manner is consistent with an individual anticipating promotion and not initiating job search (or at least not accepting outside offers) until internal decisions are made. We assert that normative expectations regarding promotion will have an effect on exit behavior once the promotion outcomes are known. We expect exits to be inversely related to internal opportunities. That is, that if promotion opportunities increase, exit rates will drop and if the opportunities decrease, exit rates will rise in response.[7] Here we conceptualize a process which generates this type of response behavior.

Consider two types of decision-makers (managers, workers) and two types of decisions (promotion, exit). Assume the original set of persons in grade i at the start of each time interval, $n_i(t) = \sum_s n_i(s,t)$, is the promotion pool from which managers make selections. Assume that the workers await the outcome of promotion decisions before making a decision to exit. This reduces the second set of decision-makers (workers) to

$$
\bar{n}_i^*(t) = n_i(t) - \bar{n}_{i,i+1}(t) . \qquad (15)
$$

Thus, the exit probability is now conditional on the individual's promotion chances. Moreover, since the distribution of labor supply does not always equal labor demanded, the seniority distribution of the available promotion pool also has an effect on behavior.

Again, denoting the individual as m, the estimate of the conditional probability of exiting, $\hat{p}_{io}^*(m)$ is

$$
\Pr\left[\begin{array}{l} \text{m exits} \\ \text{between} \\ \text{time } (t,t+1) \end{array}\middle|\begin{array}{l} \text{m is in} \\ \text{grade } i \text{ at} \\ \text{time } t\ , \end{array}\ \begin{array}{l} \text{m has} \\ \text{seniority } s \\ \text{in grade } i\ , \end{array}\ \begin{array}{l} \text{m was not} \\ \text{selected for} \\ \text{promotion} \end{array}\right]
$$

$$
= \frac{\sum_t n_{io}(s,t)}{\sum_t n_i^*(s,t)} . \qquad (16)
$$

The desired directional effects on exit behavior may be easily shown. Assume a constant promotion pool of 100 and an estimate of conditional exit as $p^*_{io}(t) = .125$ in a prior period in which there were 20 promotions and 10 exits. In the time period (t, t+1), there are also 20 promotions. Hence, $n^*_i(t) = 80$. Therefore, the model predicts the same number of exits, $n^*_i(t)\ p^*_{io}(t) = 80 \times .125 = 10$. Let opportunities decrease to 10 promotions in the time interval (t+1, t+2). Then the model predicts 11 exits in response. Similarly, let opportunities increase to 30 promotions in the period (t+2, t+3). Then the model predicts 9 exits, a decrease in loss rate. The derived exit rates for the initial population per time period are thus .10, .11

---

[7] Since external supply and demand are not treated here, we assert no specific external effects. The behavioral effects are, therefore, assumed ceteris paribus. It is reasonable to expect, for example, that an individual's exit probability is affected by the external supply of jobs relative to his internal opportunities.

and .09 as opportunities are constant, decrease and increase, respectively.

Having postulated the exit process as conditional on promotions, we may now specify the derived unconditional exit probabilities of Equations 6 and 13:

$$\bar{p}_{io}(s,t) = \hat{p}^*_{io}(s)\,[1-\bar{p}_{i,i+1}(s,t)]\,. \quad (17)$$

Stayers. Finally, once the population of unpromoted stayers is determined, these individuals are assumed to "age" in seniority by one time period. For this remaining population, a simple demographic aging process is assumed. The derived probablities for this "staying" process are, however, determined by the prior promotion and exit events.

Recruitment. In an ILM with recruitment at all grades, the recruits would diminish the number of chain reactions of promotion opportunities, as depicted in Equation 13. In which case, as noted earlier, managerial selection decisions between internal and external labor supply become an important issue. In the ILM that we examine, recruitment occurs only at the bottom grade, thus simplifying the process, namely, $\ell_j=1$ for j > 1. Recruitment is simply

$$\bar{R}_1(t) = \bar{v}_1(t)$$

$$= \sum_{l=1}^{k} \sum_{s} \bar{n}_l(s,t)\,\bar{p}_{lo}(s,t) + \bar{j}_l(t)\,. \quad (18)$$

The manner in which the overall demand model is formulated—where entering vacancies open vacancies through lower grades—means that the model's predictions must be sequentially derived from the highest to the lowest grade. Moreover, the built-in priorities for checks on adequacy of labor supply to meet demand and the formulation of exits as conditional on promotions also means deriving promotions prior to exits within each grade. The derivation of promotions is rather direct—from available vacancies and seniority selection rules. The derivation of staying probabilities is more complex, however, having been constructed from a two-step process of promotions and exits that are conditional on promotions. Thus, given the new exit specification, all transition probabilities become interrelated—promotions affect exits within the same grade and these conditional exits affect promotion opportunities in lower grades due to the chain reaction of opportunities depicted in Equations 6 and 13.

Markovian Labor Supply Models

The Duration Markov Chain (DMC) Model

As the simplest and most natural Markovian model of seniority flows, we suggest the Cornell model, developed by McGinnis (1968) and Henry et al. (1971). This model is an important advance over simple Markovian models which assume population homogeneity and stationarity of transition probabilities. In tests of the axioms of cumulative inertia (Morrison, 1967; Land, 1969) and of destination-duration independence (Ginsberg, 1977), the model's underlying assumptions have generally been supported in studies of geographical mobility. However, very little is known concerning its utility for predicting labor movement.

Whether supply attributes, such as seniority, are sufficient to predict organizational behavior or whether some internal opportunity-supply combination, as in the labor demand model, are more accurate will be empirically tested later. At the highest grade, from which there is no internal promotion, the two models will have the same exit parameter. But, since the "selection" process into the highest grade differs radically, predictions even in the highest grade will differ. The demand model makes selections to fill a specified set of vacancies in grade k; the duration-specific Markovian model, on the other hand, makes selections proportional to the available supply in grade k−1, while ignoring vacancies in grade k.

Since all seniority specific transition probabilities are assumed to be constant in this first Markovian model, we drop the time argument from the transition terms. The model is simply

$$\hat{p}_{ij}(s) = \frac{\sum_t n_{ij}(s,t)}{\sum_t n_i(s,t)}\,, \; j = i,\ i+1 \text{ and } o\,. \quad (19)$$

For individual m, we have $p_{ij}(m)$, as

$$\Pr\left[\begin{array}{l|ll} \text{m is in} & \text{m is in} & \text{m has} \\ \text{state j at} & \text{grade i} & \text{seniority s} \\ \text{time t+1} & \text{at time t,} & \text{in grade i} \end{array}\right]$$

$= \hat{p}_{ij}(s)$ for state $j = i, i + 1$, and o, which indicates staying in grade i, being promoted to grade $i + 1$ and exiting to the outside. For a lucid matrix presentation of this duration Markov chain (DMC) model, see Henry et al. (1971).

Growth and recruitment. To complete the model, we must account for organizational growth and recruitment. Since all internal staffing is specified by the seniority specific transition probabilities, $\{p_{ij}(s)\}$, the remaining task is to allocate recruitment. In the organization we examine, all recruits enter at the bottom,[8] which simplifies our task: we take total ILM size, N(t), as observed, as in the opportunity labor demand (OLD) model. Thus recruitment at the bottom, $R_1(t)$, during year t is assumed to be

$$\bar{R}_1(t) = N(t+1) - \sum_i \sum_s \bar{n}_i(s,t+1)\ . \qquad (20)$$

The Organizational Markov Chain (OMC) or Constant Push Model

As an alternative to the DMC model which allocates promotions and exits in proportion to the supply at the individual seniority level, we propose a simple Markovian model with stationary promotion rates, corresponding to a more stable process at the *organizational* level. Given grade specific promotion probabilities, selection by seniority occurs analogous to the labor demand model. This model has some plausibility in that, contrary to common impressions of sociologists regarding the failure of the simple Markov chain (MC), most tests of the simple MC model in organizational ILMs have found the model reasonably satisfactory. Evidence for the model's support has been found in the following ILMs: higher education (Young, 1965), manufacturing (Young, 1965; 1971), private research and development (Young, 1965), governmental scientists (Sales, 1971), a women's military unit (Forbes, 1971), the large civilian work force supporting the U.S. Navy (Charnes et al., 1972) and a state police organization (Stewman, 1975b). When tested against vacancy models (Young and Vassiliou, 1974; Stewman, 1978), the simple MC model with homogeneous populations has fared less well. We stress that it is in such comparative tests that the model's adequacy must be determined—not in individual tests across multiple ILMs.[9]

The simple MC must be modified to specify internal selection. Thus, while we assume the internal movement is Markovian at the aggregate grade level, it will no longer be Markovian at the seniority level. This is the opposite of the DMC model presented earlier, where the grade specific seniority mobility remained Markovian, but the aggregate level process was no longer a Markov chain.

Managerial selection rules. First, assume promotion probabilities are constant. That is,

$$\hat{p}_{i,i+1} = \frac{\sum_t \sum_s n_{i,i+1}(s,t)}{\sum_t \sum_s n_i(s,t)}\ . \qquad (21)$$

Second, assume managers select persons to be promoted according to a decision rule keyed to seniority as in the labor demand model,

$$\hat{\lambda}_{i+1,i}(s) = \frac{\sum_t n_{i,i+1}(s,t)}{\sum_t n_{i,i+1}(t)}\ . \qquad (22)$$

Thus, to derive an individual's promotion chances from this model, we observe that

$$\bar{n}_i(s,t)\bar{p}_{i,i+1}(s,t) = \bar{n}_i(t)\ \hat{p}_{i,i+1}\ \hat{\lambda}_{i+1,i}(s). \qquad (23)$$

---

[8] If lateral recruitment is allowed at all grades, then the extent of recruitment per grade must also be specified. In none of the three models we examine is this necessary since recruitment is entirely at the bottom grade.

[9] Furthermore, we wish to correct an impression of sociologists that simple Markov chains have always resulted in predictive failures. This impression arises from reading only tests conducted on cohort populations in the U.S. Most, but not all, tests of organizational ILMs cited above, and in which a continuously operative labor market process is represented, have been conducted in Europe and generally in the U.K. At issue is simply the proposition that we make conclusions regarding the simple MC on the basis of the evidence.

The model's prediction is, therefore,

$$\bar{p}_{i,i+1}(s,t) = \frac{\bar{n}_i(t)}{\bar{n}_i(s,t)}\, \hat{p}_{i,i+1}\, \hat{\lambda}_{i+1,i}(s) = \frac{1}{\bar{\alpha}_i(s,t)}\, \hat{p}_{i,i+1}\, \hat{\lambda}_{i+1,i}(s) \quad . \qquad (24)$$

Of course, the relative distribution of seniority in grade i, $\alpha_i(s,t)$, is observed only at the beginning of the first time period and is derived thereafter. The model asserts that one's promotion chances vary over time in response to the distribution of seniority in the promotion pool. That is, as the relative size of the individual's seniority level declines, the individual's promotion chances improve and vice versa. These changes result from the assumptions of constant grade promotion probabilities and managerial selection rules. As in the labor demand model, there is a possibility of an insufficient supply. In such a case, $\hat{\lambda}_{ji}(s)$ is renormalized iteratively until the supply is adequate to satisfy the aggregate parameter, $\hat{p}_{ij}$.

Worker exit decisions. Just as in the labor demand model, a two-step decision process is postulated. That is, we assume workers await the managerial promotion decison before deciding whether or not to leave. The conditional exit probability is thus given by Equation 16. Since this model and the labor demand model differ in the derivation of promotion probabilities, the populations of workers making exit decisions in the two models will also differ; this result yields different unconditional exit probabilities for the initial worker population.

Growth and recruitment. Growth is accommodated the same way by both Markovian models. That is, all upper grade movement is generated, reallocating persons per grade. Since total ILM size is taken as observed, the remaining staff to meet growth and replacement requirements is recruited at the bottom grade via Equation 20.

Parametric Comparison of the Models

A brief overview of the basic parameters and derived terms differentiating the three models is shown in Table 1. Only terms which will be used in the remainder of the paper are presented. The first three terms, which relate to jobs or job vacancies, are treated only in the OLD model. Internal managerial selection rules are explicit in the OLD and OMC models, but not in the DMC model. Flow parameters at the individual seniority level $\{p_{ij}(s)\}$ are estimated from observed data in the DMC model, but are derived terms in the OMC and OLD models. The characteristics that distinguish the derived flows in the OLD and OMC models may be seen in the last row, where the grade-level promotion probabilities are estimates from observed data in the OMC model, but derived (as the ratio of vacancies to labor supply) in the OLD model.

Table 1. Comparison of Parameters and Derived Terms**

	OLD	OMC	DMC
Allocation of new jobs	$\hat{\rho}_i$	—	—
New jobs	$[\bar{j}_i(t)]$	—	—
Job vacancies	$[v_i(t)]$	—	—
Managerial selection rules	$\hat{\lambda}_{i+1,i}(s)$ $\hat{\ell}_{i+1}$	$\hat{\lambda}_{i+1,i}(s)$	—
Conditional exit	$\hat{p}^*_{i,o}(s)$	$\hat{p}^*_{i,o}(s)$	—
Unconditional exit	$[\bar{p}_{i,o}(s,t)]$	$[\bar{p}_{i,o+i}(s,t)]$	$[\hat{p}_{i,o+o}(s)]$
Promotion	$[\bar{p}_{i,i+1}(s,t)]$	$[\bar{p}_{i,i+1}(s,t)]$	$[\hat{p}_{i,i+1}(s)]$
Stay	$[\bar{p}_{i,i}(s,t]$	$[\bar{p}_{i,i}(s,t)]$	$[\hat{p}_{i,i}(s)]$
Grade level	$[\bar{p}_{i,i+1}(t)]$	$\hat{p}_{i,i+1}(t)$	$[\bar{p}_{i,i+1}(t)]$

* Estimated term $\hat{X}$.

** Derived term $[\bar{X}]$.

The feature which differentiates most among the models is each model's view of the underlying process generating promotions. This may be seen by a direct comparison of the promotion equations. The difference in treatment of managerial selection rules is also apparent. These equations are:

Model Promotion Equations: $\hat{p}_{i,i+1}(s,t)$

DMC: $$\frac{\sum_t n_{i,i+1}(s,t)}{\sum_t n_i(s,t)}\quad p_{i,i+1}(s)\ \forall\ t$$

OMC: $$\frac{\bar{n}_i(t)\hat{p}_{i,i+1}\ \hat{\lambda}_{i+1,i}(s)}{\bar{n}_i(s,t)}$$

OLD: $$\frac{\left\{\sum_{\ell=i+1}^{k}\ \sum_s \bar{n}_\ell(s,t)\bar{p}_{\ell o}(s,t)+\bar{j}_\ell(t)\right\}\hat{\lambda}_{i+1,i}(s)}{\bar{n}_i(s,t)}$$

3. DATA AND PARAMETER ESTIMATES

The ILM Data

In a formal organization such as the state police, there are several internal labor markets: clerical, business administration, computer programming, and maintenance (all of which are civilian in nature) and a separate police ILM. There is no exchange of staff between the police and any of the civilian ILMs. Thus, we focus only on the police segment of the organization.

The data were obtained from individual career records and extend from January, 1950 to January, 1970. Parameter estimates are made from the first ten years (1950–1959) and the tests cover the second decade (1960–1970).

State Definitions and Parameter Estimates

The state police system has a clearly defined authority and status structure. On the basis of numbers and substantive meaning of authority-status levels, the nine formal ranks were grouped into five grades. They are (1) COL: Colonel, Lt. Colonel, Major, Captain, Detective (Det.) Captain; (2) LT: Lieutenant (Lt.), Det. Lt.; (3) SGT: Staff Sergeant (S/Sgt.), Det. S/Sgt., Sgt., Det. Sgt.; (4) CPL: Corporal, Det., and (5) TPR: Trooper. The number of seniority levels chosen per grade were based on population size and on nonstationarity between seniority levels of the DMC parameter estimates. For the COL and LT grades, six seniority levels were chosen: Seniority 1 (S1) through 5 and Seniority 6 or above (S6+). SGT has S1 to S8+; CPL S1 to S10+; and TPR S1 to S17+.

We first present the parameter estimates which do not take into account seniority levels. The data for estimation are from the years 1950–1959. From Equation 8 the estimates of new job vacancy allocations per grade, $\hat{\rho}_j$ in the OLD model, are as follows:

COL	LT	SGT	CPL	TPR
.0192	.0220	.0989	.1940	.6659.

Also, using Equation 21, the estimates of the aggregate grade promotion probabilities in the OMC model, $\hat{p}_{i,i+1}$, are

TPR→CPL	CPL→SGT
.0480	.0932
SGT→LT	**LT→COL**
.0688	.1517 .

Table 2 provides the maximum likelihood estimates (MLE) of the annual transition matrix for the DMC model estimated from Equation 19. Standard errors (se) of estimation are also provided for the promotion and exit probabilities. The formulae for the se values are

$$\mathrm{se}\{\hat{p}_{ij}(s,t)\} = \left[\frac{p_{ij}(s,t)[1-p_{ij}(s,t)]}{n_i(s,t)}\right]^{1/2} . \qquad (25)$$

(See Forbes, 1971.) Staying rates are computed as residuals. The range of variation (maximum-minimum) for promotions by seniority level, not including the required waiting periods in SGT, CPL and TPR, is substantial within each grade—LT: .29, SGT: .14, CPL: .21, TPR: .50. Seniority effects on promotion are obviously clear and quite important.

The seniority specific promotion curves for each grade are shown in Figure 1. The vertical lines above and below each expected probability indicate one standard error of estimation. Thus, while we observe at least one dip in each mean value curve, which produces a bimodal distribution, the standard errors show the promotion rates to form easily a skewed, unimodal, inverted U-shaped curve at each grade, and bear out McGinnis's (1968) hypothesis.[10]

Figure 2 shows the expected behavior of the promotion process across grades. As an individual rises in the hierarchy, his promotion chances peak at lower seniority levels, suggesting an *acceleration effect* of hierarchical grade. That is, one's best promotion chances come earlier, the higher the grade. Moreover, the rise and fall in promotion probabilities over seniority levels within a grade does not easily fit the argument of constant transition probabilities at the individual level. It does suggest an experience effect as measured

---

[10] All but three probabilities lie within one standard error of this form of curve (see dotted line); however, only 7 in 10 points are expected to be within this range.

Table 2. Ten-year Maximum Likelihood Estimates of Transition Probabilities (1950–1959) for the DMC Model

State		Probabilities					
		Promotion†	(se)††	Exit†	(se)††	Stay†	N
COL	S6+	—		.2034	(.0524)	.7966	59
	S5	—		.1000	(.0949)	.9	10
	S4	—		.0714	(.0688)	.9286	14
	S3	—		.0556	(.0540)	.9444	18
	S2	—		.1429	(.0764)	.8571	21
	S1	—		.1212	(.0568)	.8788	33
LT	S6+	0	—	.3125	(.1159)	.6875	16
	S5	.2857	(.1708)	.2857	(.1708)	.4286	7
	S4	.1250	(.0827)	.1875	(.0976)	.6875	16
	S3	.2069	(.0752)	.1379	(.0640)	.6552	29
	S2	.2549	(.0610)	.0784	(.0376)	.6667	51
	S1	.0678	(.0327)	0	—	.9322	59
SGT	S8+	.0559	(.0192)	.2098	(.0340)	.7343	143
	S7	.0909	(.0500)	.0303	(.0298)	.8788	33
	S6	.1458	(.0509)	.1458	(.0509)	.7083	48
	S5	.1515	(.0441)	.0303	(.0211)	.8182	66
	S4	.1124	(.0335)	.0337	(.0191)	.8539	89
	S3	.1240	(.0290)	.0310	(.0153)	.8450	129
	S2	.0070	(.0070)	.0070	(.0070)	.9860	143
	S1	0	—	.0203	(.0116)	.9797	148
CPL	S10+	.1400	(.0347)	.0600	(.0237)	.8	100
	S9	.2083	(.0586)	.0208	(.0206)	.7708	48
	S8	.1618	(.0447)	.0142	(.0146)	.8235	68
	S7	.2289	(.0461)	0	—	.7711	83
	S6	.2162	(.0391)	.0090	(.0090)	.7748	111
	S5	.1733	(.0309)	0	—	.8267	150
	S4	.0899	(.0214)	.0056	(.0056)	.9045	178
	S3	.0797	(.0169)	.0118	(.0068)	.9095	254
	S2	.0205	(.0083)	.0068	(.0048)	.9726	292
	S1	0	—	.0035	(.0035)	.9965	283
TPR	S17+	.2581	(.0786)	.0656	(.0441)	.6774	31
	S16	.4483	(.0924)	0	—	.5517	29
	S15	.5085	(.0651)	0	—	.4915	59
	S14	.2989	(.0491)	.0345	(.0196)	.6667	87
	S13	.2774	(.0383)	.0292	(.0144)	.6934	137
	S12	.1022	(.0259)	0	—	.8978	137
	S11	.1864	(.0263)	.0318	(.0118)	.7818	220
	S10	.1579	(.0216)	.0175	(.0078)	.8246	285
	S9	.0727	(.0157)	.0145	(.0072)	.9127	275
	S8	.0550	(.0134)	.0241	(.0090)	.9210	291
	S7	.0168	(.0074)	.0101	(.0058)	.9732	298
	S6	.0065	(.0046)	.0228	(.0085)	.9797	307
	S5	0	—	.0233	(.0077)	.9767	386
	S4	0	—	.0153	(.0057)	.9847	458
	S3	0	—	.0313	(.0066)	.9688	704
	S2	0	—	.0243	(.0052)	.9757	865
	S1	0	—	.0345	(.0064)	.9655	811

† Equation [19].

†† Equation [25].

by seniority. With regard to the expected promotion behavior per grade, we observe the following from the estimates: (1) LT: discounting the S5 values somewhat (due to the small N and associated high standard error), there is a very rapid increase in promotion chances from S1 to S2; then the probabilities decrease. At S2, one's promotion chances are 25%; (2) SGT: there is a rapid increase in the third year, peaking in the fifth year, S5, at 15%; (3) CPL: there is a gradual increase in promotion chances until the fifth year, then a rapid jump peaking in the seventh year,

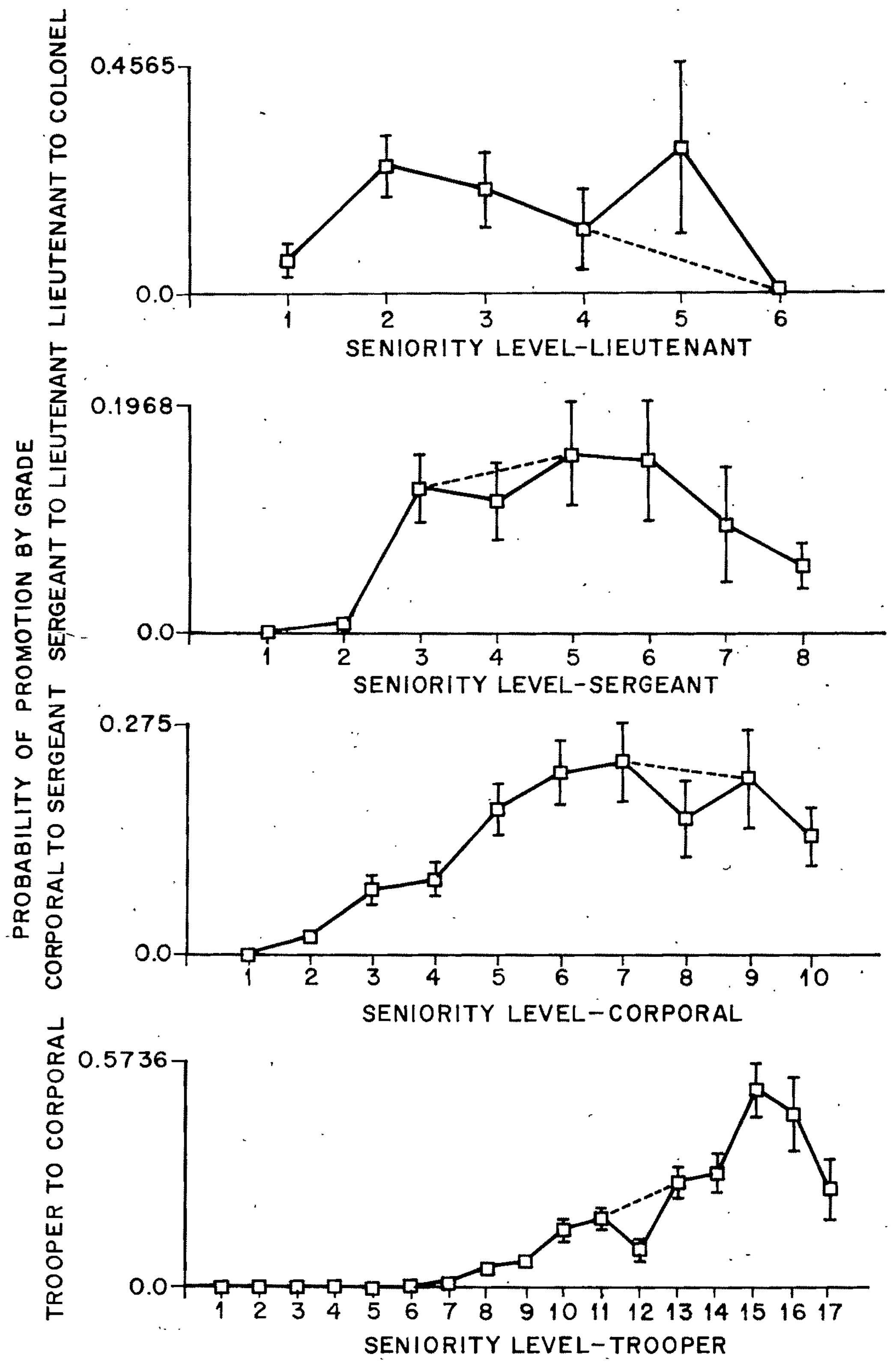


FIGURE 1. Promotion Probability Distribution by Grade (1950-59)

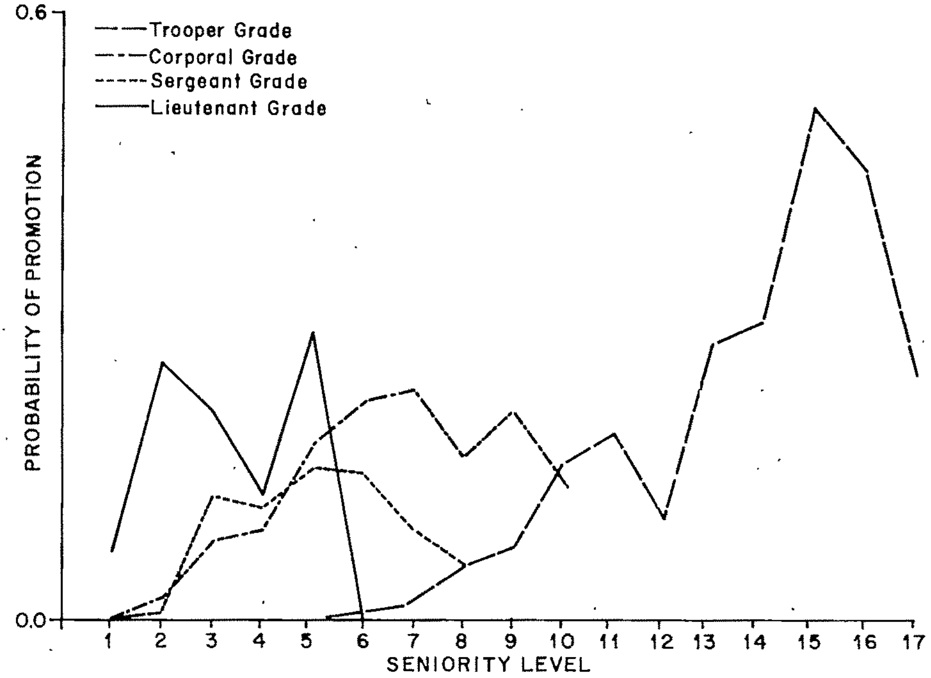


FIGURE 2: Promotion Probability by Grade and Seniority for the Entire Organizational ILM (1950-59)

S7, at 23%; (4) TPR: there is a waiting period of five years, then a gradual increase until the tenth year, with a peak at the fifteenth year, S15, when 51% are promoted.

The estimates of the managerial selection rules and conditional exits for the OLD and OMC models are given in Table 3. The $p^*_{10}(s)$ estimates at the COL grade are the same as the $p_{10}(s)$ estimates of the DMC model since there are no promotions from the highest grade. Thus, the table includes only four grades for both managerial selection and conditional exits. As in the promotion estimates of the DMC model, the managerial selection probabilities differ greatly between seniority levels. The range of variation within each grade, not including required waiting time, is as follows—LT: .48; SGT: .27; CPL: .14; and TPR: .16. The smaller variation in the lower grades contrasts with the pattern found in the DMC probability estimates and the variation in the upper grades is much higher here.

Figure 3 presents the managerial selection probability distributions. The image one receives of how the individual promotion process operates is quite different from that of the DMC model. In the OLD model, the managerial selection rules may be interpreted in the following way: given a vacancy, what is the probability that a manager decides to fill it with an individual having seniority level s? The OMC model's interpretation of this parameter differs in one important way: given a promotion, what is the probability that a manager decides to promote an individual with seniority s? For the present analysis of the models, with no lateral recruitment, this difference in interpretation seems minor. However, in ILMs with lateral recruitment, the managers in the OLD model can choose to fill the vacancy internally or externally. No such choice exists in the OMC model since vacancies are not conceptualized. Thus, the distinction we draw between the meanings of the selection rules in the OLD and OMC models depicts an important difference in viewpoints regarding the underlying managerial selection process. For convenience here, we will discuss the expected managerial decision rules using the vacancy model interpretation. *The* $\hat{\lambda}_{jl}(s)$ *estimates form a probability distribution of managerial preference across seniority*

Table 3. Maximum Likelihood Estimates of Managerial Selection and Conditional Exit Probabilities

Grade		Managerial selection $[\lambda_{i+1,i}(s)]$†	Conditional exit $[p^*_{i0}(s)]$††	(n^*_i)
LT	S6+	0	.3126	(16)
	S5	.0741	.4	(5)
	S4	.0741	.2143	(14)
	S3	.2222	.1739	(23)
	S2	.4815	.1053	(38)
	S1	.1482	0	(55)
	(N)	(27)		
SGT	S8+	.1455	.2222	(135)
	S7	.0545	.0333	(30)
	S6	.1273	.1707	(41)
	S5	.1818	.0357	(56)
	S4	.1818	.0580	(79)
	S3	.2909	.0354	(113)
	S2	.0182	.0070	(142)
	S1	0	.0203	(148)
	(N)	(55)		
CPL	S10+	.0959	.0698	(186)
	S9	.0685	.0263	(38)
	S8	.0753	.0175	(57)
	S7	.1301	0	(64)
	S6	.1644	.0115	(87)
	S5	.1781	0	(124)
	S4	.1096	.0062	(162)
	S3	.1370	.0128	(234)
	S2	.0411	.0070	(286)
	S1	0	.0035	(283)
	(N)	(146)		
TPR	S17+	.0310	.0870	(23)
	S16	.0504	0	(16)
	S15	.1163	0	(29)
	S14	.1008	.0492	(61)
	S13	.1473	.0404	(99)
	S12	.0543	0	(123)
	S11	.1589	.0391	(179)
	S10	.1744	.0208	(240)
	S9	.0775	.0157	(255)
	S8	.0620	.0255	(275)
	S7	.0194	.0102	(293)
	S6	.0078	.0230	(305)
	S5	0	.0233	(386)
	S4	0	.0153	(458)
	S3	0	.0312	(704)
	S2	0	.0243	(865)
	S1	0	.0345	(811)
	(N)	(258)		

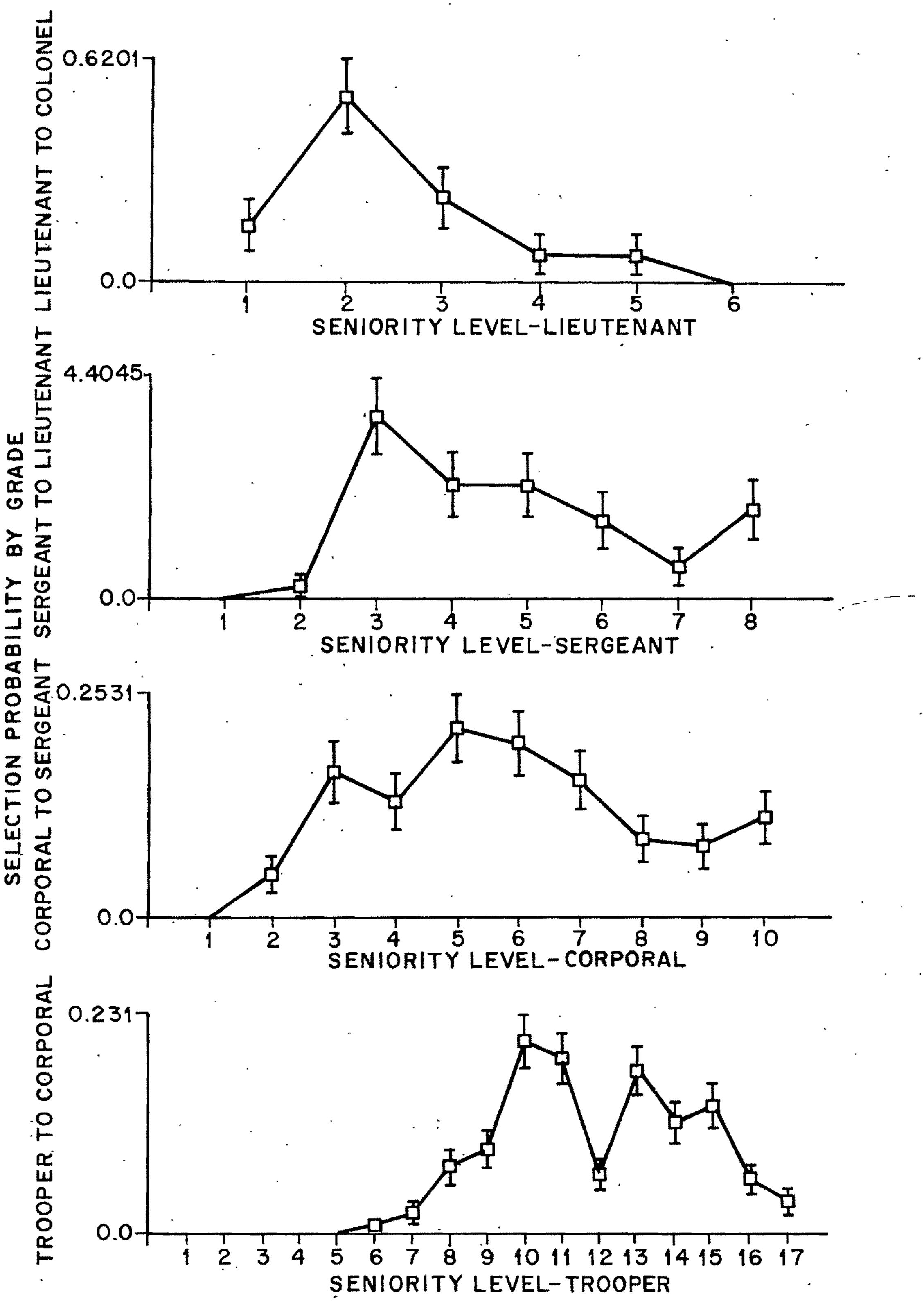
† Equation [12].
†† Equation [16].

levels. At the LT grade there is a 48% chance of selecting from Seniority 2 (S2) and a 70% chance of the selection being from S2 or S3, which indicates a very narrow selection band. Selections from the SGT grade are also concentrated (80%) from S3 to S6 with the largest being from S3 (29%). In the CPL grade there is a somewhat greater dispersion with selection probabilities .18 and .16 for S5 and S6, respectively. Finally, selections from the TPR grade are once again more concentrated, with the greatest preferences for S10 and S11 (17 and 16%, respectively). Given the standard error of estimation, it is plausible that these estimates have an underlying *unimodal distribution*. It is also interesting to observe that an *acceleration effect* of hierarchical grade as seen in the DMC model is also obtained here. What varies from the DMC model estimates is the behavior of the curve—lower seniority levels having higher probabilities—and, of course, the meaning of the probabilities. The selection probabilities (λ's) refer to selectivity from the entire grade promotion pool whereas the DMC promotion probabilities ($p_{i,i+1's}$) are based only on the number of persons within the individual's own seniority level. Thus, in regard to the acceleration effect, one's best selection chances occur earlier, the higher the grade.

To make direct comparisons between the different ways of modeling the process, we must have analogous "managerial selection rules" for the DMC model. The OLD and OMC models assume that managerial selection across seniority levels is stable and that the promotion and exit probabilities within seniority levels vary over time. Conversely, the DMC model assumes that promotion and exit probabilities within seniority levels are stable and that managerial selection across seniority levels varies over time (in response to fluctuations in the seniority distribution within grade). The derived managerial selection rules *across* seniority levels for the DMC model are obtained from the following equation:

$$\bar{\lambda}_{i+1,i}(s,t) = \frac{\bar{n}_{i,i+1}(s,t)}{\sum_s \bar{n}_{i,i+1}(s,t)} = \frac{\bar{n}_i(s,t)\ \hat{p}_{i,i+1}(s)}{\sum_s \bar{n}_i(s,t)\ \hat{p}_{i,i+1}(s)}. \tag{26}$$

Figures 4–6 provide illustrative comparisons between the models' descriptions of the process for the SGT grade. Figure 4 shows the variation between models in the



IGURE 3. Managerial Selection Probability Distribution by Grade (1950-59)

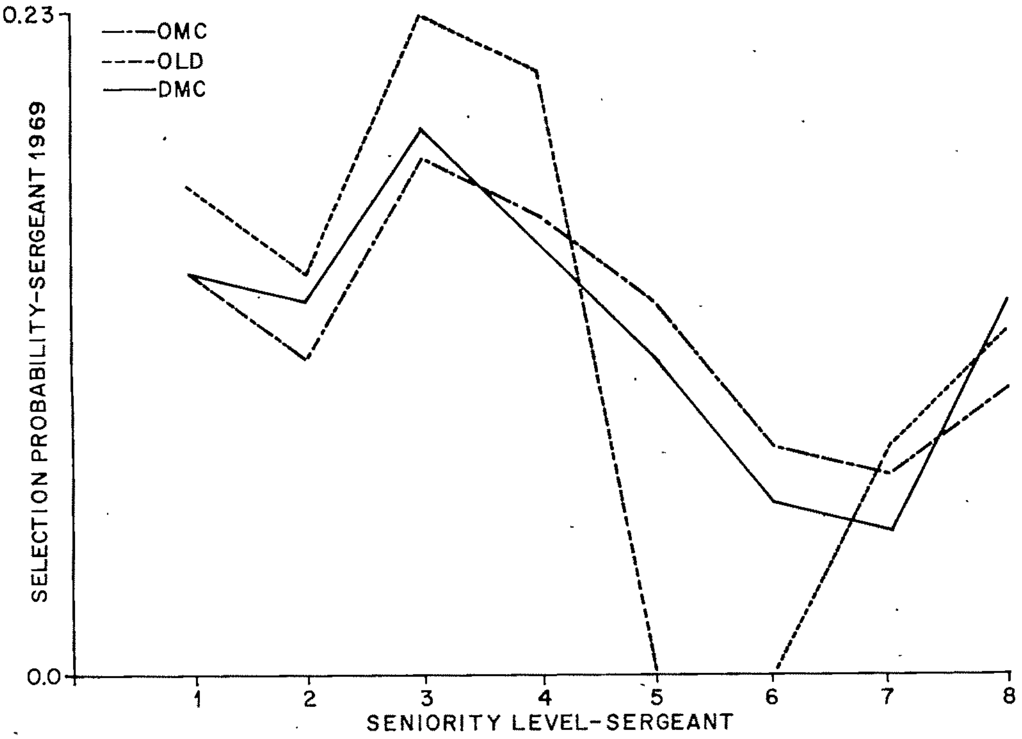


FIGURE 4: Managerial Selection Probability Distribution for SGT: by Model (1969)

distribution of managerial selection probabilities (λ's). The OLD and OMC models differ in this case since the λ estimates are renormalized (see fn. 5) due to the labor supply constraint (Equation 12) in the OLD model. In general, the form of the curves is similar, with greater differences between the OLD model and the DMC and OMC models. All models show S5 to have the highest managerial preference. Figures 5 and 6 reveal radical differences in the models' predictions and, in particular, show the expected nonstationarity of the seniority specific promotion probabilities of the OLD and OMC models. The large differences in Figure 5 indicate the effect of postulating a contingent managerial decision process, as in the OLD and OMC models, vs. a direct supply process as in the DMC model. Much larger promotion probabilities occur in the OLD and OMC models (at S5) when the supply of individuals in a seniority level is near the number of vacancies that the managerial preference function assigns to that level. Figure 6 shows the effects of insufficient seniority supply at S5 and S6 for the OLD model; note the volatile response of the promotion probabilities at S4 and S7 as unfilled vacancies are reassigned to other seniority levels.

4. COMPARATIVE TESTS

The tests extend over a 10-year period, 1961–1970, with parameters estimated from the prior 10 years, 1950–1959. All three models are given exactly the same observed information, although it is used differently within each model. For the parameter estimates, these observations are simply $n_{ij}(s,t)$, $j = i, i+1, o$. And, in the tests, two types of observed data are used—the starting seniority distributions at each grade in 1960, $n_i(s,1960)$ and the total organization size for all 10 years, $N(t)$. From 1961 to 1970, all grade and seniority distributions within grade are derived. That is, while the 1961 prediction is based on the 1960 observed values, the 1962 forecast uses the 1961 prediction; the 1963 forecast uses the 1962 prediction, and so on. Therefore, predictions in 1963 and 1970 involve a 3-year and 10-year forecast, respectively. These tests are commonly called n-step tests, where the

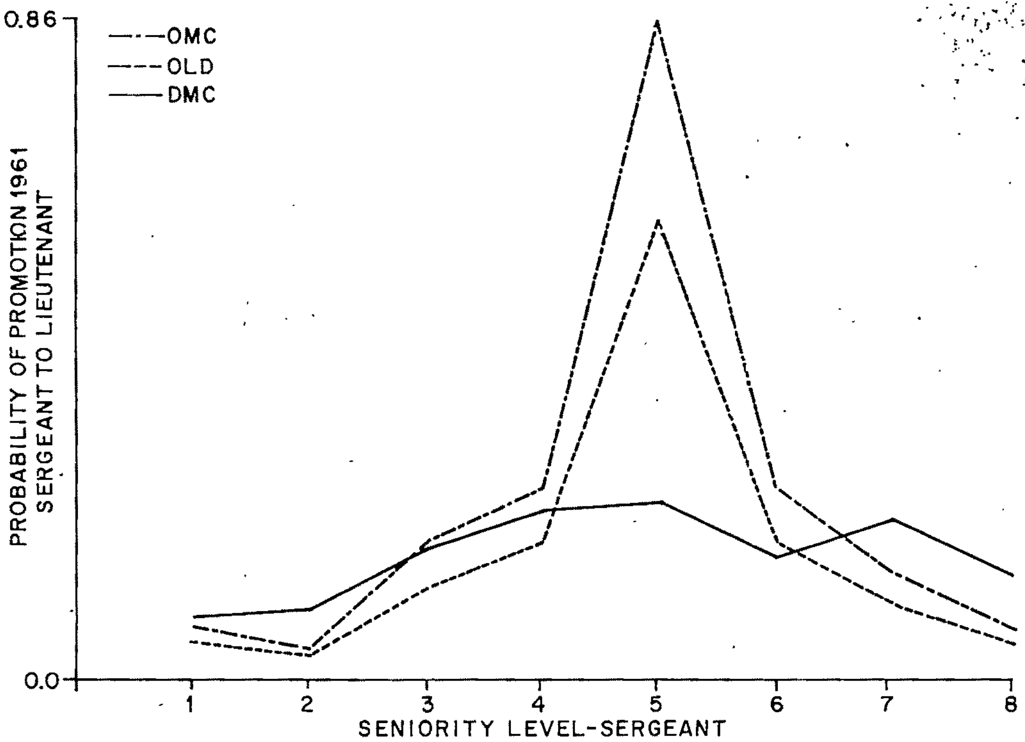


FIGURE 5: Promotion Probability by Seniority Level for SGT: 1961

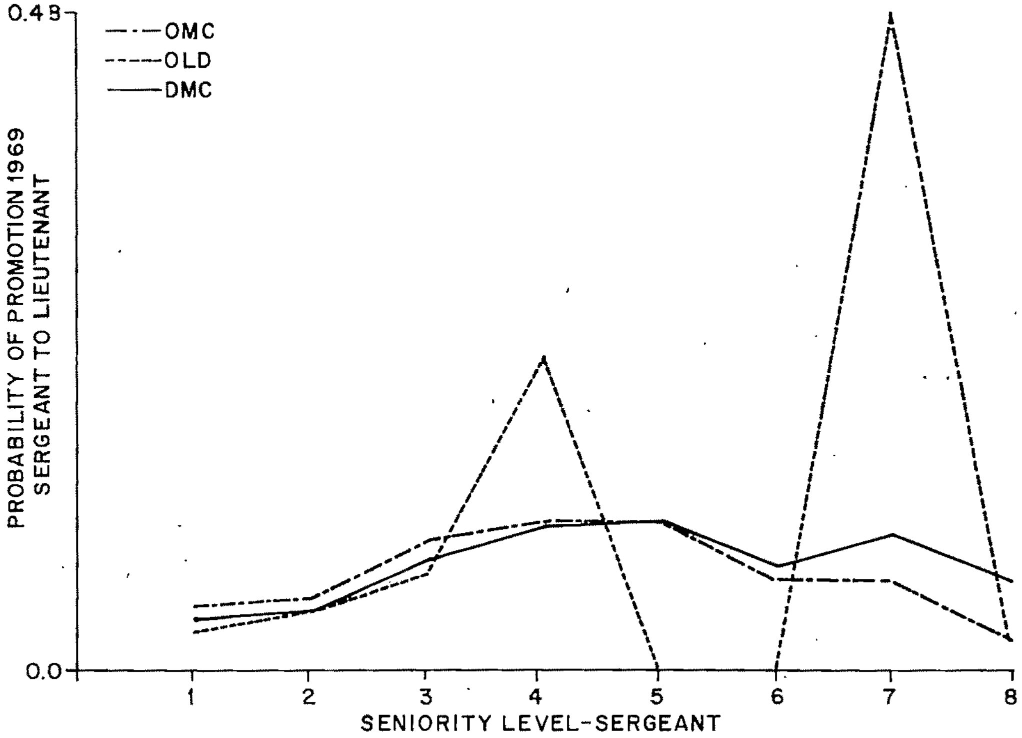


FIGURE 6: Promotion Probability by Seniority Level for SGT: 1969

prediction for the nth year is based on the predicted distributions for the (n-1)st year. This is a much more rigorous test than a set of n 1-step tests. For brevity, we focus on results of the 3-, 5- and 10-step predictions and on the average predictive capability of each model after 3, 5 and 10 years.

Table 4 presents the predicted and observed seniority distributions within grade for the 3-, 5- and 10-step tests and Table 5 provides summary measures.

Table 4. Predicted and Observed Seniority Distributions by Grade for Three-, Five- and Ten-Year Tests

		1963				1965				1970			
State		OMC[a]	DMC[b]	OLD[c]	OBS[d]	OMC	DMC	OLD	OBS	OMC	DMC	OLD	OBS
COL	S6+	6.46	6.46	6.46	6.	8.41	8.41	8.41	10.	10.01	11.17	9.59	7.
	S5	6.01	6.01	6.01	8.	2.10	2.92	1.61	0.	2.91	3.42	2.94	1.
	S4	0.00	0.00	0.00	0.	2.33	2.16	1.68	2.	3.34	3.82	4.59	5.
	S3	2.40	3.33	1.84	1.	2.67	3.22	2.17	4.	3.73	4.39	3.82	2.
	S2	2.88	2.67	2.08	2.	3.33	4.05	2.90	0.	4.57	5.55	4.51	2.
	S1	3.55	4.28	2.88	4.	4.08	4.54	4.95	4.	5.44	6.67	4.95	6.
LT	S6+	3.70	2.58	3.91	3.	2.42	1.99	2.70	5.	3.57	3.32	2.20	2.
	S5	1.94	1.80	2.33	5.	1.82	2.72	1.69	3.	2.66	3.07	1.84	2.
	S4	0.00	0.81	0.00	2.	2.93	3.65	1.58	2.	3.89	4.88	5.80	3.
	S3	4.11	6.05	3.88	5.	4.88	5.54	2.60	1.	6.18	7.70	5.37	4.
	S2	7.03	8.37	5.18	2.	7.98	8.41	6.18	2.	9.97	11.78	8.97	3.
	S1	8.19	8.92	6.01	2.	9.04	9.63	9.38	3.	11.29	12.86	9.88	8.
SGT	S8+	18.90	17.77	20.15	18.	30.61	24.74	33.07	33.	27.43	28.54	16.60	15.
	S7	13.15	10.39	13.86	12.	0.78	3.30	1.75	5.	10.20	10.57	7.20	9.
	S6	15.95	13.58	17.01	20.	7.76	8.55	8.58	14.	14.09	13.96	17.48	7.
	S5	3.74	5.69	4.70	7.	13.87	15.64	7.87	18.	17.09	17.48	18.22	19.
	S4	11.64	12.24	12.29	15.	17.14	20.51	9.18	9.	20.58	21.58	31.60	33.
	S3	19.16	21.68	12.28	19.	21.44	25.75	14.07	17.	25.52	27.66	24.60	28.
	S2	20.70	24.62	12.46	10.	22.60	25.64	18.59	14.	27.37	31.36	24.58	23.
	S1	22.21	26.65	14.64	17.	23.81	24.48	26.91	12.	29.53	36.23	24.37	24.
CPL	S10+	27.09	23.78	30.62	26.	28.25	25.44	38.18	35.	61.89	33.59	61.43	22.
	S9	4.83	6.40	7.85	11.	38.59	20.33	42.70	34.	11.20	10.57	0.00	7.
	S8	9.57	9.88	13.47	14.	23.68	16.66	27.42	23.	11.98	13.79	0.00	6.
	S7	44.08	31.86	47.96	41.	1.19	8.20	3.86	13.	14.94	19.72	5.16	8.
	S6	30.89	27.88	34.40	29.	9.54	13.84	10.99	19.	20.49	33.10	30.60	12.
	S5	9.22	12.80	11.71	15.	29.16	21.33	9.51	30.	29.20	46.58	31.77	37.
	S4	16.39	18.51	17.96	20.	30.47	28.73	12.07	11.	35.00	57.91	65.69	102.
	S3	35.52	25.93	15.29	35.	33.32	33.93	18.36	20.	46.47	69.03	46.22	83.
	S2	35.31	32.49	16.80	11.	34.11	38.48	28.47	25.	50.63	52.79	44.74	47.
	S1	34.66	35.01	19.66	20.	35.01	50.22	48.99	15.	53.03	59.53	42.40	67.
TPR	S17+	1.87	3.62	6.20	7.	11.07	6.28	21.81	26.	5.81	4.83	22.78	28.
	S16	0.00	1.82	0.35	5.	4.88	4.63	8.72	13.	2.65	7.31	3.51	12.
	S15	19.33	14.53	26.36	20.	8.57	10.17	15.63	19.	3.60	13.49	8.45	19.
	S14	11.22	14.12	19.84	19.	19.75	17.22	24.09	20.	163.63	79.48	153.79	61.
	S13	19.04	21.99	28.46	25.	2.28	13.14	9.87	14.	104.28	75.36	100.83	74.
	S12	25.85	27.65	35.74	29.	0.00	0.00	0.00	0.	0.00	0.00	0.00	0.
	S11	11.49	18.71	19.15	18.	46.54	45.83	49.14	50.	0.00	0.00	0.00	0.
	S10	0.00	0.00	0.00	0.	50.58	50.41	51.76	51.	5.00	15.67	7.12	15.
	S9	62.01	60.90	64.18	58.	230.13	216.70	230.37	231.	20.01	26.04	23.64	18.
	S8	60.17	59.97	60.84	55.	157.09	154.90	157.18	158.	33.54	34.30	35.28	36.
	S7	243.03	241.79	243.22	243.	0.00	0.00	0.00	0.	53.40	53.69	54.53	51.
	S6	163.98	163.98	163.98	164.	0.00	0.00	0.00	0.	110.03	112.05	112.76	108.
	S5	0.00	0.00	0.00	0.	20.52	20.20	21.40	30.	94.90	96.27	99.13	123.
	S4	0.00	0.00	0.00	0.	30.22	31.12	32.87	35.	188.65	186.47	182.71	220.
	S3	21.51	21.18	22.43	31.	39.79	38.97	41.27	44.	123.53	119.88	123.31	115.
	S2	31.97	32.92	34.77	35.	60.85	60.84	61.98	63.	116.46	115.20	115.24	124.
	S1	42.24	41.37	43.81	44.	125.37	127.66	128.47	124.	112.32	109.44	111.80	114.

[a] Organizational Markov Chain Model.

[b] Duration Markov Chain Model.

[c] Opportunity Labor Demand Model.

[d] Observed Values.

Table 5. Relative Error for Seniority Level Predictions[a] by Grade for Three-, Five- and Ten-Year Tests

Year	Model	Grade COL	LT	SGT	CPL	TPR	All Grades[b]
1963	OMC	.25	.94	.24	.30	.08	.36
	DMC	.27	1.01	.33	.32	.06	.40
	OLD	.21	.73	.20	.23	.06	.29
1965	OMC	.44	1.29	.41	.43	.08	.53
	DMC	.50	1.41	.51	.57	.10	.62
	OLD	.46	1.01	.34	.42	.05	.46
1970	OMC	.50	.71	.30	.50	.25	.45
	DMC	.63	.98	.35	.35	.13	.49
	OLD	.45	.56	.14	.46	.22	.37

[a] Equation [27].

[b] With equal weights per grade.

We utilize a measure which equally penalizes each predictive error within a grade—both overpredictions and underpredictions—and weights seniority levels by size. The measure per grade is

$$\frac{\sum_s | P_s - O_s |}{\sum_s O_s}, \qquad (27)$$

where P_s is the predicted number of persons with seniority s and O_s, the observed number. The observed term is used in the denominator since it is constant across different models. We interpret the measure calculated by Equation 27 as the average percentage error per seniority level within the grade. After 3 years, the OLD model predicts better at each grade and the OMC model is slightly more accurate than the DMC model. In only one grade, TPR, is the DMC model better than the OMC model. After five years, the same relative standing exists: OLD > OMC > DMC. However, at 10 years, the OMC and OLD models' errors in the CPL and TPR grades are much larger than those of the DMC model. If equal weights are assigned to all relative grade errors, however, the same ranking in all three years holds: OLD > OMC > DMC. In fact, for the 15 predictions, only three predictions are better than that of the OLD model—COL grade in 1965 and CPL and TPR grades in 1970. For these 3-, 5- and 10-year predictions, the OLD model clearly has the best overall fit.

A different perspective is gained if we also compare the models' predictions for aggregate grade distribution obtained from the seniority predictions. That is, rather than concentrate on an individual's seniority level by grade, we take a more aggregate look and simply predict an individual's grade. These predictions and the relative errors per grade are given in Tables 6A and 6B. As expected, the errors are much lower and the same comparative finding remains.

As the time horizon is extended, we may also be interested in the consistency of a model's predictive accuracy over the entire time period. Evaluating overall consistency means utilizing all one-point, n-step (n = 1,...,10) predictions rather than a few of them. To do so, we use a summary measure of the average predictive error within a time period. Three-, five- and ten-year periods are taken, which correspond to the 1-point tests

Table 6A. Predicted and Observed Grade Distributions for Three, Five and Ten Years

Year	Model	Grade COL	LT	SGT	CPL	TPR
1963	OMC	21.30	24.97	125.45	247.56	713.71
	DMC	22.75	28.53	132.62	224.54	724.55
	OLD	19.27	21.31	107.39	215.72	769.33
	OBS	21.	19.	118.	222.	753.
1965	OMC	22.92	29.07	138.01	263.32	807.64
	DMC	25.30	31.94	148.61	257.06	798.07
	OLD	21.72	24.13	120.82	240.55	854.56
	OBS	20.	16.	122.	225.	878.
1970	OMC	30.00	37.56	171.81	334.83	1137.81
	DMC	35.02	43.1	187.38	396.61	1049.38
	OLD	30.40	34.06	164.65	328.01	1154.88
	OBS	23.	22.	158.	391.	1118.

Table 6B. Relative Error Per Grade[a]

Year	Model	Grade COL	LT	SGT	CPL	TPR	All Grades[b]
1963	OMC	.01	.31	.06	.12	.05	.11
	DMC	.08	.50	.12	.01	.04	.15
	OLD	.08	.12	.09	.03	.02	.07
1965	OMC	.15	.82	.13	.17	.08	.27
	DMC	.27	1.00	.22	.14	.09	.34
	OLD	.09	.51	.02	.07	.03	.14
1970	OMC	.30	.71	.09	.14	.02	.25
	DMC	.52	.98	.19	.01	.06	.35
	OLD	.32	.55	.04	.16	.03	.22

[a] $\left| \sum_s P_s - \sum_s O_s \right| / \sum O_s$.

[b] With equal weights per grade.

above. The ten-year average allows comparison across models for the full time period. The 3- and 5-year data are shown to illustrate the models' capability in shorter periods, such as might be used in EEO planning and labor negotiations. The data for these seniority level predictions are shown in Table 7. Again, the same relative predictive accuracy holds. The vacancy model is the best; the organizational Markov chain ranks second and the duration Markov chain is third. These findings hold for all three time periods and are viewed as significant in supporting the view that managerial choice and vacancy processes need to be accounted for in models of organizational labor mobility. Moveover, there is also indirect support for the incorporation of worker exit decisions which are conditional on managerial promotion decisions, since both models postulating such choice behavior outperformed the duration Markov chain model, which did not assume such processes.

5. FURTHER ANALYSIS OF THE VACANCY AND DECISION PROCESSES

In this section we further explore the relations between opportunities and individual behavior. This is especially important given the outcome of the comparative tests in which the OLD or demand model had the best predictive capability. We will take a fresh look at the data and focus on the two basic internal driving forces of the model: (1) managerial selection conditioned on opportunities and (2) vacancy flows across grades. For the selection process, we take the SGT grade as data[11] and for the vacancy flow processes, we use the entire ILM as data. These analyses are not to be construed as tests of the same kind as those in Section 4. Rather, they are intended to give further insight into the underlying processes which produced those results. Due to the small numbers at individual seniority levels, no discussion of statistical testing is considered in the analysis of managerial selection and opportunities; but, this by no means limits the importance of the inferential findings as to substantive significance. In the section on vacancy flows, there are two issues: the use of a model with a chain reaction effect to depict the underlying process generating individual mobility and the sources of the demand model's predictive errors.

Table 7. Average Yearly Relative Error[a] Over Time for Seniority Level Predictions

Time	Model	Grade COL	LT	SGT	CPL	TPR	All Grades[b]
1961–63	OMC	.25	.62	.21	.19	.07	.27
	DMC	.30	.74	.27	.21	.06	.32
	OLD	.20	.47	.20	.19	.06	.22
1961–65	OMC	.34	.86	.28	.26	.08	.36
	DMC	.39	.97	.35	.32	.07	.42
	OLD	.30	.65	.25	.25	.06	.30
1961–70	OMC	.44	.78	.31	.41	.14	.42
	DMC	.49	.94	.37	.37	.10	.45
	OLD	.37	.63	.25	.38	.11	.35

[a] Equation [27].

[b] Equal weights per grade.

Managerial Selection and Opportunities

The demand model asserts that an individual's promotion chances over time are determined by managerial selection rules, the supply of individuals and their attribute profiles, and the number of promotion opportunities available. The number of opportunities fluctuates over time and we will look at years having few vacancies vs. years having more than a few vacancies entering the SGT grade. We refer to these selection years as "lean" and "fat" years, respectively. Within this dichotomy, we will examine the managerial selection processes for evidence of dependency on vacancy availability; that is, do the selection rules change as vacancies become fewer so that only "the cream of the crop" is selected, whereas, when more vacancies become available, a less discriminating selection process occurs? Since we are dealing only with selection by seniority, a thorough test of the stability of selection rules is not possible. Nevertheless, the seniority attribute per se provides interesting insights into the process. Lastly, we will look for evidence of cohort differences in terms of the degree to which they are "loaded with talent." Inferential evidence on this aspect may be obtained by looking at cohort promotion rates over the ten-year period, 1960–1969.

---

[11] Similar analyses may be conducted for other grades. The SGT grade, rather than the entire ILM, was chosen as a compromise between the need for adequately large populations and brevity.

Figure 7 provides the data necessary for these inquiries. The rows refer to seniority levels, the columns to years and the diagonal sets of entries to cohorts. A SGT cohort is defined as the group of persons entering the SGT grade at a specific year (analogous to a birth cohort in demography). The number of promotion opportunities or vacancies *entering* the SGT grade are given below each year at the bottom of the figure. The denominator in each cell is the number of persons originating in that state for that year. The numerator is the number of persons promoted to LT.

We make a crude cut between "fat" and "lean" opportunity years as follows:

LEAN—2-3 vacancies (1961, 1962, 1963, 1964, 1968),

FAT—5-11 vacancies (1960, 1965, 1966, 1967, 1969).

If we take the proportion of persons selected for promotion by seniority level for the FAT and LEAN periods, the selection parameters $\lambda_{LT,SGT}(s,t)$ are:

	S1	S2	S3	S4	S5	S6	S7	S8+	(N)
LEAN:	0	0	.08	.08	.15	.23	.31	.15	(13)
FAT:	0	.10	.05	.28	.13	.23	.05	.18	(40)

It appears that the selection probabilities change in response to opportunities. As opportunities expand (fat years), a much greater proportion of selections is made from lower seniority levels than when promotions are tight. In the lower four seniority levels, this ratio is 16% in lean years to 43% in fat years. Looked at from yet another perspective, in FAT years, 85% of selections are from S4–S8+; in LEAN years 84% are from S5–S8+; thus, the selection band decreases in LEAN years.

Evidence of cohort variation exists if we look at the diagonal cells. To view a cohort's behavior over time, take the 1960 cohort (S1, 1960) as an example. In each of the first four years none of the 15 persons was promoted. In the fifth year (S5), which was 1964, one person was promoted; in the sixth year, six were promoted (and one left—inferred from 7 in the denominator in seventh year) and in the seventh year no one was promoted from the seven persons remaining. The last year, S8+ has a mixture of cohorts—1960 and before—and, thus, has an asterisk on the denominator.

Consider the first seven cohorts (1960–1966) which have from 7 years (1960–1963 cohorts) to 4 years (1966 cohort) to be promoted from the S1–S7 levels. All have 3–4 fat years. The cumulative promotion ratio is as follows:

Seniority Level	S1,1960	S1,1961	S1,1962	S1,1963	S1,1964	S1,1965	S1,1966	S1,1967	S1,1968	S1,1969
S1	0/15	0/19	0/10	0/17	0/14	0/12	0/30	0/36	0/28	0/23
S2	0/8	0/15	0/19	0/10	0/17	0/14	0/12	4/30	0/36	0/28
S3	1/23	0/8	0/15	0/19	0/10	0/17	0/13	0/11	1/25	1/34
S4	3/21	0/22	0/8	0/15	1/19	1/9	3/16	1/12	0/11	3/23
S5	0/15	1/15	0/22	0/7	1/15	3/18	0/7	0/11	0/11	2/10
S6	0/9	0/15	1/13	1/20	1/6	6/14	1/11	1/6	0/11	1/10
S7	0/1	1/9	1/15	2/12	0/18	0/5	0/7	2/9	0/4	0/11
S8+	1/14*	0/7*	0/12*	0/18*	0/22*	1/33*	3/23*	1/13*	2/11*	1/8*
Year	1960	1961	1962	1963	1964	1965	1966	1967	1968	1969
No. Vacancies	(5)	(2)	(2)	(3)	(3)	(11)	(7)	(9)	(3)	(8)

Figure 7. Promotion Ratios by Seniority Level, Year, and Cohort for the SGT Grade: 1960–69

	Cohort						
	1960	1961	1962	1963	1964	1965	1966
% Promoted	.47	.37	.2	.18	.14	.17	.27
Cohort Size	15	19	10	17	14	12	30

The first four cohorts all have 7 years and show considerable variation in percentage promoted. This suggests that the distribution of talent, as measured by the selection criteria of the managers, differs by cohort. In fact, the 1966 cohort, which had but 4 chances (years), had a higher selection ratio than any cohort since 1961. These cohort data indicate, as we expected, that a much wider range of heterogeneity is operative than seniority can capture.

The two findings—cohort effects and variable selection rules—suggest that a much more complex selection process is operative. From the cohort data, we infer that multiple attributes of individuals must be considered. From the selection data, we add that one of these attributes must be seniority. A much larger issue is whether or not there are both multiple attribute heterogeneity and changing selection rules. To address this issue, much more detailed data on individual attributes are necessary. Moreover, the partitioning strategy used here (seniority levels within grade) will not be a feasible approach and some form of multivariate model for selection, such as a regression model, becomes necessary. Ginsberg (1972), Spilerman (1972), Wise (1975), and Tuma (1976) have all formulated regression models to generate the transition parameters of a PUSH or supply type of process for Markovian or semi-Markovian models. However, these approaches do not solve the problem where selection of individuals is CONDITIONAL ON OPPORTUNITIES: that is in a PULL or demand type of process.[12]

Vacancy Processes and Promotions

Lastly, we examine the mechanics of staff flows as depicted by the OLD model. First, we recast the observed flow data by using the OLD model to interpret theoretically the underlying vacancy processes which generate individual promotions. Second, we examine the sources of the OLD model's prediction errors.

The observed flow data are shown in Figure 8a. The solid lines denote person flows and the dotted lines vacancy flows, in this case the *net* number of new jobs (new jobs−jobs abolished). For instance, at the COL grade there are 29 exits, 4 new jobs (net) and 33 promotions into the grade from LT. Figure 8b takes these flow data and reinterprets them as a vacancy-driven process, having a chain reaction effect. Hence, the 33 promotions from LT open 33 vacancies which are added to the 20 vacancies arising there from exits and new jobs. This generates 53 vacancies which are filled by promotions from SGT, and so on. Thus, from the right side of Figure 8b, we observe a total of 475 vacancies entering (33 + 20 + 160 + 262) the ILM and generating a total of 774 promotions.

Had the parameters estimated from the 1950–1959 period been totally accurate, the model's predictions for 1960–1969 would have perfectly matched the flows in Figure 8b. Instead, we may observe where the model's parameters went wrong by viewing Figure 8c. Hence, the observed data, when theoretically interpreted as in 8b, act as a base line with which to investigate the source and magnitude of the OLD model's prediction errors.

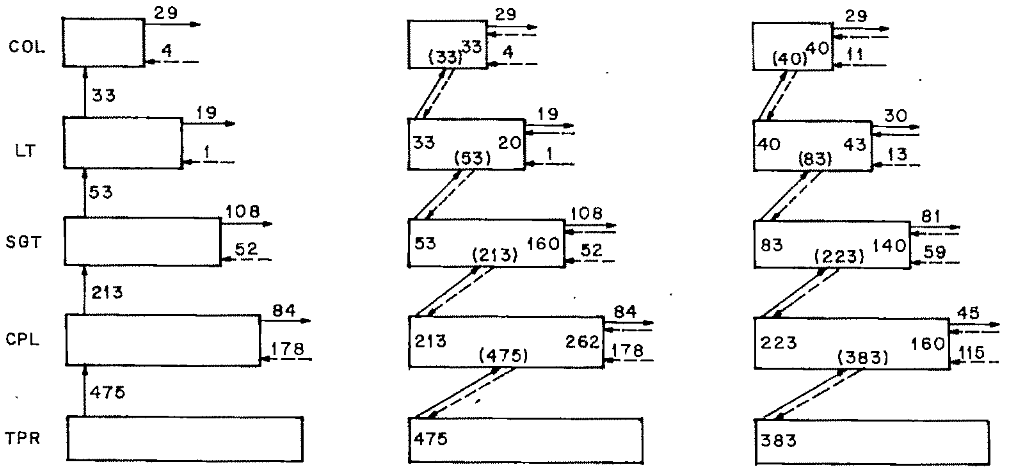
The two types of parameters causing prediction errors for promotions are exits and net new jobs. Looking at Figure 8b and c we find the data on predicted and observed exit rates are as follows:

	COL	LT	SGT	CPL
Predicted Exits	29	30	81	45
Observed Exits	29	19	108	84
Error	0	+11	−27	−39

The COL grade has no error, whereas the LT grade is overpredicted and the SGT and CPL grades underpredicted. In three of the 10 years, there were also un-

---

[12] In a sequel paper, one type of multivariate conditional selection model has been developed by the authors. The strategy assumes constant selection rules, as assumed in this paper, but specifies a ranking of individuals for selection. Thus, managers select the number of individuals from this ranking to fill available vacancies. And since vacancies fluctuate over time, an individual's promotion chances respond to *both* attribute profiles and available vacancies as they change over time.



8a. 1960–69 Observed Data

8b. OLD Model's Imagery of 1960–69 Observed Data

8c. OLD Model's Imagery Using 1960–69 Predictions

Figure 8. 1960–69 ILM Flow Data

derpredictions of exits at TPR, possibly suggesting the need for the incorporation of other individual attributes—in particular, age.

Data on predicted and observed *net* new jobs allocated per grade for this 10-year period may also be seen in Figures 8b and c. They are:

	COL	LT	SGT	CPL
Predicted net new jobs	11	13	59	115
Observed net new jobs	4	1	52	178
Error	+7	+12	+7	−63

We observe that all 7 promotion errors (40–33) to COL are accounted for by the error in new job allocations, as well as about half of the overestimates at LT. New job allocation errors at the SGT grade are small. The largest errors clearly arise in the CPL grade with underestimates of both new jobs and exits. The basis for new job allocation errors may be observed by comparing the predicted and observed relative grade distributions after the 10-year forecast:

	COL	LT	SGT	CPL	TPR
Predicted (1970)	.0192	.0220	.0989	.1940	.6659
Observed (1970)	.0134	.0129	.0923	.2284	.6530

The predicted values for 1970 are assumed to have the same distribution as in the estimation period, 1950–1959. The mechanism for obtaining this constancy is via $\hat{\rho}$, the parameter for allocation of new jobs, Equation 8. Compared with the observed distribution, we generally find economies of scale in the administrative ratio.[13] Economies of scale occur at COL, LT and SGT grades, but not at CPL, which results in the overpredictions of new jobs in the upper three grades and an underprediction at the CPL grade. Blau's (1970; 1972) organizational analyses suggest decreasing economies of scale as the work force being managed becomes more professional and Meyer (1972) adds to this discussion a component of technology—automation. He finds economies of scale at the first-line supervision of data-processing staff, but more vertical differentiation and less economy of scale for intermediate level management. Our case is the opposite of that found by Meyer; we find economies of scale at intermediate and higher levels of management. The most likely rationale for this unexpected divergence is the sudden shift from a 48- to 40-hour work week in 1966–1967. A historical analysis (Stewman, 1977) suggested labor input should be measured in terms of man-hours as well as number of workers. Thus, a decline in hours per worker would necessitate a corresponding managerial increase at some level, the most probable being the lowest administrative level. Hence, the underestimates of both exits and new jobs at CPL account for almost all of the TPR → CPL promotion errors, with the chain reaction component (223) very close to that which is observed (213).

---

[13] An economy of scale effect, in the context of an organization, is said to operate if a fractional increase in the size of the lower grade(s) requires a less than proportional increase in the size of the higher (administrative) grades. Correspondingly, if the proportional increase in the size of the higher grade were larger, then dis-economies or decreasing economies of scale are in effect.

6. CONCLUSIONS

This has been a first step in comparative analysis of models of organization ILMs with heterogeneous populations. We have comparatively tested two Markovian models of labor supply and a vacancy driven model of labor demand. The demand model's predictive capability was consistently better than either Markovian model. This finding is also consistent with prior ILM tests for homogeneous populations (Young and Vassiliou, 1974; Stewman, 1978). The results in this paper are viewed as particularly encouraging since the demand model made rather strong assumptions about how both vacancy and decision processes behave. In both types of processes, the assumptions are viewed as beginnings. An important step in the future will be to incorporate multivariate attributes of individuals in the selection and exit processes.

In view of these tests, the duration specific Markovian model seems most useful within organizational ILMs as a heuristic tool to summarize career segments. It is less useful as a predictive model than either the organizational Markov chain or the organizational labor demand model.

In summary, we have begun our analysis at the organization level—with internal labor markets defining the first structural effect. Secondly, we have considered an opportunity labor demand model which has secondary promotion impacts beyond those directly generated. (Thus, we treat second-degree structural effects.) Moreover, we have shown that this vacancy-driven model can predict person flows, which add further evidence to the predictive capabilities of vacancy type models in general. (White [1970], Stewman [1975b] and Brown [1975] had previously shown White's vacancy chain model to be a powerful model for predicting vacancy flows.) In brief, the vacancy type of model for person flows is not only more comprehensive theoretically than Markovian models, but it is also more accurate in predictions—long and short term.[14] Thirdly, we have postulated three types of decision-processes: allocation of new jobs, worker exits, and managerial selection. The importance of explicitly including decision processes is that we once again bring back individuals into models of organizational social structure. Moreover, these individuals are assumed to have minds and cognitive abilities to make choices, and, thus, in part at least, participate in actively structuring the basic processes themselves.

REFERENCES

Bartholomew, D. J.
1963 "A multi-stage renewal process." Journal of the Royal Statistical Society B25:150–68.
1973 Stochastic Models for Social Processes. New York: Wiley.
Becker, G.
1975 Human Capital. New York: NBER.
Blau, P. M.
1970 "A formal theory of differentiation in organizations." American Sociological Review 35:201–18.
1972 "Interdependence and hierarchy in organizations." Social Science Research 1:1–24.
Blau, P. M., and O. D. Duncan
1967 The American Occupational Structure. New York: Wiley.
Blumen, I., M. Kogan and P. J. McCarthy
1955 The Industrial Mobility of Labor as a Probability Process. Ithaca, New York: Cornell Studies of Industrial and Labor Relations.
Brown, D.
1975 Men Against Chance. Unpublished manuscript, Department of Educational Administration, SUNY, Buffalo, N.Y.
Caplow, T.
1954 The Sociology of Work. New York: McGraw-Hill.

---

[14] It has been suggested that in certain types of ILMs (e.g., firms in the electronics industry and insurance firms), internal structure and distributions may be much more fuzzy and, hence, difficult to delineate. In such cases, long term forecasts in particular become more difficult.

Charnes, A., W. W. Cooper, R. J. Niehaus
1972 Studies in Manpower Planning. Washington, D.C.: Department of the Navy.

Doeringer, P. B., and M. J. Piore
1971 Internal Labor Markets and Manpower Analysis. Lexington, Mass.: Heath.

Duncan, O. D., D. L. Featherman and B. Duncan
1972 Socioeconomic Background and Achievement. New York: Seminar Press.

Forbes, A. F.
1971 "Markov chain models for manpower systems." Pp. 93–114 in D. J. Bartholomew and A. R. Smith (eds.), Manpower and Management Science. Lexington, Mass.: Heath.

Ginsberg, R. B.
1972 "Incorporating causal structure and exogenous information with probabilistic models: with special references to choice, gravity, migration and Markov chains." Journal of Mathematical Sociology 2:83–104.
1977 "The relationship between timing of moves and choice of destination in stochastic models of migration." Discussion Paper #119, University of Pennsylvania, Philadelphia, Penn.

Hauser, R., and D. Featherman
1974 "Socio-economic achievement of U.S. men, 1962 to 1972." Science 185:325–31.

Heckman, J.
1977 "Statistical models for discrete panel data developed and applied to test the hypothesis of true state dependence against the hypothesis of spurious state dependence." Paper presented at the Conference on the Uses of Panel Data, INSEE, Paris.

Henry, N. W., R. McGinnis and H. W. Tegtmeyer
1971 "A finite model of mobility." Journal of Mathematical Sociology 1:107–18.

Hodge, R. W.
1966 "Occupational mobility as a probability process." Demography 3:19–34.

Hopes, R. F. A.
1971 "Some statistical aspects of manpower planning in the civil service." OMEGA The International Journal of Management Science 1:165–80.

Jencks, C., M. Smith, H. Acland, M. J. Bane, D. Cohen, Herbert Gintis, B. Heynes, and S. Michelson
1972 Inequality.: A Reassessment of the Effect of Family and Schooling in America. New York: Harper.

Kerr, C.
1954 "The Balkanization of labor markets." Pp. 93–109 in E. W. Bakke, et al. (eds.), Labor Mobility and Economic Opportunity. Cambridge, Mass.: MIT Press.

Land, K. C.
1969 "Duration of residence and prospective migration: further evidence." Demography 6:133–40.

Mayer, T.
1972 "Models of intragenerational mobility." Pp. 308–57 in J Berger, M. Zelditch, Jr., and B. Anderson (eds.), Sociological Theories in Progress, Vol. 2. New York: Houghton-Mifflin.

McFarland, D. D.
1970 "Intragenerational social mobility as a Markovian process: including a time stationary Markovian model that explains observed decline in mobility ratio over time." American Sociological Review 35:463–76.

McGinnis, R.
1968 "A stochastic model of social mobility." American Sociological Review 33:712–22.

Meyer, M. W.
1972 Bureaucratic Structure and Authority. New York: Harper and Row.

Mincer, J.
1974 Schooling, Experience and Earnings. New York: NBER.

Morrison, P. A.
1976 "Duration of residence and prospective migrations: the evaluation of a stochastic model." Demography 4:553–61.

Ross, A. M.
1958 "Do we have a new industrial feudalism?" American Economic Review 68:914–5.

Sales, P.
1971 "The validity of the Markov chain model for a class of the civil service." The Statistician 20:85–110.

Sofer, C.
1970 Men in Mid-Career: A Study of British Managers and Technical Specialists. Cambridge: Cambridge University Press.

Sørenson, A. B.
1975 "The structure of intragenerational mobility." American Sociological Review 40:456–71.

Spilerman, S.
1972 "The analysis of mobility process by the introduction of independent variables into a Markov chain." American Sociological Review 37:277–94.
1977 "Careers, labor market structure and socio-economic achievement." American Journal of Sociology 83:551–93.

Stewman, S.
1975a "An application of job vacancy chain model to a civil service internal labor market." American Sociological Review 37:277–94.
1975b "Two Markov models of system occupational mobility: underlying conceptualizations and empirical tests." American Sociological Review 40:298–321.
1977 "Socio-historical analysis of the Michigan State Police: 1917–1970." Unpublished paper, School of Urban and Public Affairs, Carnegie-Mellon University, Pittsburgh.
1978 "Markov and renewal methods for total manpower system." OMEGA The International Journal of Management Science 6:1–11.

Stolzenberg, R. M.
1975a "Education, occupation and wage differences between white and black men." American Journal of Sociology 81:299–323.
1975b "Occupations, labor markets and the process of wage attainment." American Sociological Review 40:645–65.

Tuma, N. B.
1976 "Rewards, resources and the rate of mobility: a nonstationary multivariate stochastic model." American Sociological Review 41:338–60.

U. K. Civil Service Department
1974 "EQUILIBRIUM: a model for producing a graded stead state system." Statistics Division Paper, London.

White, H. C.
1970 Chains of Opportunity: System Models of Mobility in Organizations. Cambridge, Mass.: Harvard University Press.

Wise, D. A.
1975 "Personal attributes, job performance and probability of promotion." Econometrica 43:913–31.

Young, A.
1965 "Models for planning recruitment and promotion of staff." British Journal of Industrial Relations 3:301–10.

Young, A.
1971 "Demographic and ecological models for manpower planning." Pp. 75–97 in D. J. Bartholomew and B. R. Morris (eds.), Aspects of Manpower Planning. New York: American Elsevier.

Young, A., and P-C. G. Vassiliou
1974 "A non-linear model on the promotion of staff." Journal of the Royal Statistical Society 137A:584–94.

RESEARCH NOTES

TRUTH TESTS AND UTILITY TESTS: DECISION-MAKERS' FRAMES OF REFERENCE FOR SOCIAL SCIENCE RESEARCH*

CAROL H. WEISS
Harvard University

MICHAEL J. BUCUVALAS
Louis Harris and Associates, Inc.

American Sociological Review 1980, Vol. 45 (April):302–313

In an effort to contribute to an empirically based "sociology of knowledge application," this paper explores the frames of reference that decision-makers employ in assessing the usefulness of social science research for their work. Analysis of responses of 155 decision-makers in mental health fields to 50 actual research reports reveals five frames of reference: relevance of research topic, research quality, conformity of results with expectations, orientation to action, and challenge to existing policy. All frames are positively associated with perceived likelihood of using a study. Two significant interactions among the frames suggest that, in essence, decision-makers apply a "truth test" and a "utility test" in screening social science research. They judge truth on two bases: research quality and/or conformity with prior knowledge and expectations. They also assess utility on alternaive bases: feasible direction for action and/or challenge to current policy. The ways in which they apply research conclusions to their work is a broader, more diffuse, and wider-ranging process than many earlier investigators have recognized.

In 1949, Robert Merton called for systematic study of applied social science and of the factors that facilitated or impeded its utilization for purposes of practical action. His luminous analysis discussed the cultural, organizational, situational, and cognitive contexts of applied social research and pointed to the need for empirical investigation of their implications for research use. He identified such dimensions as the prevailing images of applied social science and the often divergent value frameworks of policy-maker and researcher as they affect "the relevance, scope and utility of the research itself" (Merton, 1949:173). In the ensuing years, much speculation (but little research) has followed the lines of Merton's prescriptions.

Recently Holzner has continued the discussion by suggesting the development of a "sociology of knowledge application," in effect a sociology of knowledge "turned upside down" (Holzner, 1978:8). Whereas the classical sociology of knowledge was concerned with the social basis of intellectual productions, a sociology of knowledge application would be concerned with the social consequences of knowledge. In particular, it would investigate the conditions under which knowledge is produced, diffused, and applied. Where Merton called for an applied social science of applied social science, Holzner is, in effect, pointing the way toward a basic social science of applied social science. He, too, calls attention to the need for understanding the multiple frames of reference with which actors perceive knowledge and the discrepancies between the frames of reference of knowledge producers and knowledge users.

This paper reports empirical evidence on decision-makers' frames of reference for social science research. Frames of reference, as we use the term, refers to the dimensions of research that are salient to persons in assessing a study, in effect their criteria for accepting or rejecting the results of the research. Based on interviews with 155 officials in upper-level positions

---

* Direct all correspondence to: Carol H. Weiss; Graduate School of Education; Harvard University; Cambridge, MA 02138.

This study was conducted at Columbia University under a grant from the National Institute of Mental Health.

in federal, state, and local agencies in the fields of mental health, alcoholism, and drug abuse,[1] our data come from their responses to 50 actual research reports.[2] Each person read two of the 50 research reports, which we had summarized in standard-format two-page abstracts.[3] They then rated each one on 29 descriptive dimensions, including such items as technical quality, objectivity, applicability of findings within existing programs, congruence with potential users' ideas and values, and political acceptability of the findings.[4] From their ratings of concrete studies, we derived their frames of reference.

Frames of Reference

The analytic strategy for identifying frames of reference was factor analysis. In this case, factor analysis of respondents' ratings was more than a data reduction technique. It was an effort to locate the constellations of study characteristics that formed analytically separable dimensions. It was, in effect, a means to define the constructs that respondents use to describe and assess research. Table 1 shows the five factors that emerged from the analysis.[5]

The first factor, Research Quality, contains a cluster of items related to the scientific merit of the research: technical quality, statistical sophistication, objectivity, quantitative data, internal consistency, data support for recommendations, comprehensive set of explanatory variables, generalizability, validity, addition to descriptive or causal knowledge. This factor captures the basic components of research merit. It includes methodological

---

[1] We interviewed 51 federal, 52 state, and 52 local officials. Respondents at the federal level held positions at bureau chief level or above in the National Institute of Mental Health (NIMH), the National Institute of Drug Abuse (NIDA), the National Institute of Alcohol Abuse and Alcoholism (NIAAA), and their parent body, the Alcohol, Drug Abuse, and Mental Health Administration. State officials served in the tier of positions immediately below the commissioner or director of mental health in ten states, with titles such as deputy director, director of community programs, director of psychiatric facilities, director of addiction services. Local officials were directors of mental hospitals, mental health centers and clinics, or chiefs of service within those facilities. Also interviewed were 50 social scientists who did research under funding from NIMH, NIDA, and NIAAA and 50 members of research review committees in the same Institutes, but we do not discuss those data here.

[2] The reports that we presented to respondents during the interview were drawn from reports of recently completed social science research studies sponsored by NIMH, NIDA, and NIAAA that evidenced a modicum of technical competence. We selected them to be representative of the spectrum of both grant and contract research, with purposive variation on three dimensions: manipulability of major independent variables, administrative implementability of study's conclusions, and political acceptability of the study's findings in light of current policy. A major selection concern was to uncouple these three characteristics; so we developed a matrix representing the eight possible combinations and selected studies to fill all cells of the matrix. We also tried to vary such other dimensions as type of study, type and size of sample, and substantive focus.

[3] The abstracts included sections on: objectives of the study, methods (site, sample, variables investigated, how variables were operationalized, methods of data collection and analysis), major findings, conclusions, and (if relevant) recommendations.

[4] The descriptors were intended to be a comprehensive list of research characteristics, with particular emphasis on those which observers have claimed affect the use of research. The list was developed after extensive review of the literature and considerable pretesting. Twenty-six of the items are listed in Table 1; the other three are described in fn. 5. Respondents rated the extent to which each descriptor applied to the study on a 5-point scale, from "to a great extent" to "not at all."

[5] The factor solution presented is based on 510 ratings by 255 interview respondents, including social science researchers and research review committee members, because we want a solution that can be used in other analyses for comparisons across groups. The solution for decision-makers alone is not materially different. Four of the descriptors that were rated are excluded from the factor solution. "Inexpensive to implement" was not rated by 35% of the respondents; when the item is included in the factor analysis, it forms a separate factor. "On time for a pending decision" and "deals with a high priority issue" load on a fifth factor with the item, "relevant to the issues your office deals with," which was asked only of decision-makers. This factor, which we call Relevance, is not included here because the key item was rated only by decision-makers and "on time for a pending decision" has missing observations in 41% of the cases. For analysis of decision-makers' responses, we use the single item, "relevant to the issues your office deals with" as the indicator of Relevance. The solution presented is the result of an oblique rotation using a direct oblimin criterion. The four unrotated factors were the principal components with eigenvalues greater than 1.0 from a matrix of Pearson correlation coefficients with missing observations deleted pairwise.

Table 1. Factors of Research Characteristics

Loading[a]	Factor I (Research Quality)
.922	Technical quality of the research is high
.890	Statistically sophisticated
.794	Objective, unbiased
.745	Provides quantitative data
.702	Findings are internally consistent and unambiguous
.634	Recommendations are supported by the data
.625	Comprehensive—that is, includes most of the potentially explanatory variables in analysis
.612	Generalizable to equivalent populations
.569	Validity of findings
.560	Adds to descriptive, causal, theoretical knowledge
	Factor II (Action Orientation)
.706	Contains explicit recommendations
.703	Analyzes effects of factors that decision-makers can do something about
.664	Targeted—that is, focuses on a narrow set of dependent, or outcome, variables
.661	Has direct implications for a course of action
.605	Findings can be applied within existing agencies and programs
.526	Adds to practical knowledge about the operation of policies or programs
	Factor III (Conformity to User Expectations)
.745	Supports a position already held by the user
.718	Consistent with a body of previous knowledge
.663	Compatible with the ideas and values of the potential user
.613	Agrees with respondent's sense of the situation
−.550	Findings unexpected or novel (negative)
	Factor IV (Challenge to Status Quo)
.764	Challenges existing assumptions and institutional arrangements
.738	Implies the need for major change in philosophy, organization, or services
.574	Raises new issues or offers new perspectives
−.437	Implications of the findings are politically acceptable (negative)

[a] The loadings are from the factor pattern matrix after oblique rotation. All loadings over .40 are shown.

and normative items, items related to internal and external validity, and items suggesting appropriate theoretical grounding (for example, "comprehensive set of explanatory variables") as well as appropriate methods.

The second factor deals with the practicality of implementing the findings. Labeled Action Orientation, it contains the items on practical problem-solving: explicit recommendations, manipulable variables, targeted set of dependent variables, direct implications for a course of action, applicability within existing programs, and addition to *practical* knowledge. This is the frame of reference widely attributed to clients of applied research. It tells them what to do on things that they can do—within existing constraints.

Conformity to User Expectations, the third factor, represents compatibility with what users know and believe. As social scientists have long suggested, decision-makers view research through the lens of their previous beliefs. The items in this factor include: support for users' position, consistency with previous knowledge, compatibility with users' ideas and values, agreement with the users' sense of the situation, and findings *not* unexpected or novel. Thus, the Conformity frame of reference contains the notion of congruence with users' values and policy positions, but even more pervasive is the sense of conformity with what they already know—or believe they know. Four of the five descriptors that comprise the factor refer to users' knowledge, ideas, sense of the situation, and expectations. The emphasis is on agreement with the users' construction of reality. Furthermore, it is agreement with the users as individuals rather than as organizational members. Items relating to organizational practice and political context load on the next factor.

We call the fourth factor Challenge to the Status Quo. It relates primarily to the operation of institutions rather than to the preferences of individual users, and its thrust is major disagreement with institutional practice. Included in this factor are:

challenge to assumptions and institutional arrangements; implications for major change in philosophy, organization, or services; new issues or new perspectives; and politically *un*acceptable implications.

A fifth frame of reference has to do with the Relevance of a research study to the users' work. Timeliness of the research and high priority of the issue loaded on the same factor as the key item, "relevant to the issue your office deals with," but because of rating procedures and missing observations, we (see fn. 5) use the single item on relevance as the indicator. Clearly, the substantive fit of a study to the users' job responsibilities is a criterion to which people are alert in assessing research. The procedures of our study aimed to ensure relevance by matching studies to respondents' job titles insofar as possible, but enough variability remained so that Relevance emerges as a dimension to which they respond.

The major surprise to us in these data is respondents' separation of their own beliefs from the beliefs and arrangements underlying institutional practice. Conformity to User Expectations and Challenge to the Status Quo are separate dimensions, not opposite ends of a continuum. In our data, they are not negatively correlated; they are essentially not correlated at all (r = .05). The pattern of data suggests that people in positions of organizational authority are about equally likely to agree—as they are to disagree—with findings that challenge institutional policy and practice. Although the items referred to mental health arrangements and practice, generally, rather than to the practices of the particular agencies with which each respondent was connected, there is little support here for the prevalent assumption that bureaucrats are firmly committed to institutional policy.

Relationship of Frames of Reference to Usefulness

Having located five frames of reference, the next step was to see what effect each factor had on the perceived usefulness of research. One measure of usefulness was the question, "Assuming your office had to consider the issues discussed in the study, how likely is it that you would take the study results into account?" Respondents rated each study on a 5-point scale (analogous to the 5-point ratings on the descriptor items) from "very likely" to "not likely at all."

It can be argued that the perceived usefulness of social science research is a soft indicator and that actual use would be more appropriate. However, one of the most powerful learnings from the first round of empirical investigation of research utilization (for example, Caplan et al., 1975; Caplan and Barton, 1976; Rich, 1975; Patton et al., 1977) is that "use" is an exceedingly ambiguous concept. Officials interpret using research to mean anything from adoption of the recommendations of a single study all the way to a general sensitivity to social science perspectives (cf. Caplan et al., 1975:18–19). And not only are interpretations of use highly variable, but also the *phenomenon* of use is an amalgam of diverse activities. People can use social science research to clarify the relative advantages of alternative choices, but they also use it conceptually: to understand the background and context of program operation, stimulate review of policy, focus attention on neglected issues, provide new understanding of the causes of social problems, clarify their own thinking, reorder priorities, make sense of what they have been doing, offer ideas for future directions, reduce uncertainties, create new uncertainties and provoke rethinking of taken-for-granted assumptions, justify actions, support positions, persuade others, and provide a sense of how the world works. Pelz (1978:347) has summarized the diverse uses of research in three categories: instrumental/engineering, conceptual/enlightenment, and symbolic/legitimative. (See also Rein, 1976; Gans, 1971; Coleman, 1972; Lazarsfeld and Reitz, 1975; Lindblom and Cohen, 1979; Straussman, 1978; Aaron, 1978; Cohen and Garet, 1975; Cherns, 1976; Cherns et al., 1972; Weiss, 1979; 1980; Frankel, 1976). We want to capture the array of uses of social science research, partly because direct and immediate applications of research to decisions are rare—given the nature of organizational decision-making processes;

more importantly, direct applications are one extreme of a continuum that gradually shades into uses that are less immediately obvious but, nonetheless, real—and potentially powerful in cumulative consequence.

Before any kind of use can occur, officials have to consider a study worth their attention and give it a hearing. Their determination that a study deserves notice and should be taken into account in their work is what our measure taps. Much information comes and goes without a trace, but a study that is deemed worthy of serious attention deposits a residue of information and ideas that can be retrieved when circumstances are conducive to use.

We regressed the usefulness ratings on the four factors and Relevance. Table 2 presents the standardized regression coefficients. All of the factors are positively associated with the usefulness of research studies, and all of them are significant predictors. Together they account for 42% of the variance in the likelihood of using a research study. Characteristics of studies make a sizable difference in their usefulness.

In light of much of the previous literature on research use, two of the coefficients are unexpected. First is the salience of Research Quality, which has the highest coefficient in the equation. The weight of opinion among social scientists (including those whom we interviewed in this inquiry) is that decision-makers are unconcerned about scientific quality: if a study supports what they want to do, they accept and use it, but if it does not, they reject it (Horowitz, 1970; Komarovsky, 1975; Wilson, 1978). One possible explanation for the unexpected showing is that respondents are overstating their allegiance to Research Quality in the interests of social acceptability. However, the questions in the interview did not ask them directly about the factors that affected their likelihood of using a study. Rather, each person rated the usefulness of the study, answered a set of open-ended questions, and then rated the study on the descriptors. Our analysis is based on the statistical relationships between the ratings of the descriptors and of usefulness.

Another set of evidence suggests that lying is not the explanation. Early in the interview, we asked respondents' *opinions* about the characteristics of research studies that would affect their use. Here, if anywhere, social desirability biases should surface. On this question, some of the items that were part of the Research Quality factor received high ratings but others did not, and comparison (between the opinion ratings and the effects of the same items on the usefulness ratings) indicated little correspondence. The items that a person *believed* were important for use did not match the items that were associated with that person's own ratings of the usefulness of a study. This datum makes the notion of purposeful misreporting less persuasive.

On the other hand, in the interview a person may have tended to inflate the Research Quality ratings of the studies he/she found useful. Such a tendency may have elevated the relationship between Research Quality and likelihood of use. However, her/his ratings of Quality correlated significantly, study by study, with the ratings given by social science researchers and members of research review committees. And even if we allow for some possible ambiguity in the causal

Table 2. Regression of Ratings of Likelihood of Use on Research Characteristics

Independent Variables	Unstandardized Regression Coefficient	Standard Error	Standardized Regression Coefficient
Relevance	.14	.05	.15
Research Quality	.54	.08	.39
Conformity to User Expectations	.25	.07	.18
Action Orientation	.16	.08	.12
Challenge to the Status Quo	.26	.08	.19
Constant	3.34		
$R^2 = .42$.			

relationship, the strong association between Research Quality and usefulness remains noteworthy.

There are good reasons for consideration of a study's Research Quality within the environment of an operating agency. When officials rely on social science research as a source of information, rather than such available alternatives as impressionistic accounts, program records, or the agency grapevine, the rationale is that research provides more valid information. The claim to validity is based on adherence to the canons of science and, unless the canons are obeyed, research is expensive and troublesome, with no gain in accuracy. Furthermore, agency actions can have large consequences—in terms of dollars and human lives. If officials are to take research seriously, they want to have confidence that the evidence they consider is trustworthy.

Other data in the interview show that there are political reasons for insisting on Research Quality. One question asked how appropriate each study report would be for seven listed functions, such as raising an issue to attention, and formulating new policies and programs. Analysis showed that Research Quality was a particularly important feature of studies that were appropriate for two purposes: mobilizing support for a position or point of view, and changing ways of thinking about an issue. The suggestion is that Research Quality is particularly valued for studies that enter the arena of debate. If officials rely on research in the adversarial process of decision-making, they want the study to provide effective ammunition. If it can be discredited on methodological grounds, they are left out on a limb. Research Quality, thus, not only assures decision-makers that they are basing a case on good evidence but also serves the political function of protecting the case from methodologically based criticism.

The second surprise in Table 2 is the positive and significant regression coefficient for Challenge to the Status Quo. One would think that a study that challenges existing practice, implies the need for fundamental change, and is politically unacceptable would hardly seem useful. But mental health decision-makers rate studies that score high on Challenge as particularly likely to be taken into account.

Three explanations are suggested by the data. First, recall that respondents' ratings of the research studies indicate low levels of allegiance to existing practice. The correlation between the factor scores for Challenge to the Status Quo and Conformity to User Expectations is essentially nil. Therefore, studies that challenge current practice are as likely to accord with their beliefs as to contravene them. As one person noted, unless a mental health official is dissatisfied with today's programs and looks toward change, he/she forgoes a position of leadership in the field.

Second, usefulness is defined in the interview as the likelihood of taking the study into account in the work of one's office. This definition does not demand that respondents expect to implement the results or make a specific decision differently because of the results. They can take the research into account by considering its implications. This kind of research use lacks the crispness of the traditional formulation of "research utilization": using research as a basis for choosing between decision A and decision B. But elsewhere in the interview, they described their previous experiences in using research, and their descriptions show that they rarely rely on research to make such clear-cut decisional choices. Instead, the use of research in large organizations is primarily to provide background, understanding, awareness of new ideas. When they turn to research (and most of our respondents reported that they read research and believed it made a difference to their work), it is precisely for the seemingly diffuse purposes of enlightenment. Thus, the measure accords with the modes of use that are prevalent in these agencies (Caplan et al., 1975; Patton et al., 1977; Weiss, 1980; Alkin et al., 1979).

In this context, Challenge to the Status Quo is a positive feature of research. When research raises questions about current policy and suggests fundamental change, it helps people learn about problems and identify alternatives. Research that confirms that current policy is working well offers little food for thought. Research that challenges policy stimulates people to rethink their positions and to grapple with new opportunities. They find such research valuable.

A third reason for the strong showing of Challenge to the Status Quo may be that the research studies that we presented to respondents were drawn from the stock of recent federally supported social science research studies. They exhibited varying degrees of Challenge to the Status Quo, but none questioned fundamental assumptions of the economic or social order. As radical as they may have been on mental health policy issues at the time (for example, offering evidence to support community control of care-giving facilities), they did not suggest basic changes in societal institutions. We are dealing with Challenge within a limited range.

The other data in Table 2 are more expected. The Relevance of a study to a user's job responsibilities increases its likelihood of being taken into account. So does its Conformity to User Expectations, since research that is intuitively "right" and supportive of preconceptions is easier to accept. So, too, does Action Orientation, the set of characteristics that makes a study feasible to apply. That Action Orientation does not show a higher association with likelihood of use, as many social scientists would expect, is probably attributable to the same definitional premise that underlies the salience of Challenge. Because research studies are seldom used as a direct basis for decisions, Action Orientation turns out to be less important than is commonly asserted. But the practicality dimension embedded in this frame of reference retains a positive effect on usefulness.

Truth Tests and Utility Tests

From a sociology of knowledge perspective, we can see that decision-makers use two tests to screen incoming social science research: a truth test and a utility test. They are concerned with the validity of research results, and they are concerned with the direction that research provides for future action. In terms of validity, they have two bases on which to make their evaluation: the degree to which the research adheres to the canons of the scientific method (Research Quality) and the degree to which the findings of the research accord with their previous knowledge of how the world works (Conformity to User Expectations). Each component of the truth test represents an alternative mode for judging whether the results are accurate.

Scientists are accustomed to judging validity primarily on the basis of the research *process*—the degree to which appropriate theory and methods were used to arrive at conclusions. Decision-makers, in addition, judge validity by the *outcomes* of the research. They invoke a criterion derived from their direct involvement in the substantive area, and they assess the extent to which research outcomes agree with their firsthand experience and professional judgment. Because they are exposed to a variety of evidence—direct observation, descriptive accounts, program data, routine statistics, colleagues' reports, as well as a body of previous research—they use their stock of knowledge to help them judge the truth of a study. Of course, this kind of comparison is not unknown to scientists, who also have referents in an existing body of knowledge. The fact that decision-makers' prior knowledge is less formal and systematic does not diminish, but perhaps strengthens, its salience. It has the vividness of immediacy.

Thus, the truth test is composed of two independent dimensions. Decision-makers appraise a study in terms of its technical merit, but they have another basis for screening its truth claims—its conformity with their prior understanding and experience.

The utility test, too, is made up of two analytically distinct components. One is the extent to which a study provides explicit and practical direction on matters they can do something about—the constellation of characteristics we have called Action Orientation. This dimension captures the essence of the injunctions to applied social scientists that abounds in the "problem-solving" literature: be practical, accept decision-makers' parameters of action, analyze manipulable variables, draw explicit recommendations. It represents one of the grounds on which decision-makers test a study's utility.

But there is a different kind of guidance that social science research can provide, which has been called "enlightenment" (Crawford and Biderman, 1969; Janowitz, 1970). It encompasses a range of conceptual contributions, from helping to "establish new goals and bench marks of the attainable" (Merton, 1949:170), to helping to "enrich and deepen understanding of the complexity of problems and the unintended consequences of action" (Aaron, 1978:166), to guiding the "effort to interpret and structure the social world by establishing languages and symbolic universes used in comprehending and carrying on social life" (Cohen and Garet, 1975:42). Such conceptual uses suggest that there are alternatives to working within decision-makers' established constraints and to generating data to aid them in weighing the costs and benefits of available options. Research can be useful by challenging current practices and suggesting new perspectives and orientations. Our data show that decision-makers value research that scores high on Challenge to the Status Quo. They are receptive to findings about the ineffectiveness and dysfunctions of current policy as well as about its successes; they are open to new formulations and approaches, even when these are not yet politically acceptable.

Just as Research Quality and Conformity to User Expectations seem to be alternative ways of satisfying the truth test, so Action Orientation and Challenge to the Status Quo appear to be alternative grounds for satisfying the utility test. The next section analyzes the extent to which each test is in fact an entity.

Components of the Truth and Utility Tests

The components of Research Quality and Conformity to User Expectations constitute a unitary truth test if the level of one factor is contingent on the level of the other. Our hypothesis is that the more closely research results fit with what users already know (Conform to their Expectations), the less important will Research Quality be for their likelihood of taking the study into account. Conversely, when a study's conclusions diverge from the "known," the more important is its Research Quality for overcoming their doubts and motivating them to take it seriously. Similarly, we hypothesize that Action Orientation and Challenge to the Status Quo comprise a utility test because they provide *alternative* types of direction. If a study scores high on one component, it is less important to the study's usefulness that it score high on the other. Although the hypotheses are different in substance, they can both be represented by negative interactions between the factors.

Accordingly, we constructed product terms for the interaction of Research Quality and Conformity and for the interaction of Action Orientation and Challenge to the Status Quo, and added the two terms to the regression equation presented in Table 2 (Cohen and Cohen, 1975:chap. 8). Table 3 presents the unstandardized regression coefficients[6] from the regression of likelihood of use on Relevance, the four factors, and the two interactions.

Both hypothesized interactions are negative, substantial, and statistically significant ($p < .01$). Together they add 3% to the variance explained, an amount significant at $p < .01$. None of the other possible two-way interactions (not presented) is of comparable magnitude or statistically significant at the .05 level.

We hypothesized that the effects of Research Quality on usefulness are contingent on Conformity to User Expectations, rather than vice versa, because

---

[6] When there are no interactions, unstandardized coefficients are effect, or slope, coefficients; they indicate how much change will occur in the predicted value of the dependent variable for a one-unit change in any particular independent variable. When there are interaction terms, the situation is more complex. Slopes are rates of change and mathematically are equivalent to first partial derivatives. If we want to compute the slope of the regression surface for any independent variable, we must take the first partial derivative of the regression equation for that particular variable.

Table 3. Regression of Ratings of Likelihood of Use on Research Characteristics and Hypothesized Interactions

Independent Variables	Unstandardized Regression Coefficient	Standard Error	Standardized Regression Coefficient
Relevance	.13	.05	.14
Research Quality	.55	.07	.39
Conformity to User Expectations	.26	.07	.19
Action Orientation	.13	.07	.10
Challenge to the Status Quo	.28	.07	.20
Quality × Conformity	−.20	.07	−.12
Action Orientation × Challenge	−.19	.07	−.12
Constant	3.14		
$R^2 = .45$.			

decision-makers' knowledge and beliefs antedate their review of the study, and their judgments of its truth depend on their stock of knowledge. The interaction term itself is symmetric and does not indicate which effect is contingent. We impose the limiting concept. What the interaction term tells is the magnitude of change in the effects of Research Quality.

By calculating the first partial derivative of the regression equation for Research Quality, we see that the effect of Research Quality on likelihood of use is .55−(.20 × Conformity). When the value of Conformity is 0 (its mean), the slope of Quality is .55. For each unit increase in Conformity, the effect of Quality is attenuated by .20, so that studies with Conformity scores of +1.0 would show increases of .35 in likelihood of use for each unit increase in Quality. For each unit decrease in Conformity, the effect of Quality is enhanced by .20; studies with Conformity scores of −1.0 would show increases of .75 in likelihood of use for each unit increase in Quality. Thus, Research Quality has more than twice as large an effect when Conformity is −1.0 as it has when Conformity is +1.0. If a study yields counterintuitive findings, Research Quality is much more important than it is when findings are compatible with users' preexisting ideas.

The pattern of data raises the possibility that if Conformity were high enough, the effect of Quality would be negative. This does not happen within the existing range of data. Conformity scores go from −2.98 to +2.04, so that, at the highest end, Research Quality is still positive (.14). Our guess, based on the conceptualization of the truth test, is that increased Quality would never decrease usefulness, other factors being controlled. At worst, the effect of Quality might be nil, but it is clearly most significant when findings do not accord with users' beliefs.

In the utility test, where the two component factors represent alternative modes of providing direction, both the interaction term and the conceptualization are symmetric. Therefore, we compute both slopes. For Action Orientation, the slope is .13−(.19 × Challenge to the Status Quo). For Challenge, the slope is .28−(.19 × Action Orientation).

For the slopes of both components, the attenuation coefficient of the other component is relatively large, compared with the constant. Both factors have negative effects on likelihood of use within the existing range of data. For Action Orientation, the slope becomes negative for all values of Challenge of .68 or above; this represents almost one-quarter of the cases. For Challenge, the effects become negative when Action Orientation takes on values over 1.47 (about 5% of the cases). When both components are high, the study scores lower on likelihood of use than a simple additive combination would predict.

In fact, the size of the coefficient of the interaction term relative to the coefficients of the first-order terms suggests that in cases where both factors were high enough, an increment in either of them would actually decrease the study's likelihood of use. No such cases occur in our data set; there are cases where one of the components enters the required range, but in these cases the other does not.

Highly Challenging research that is also high on Action Orientation is probably rare. If it did occur, the negative interaction suggests that it would actually decline in usefulness.

The "trade-off" effect demonstrates that the two bases of utility are alternatives to each other. Action Orientation is much more important when a study does not Challenge the Status Quo, and Challenging results are more useful when a study is not Action-Oriented. When a study's results are feasible to implement, evidence of major deficiencies in practice or fundamentally new perspectives do not add much to its usefulness. When a study suggests radical redirection of policy or program, explicit direction for implementation adds relatively little to usefulness; respondents are receptive to its ideas, but they are not prepared to take immediate steps to carry out its recommendations.

Conclusion

Decision-makers in federal, state, and local mental health agencies are exposed to reports of large numbers of social science research studies—in books, journals, professional magazines, and the mass media; in reports from agencies' contract and grant awards, routed from other divisions and mailed in by researchers; through conferences, meetings, and colleagues' discussions. Their professional backgrounds (largely in psychiatry, psychology, and social work) attune them to research procedures, and they are generally receptive to the contributions that the social sciences can make to policy and practice.[7] In coping with the incoming flood of information, they invoke three basic frames of reference. One is the relevance of the content of the study to their sphere of responsibility, another is the trustworthiness of the study, and the third is the direction that it provides. The latter two frames, which we have called truth and utility tests, are each composed of two interdependent components: *Truth test:* Is the research trustworthy? Can I rely on it? Will it hold up under attack?

1. Research Quality. Was the research conducted by proper scientific methods?

and/or

2. Conformity to User Expectations. Are the results compatible with my experience, knowledge, and values?

Utility test: Does the research provide direction? Does it yield guidance—either for immediate action or for considering alternative approaches to problems?

1. Action Orientation. Does the research show how to make feasible changes in things that can feasibly be changed?

and/or

2. Challenge to the Status Quo. Does the research challenge current philosophy, program, or practice? Does it offer new perspectives?

Together with Relevance (that is, the match between the topic of the research and the person's job responsibilities), these four dimensions constitute the frames of reference by which decision-makers assess social science research. Research Quality and Conformity to User Expectations form a single truth test in that their effects are contingent on each other: Research Quality is less important for the usefulness of a study when results are congruent with officials' prior knowledge than when results are unexpected or counterintuitive. Action Orientation and Challenge to the Status Quo represent alternative functions that a study can serve. They comprise a utility test, since the kind of explicit and practical direction captured by the Action Orientation frame is more important for a study's usefulness when the study provides little criticism or reorientation (Challenge to the Status Quo) than it is when Challenge is high. Conversely, the criticism of programs and new perspectives embedded in Challenge to the Status Quo add more to usefulness when a study ignores feasible prescriptions for implementation.

It is worth emphasizing that decision-makers are receptive to studies that are high on Challenge to the Status Quo, with its connotation of fundamental reorientation in policy assumptions and practice. They want to hear reports critical of current programming and they are willing to take account of variant perspectives. Given the emphasis in much previous work on bureaucratic inertia and resistance to change, this openness in mental health agencies strikes an optimistic note—even though their attention to such research provides no guarantees that it will affect immediate decisions in direct and discernible ways.

---

[7] Caplan et al. (1975), who interviewed high officials with much more diversified backgrounds across a wide range of federal departments, found equally receptive attitudes toward social science research.

Finally, it is important to note that "using research" is a much broader and more diffuse concept here than the research-for-problem-solving literature adopts. Our formulation, "taking research into account in the work of one's office," does not restrict use to direct effect on decisions. It allows for the multiple functions that research can serve—generating awareness of needs, problems, and shortcomings in service; providing news about strategies that are proving effective; alerting people to environmental changes and emerging issues; reducing the uncertainties that inevitably attend complex issues; justifying current policies; offering ideas and concepts for making sense of experience; constituting a language for communication with other actors in the public arena. Other data in the study confirm that officials "use" research in this variety of ways (Weiss, 1980) and that research can be "useful" to them on many bases. Our understanding of research utilization has to go beyond the explicit adoption of research conclusions in discrete decisions to encompass the assimilation of social science information, generalizations, and ideas into agency perspectives as a basis for making sense of problems and pondering strategies of action.

REFERENCES

Aaron, Henry J.
1978 Politics and the Professors: The Great Society in Perspective. Washington, D.C.: The Brookings Institution.

Alkin, Marvin C., Richard Daillak, and Peter White
1979 Using Evaluations: Does Evaluation Make a Difference? Beverly Hills: Sage.

Caplan, Nathan, and Eugenia Barton
1976 Social Indicators 1973: A Study of the Relationship Between the Power of Information and Utilization by Federal Executives. Ann Arbor: Center for Research on Utilization of Scientific Knowledge, Institute for Social Research, The University of Michigan.

Caplan, Nathan, Andrea Morrison, and Russell J. Stambaugh
1975 The Use of Social Science Knowledge in Policy Decisions at the National Level: A Report to Respondents. Ann Arbor: Institute for Social Research, The University of Michigan.

Cherns, Albert B. (ed.)
1976 Sociotechnics. London: Malaby.

Cherns, Albert B., R. Sinclair, and W. I. Jenkins (eds.)
1972 Social Science and Government: Policies and Problems. London: Tavistock.

Cohen, Jacob, and Patricia Cohen
1975 Applied Multiple Regression/Correlation Analysis for the Behavioral Sciences. New York: Wiley.

Cohen, David K. and Michael Garet
1975 "Reforming educational policy with applied social research." Harvard Educational Review 45:17–43.

Coleman, James S.
1972 Policy Research in the Social Sciences. Morristown: General Learning Press.

Crawford, Elisabeth T., and Albert D. Biderman (eds.)
1969 Social Scientists and International Affairs. New York: Wiley.

Frankel, Charles (ed.)
1976 Controversies and Decisions: The Social Sciences and Public Policy. New York: Russell Sage.

Gans, Herbert J.
1971 "Social science for social policy." Pp. 13–33 in I. Horowitz (ed.), The Use and Abuse of Social Science. New Brunswick: Transactionbooks.

Holzner, Burkart
1978 "The sociology of applied knowledge." Sociological Symposium 21:8–19.

Horowitz, Irving L.
1970 "Social science mandarins: policymaking as a political formula." Policy Sciences 1:339–60.

Janowitz, Morris
1970 Political Conflict: Essays in Political Sociology. Chicago: Quadrangle.

Komarovsky, Mirra (ed.)
1975 Sociology and Public Policy. New York: Elsevier.

Lazarsfeld, Paul F., and Jeffrey G. Reitz
1975 An Introduction to Applied Sociology. New York: Elsevier.

Lindblom, Charles E., and David K. Cohen
1979 Usable Knowledge: Social Science and Social Problem Solving. New Haven: Yale University Press.

Merton, Robert K.
1949 "The role of applied social science in the formation of policy: a research memorandum." Philosophy of Science 16:161–81.

Patton, Michael Q., Patricia Smith Grimes, Kathryn M. Guthrie, Nancy J. Brennan, Barbara Dickey French, and Dale A. Blyth
1977 "In search of impact: an analysis of the

utilization of federal health evaluation research." Pp. 141–64 in C. H. Weiss (ed.), Using Social Research in Public Policy Making. Lexington: Lexington.

Pelz, Donald C.
1978 "Some expanded perspectives on use of social science in public policy." Pp. 346–57 in J. M. Yinger and S. J. Cutler (eds.), Major Social Issues: A Multidisciplinary View. New York: Free Press.

Rein, Martin
1976 Social Science and Public Policy. New York: Penguin.

Rich, Robert F.
1975 "The power of information." Ph.D. dissertation, Department of Political Science, University of Chicago.

Straussman, Jeffrey D.
1978 Limits of Technocratic Politics. New Brunswick: Transactionbooks.

Weiss, Carol H.
1979 "The many meanings of research utilization." Public Administration Review 39:426–31.
1980 "Knowledge creep and decision accretion." Knowledge: Creation, Diffusion, Utilization 1. Beverly Hills: Sage.

Wilson, James Q.
1978 "Social science and public policy: a personal note." Pp. 82–90 in L. E. Lynn, Jr. (ed.), Knowledge and Policy: The Uncertain Connection. Washington, D.C.: National Academy of Sciences.

THE SPATIAL STRUCTURE OF INCOME DISTRIBUTION IN RURAL REGIONS*

DAVID A. MCGRANAHAN
University of Wisconsin

American Sociological Review 1980, Vol. 45 (April):313–324

Drawing on the concepts of metropolitan spread and central place hierarchy, this paper develops a spatial model of income distribution in rural regions. Central to the model is the argument that proximity to growing metropolitan centers and community commercial centrality affect incomes differentially, in ways that depend on education, age, and proprietorship. It is argued that these differential effects are reflected in the distribution of income across and within communities, having implications for community stratification, structure, and, following Molotch (1976), the differentiation of interests in community growth.

INTRODUCTION

An important thesis in current stratification research is that the labor market is segmented, that the effects on income of education, experience, and other individual attributes are not uniform but depend on position in the socioeconomic structure—namely, on class position (Wright and Perrone, 1977); on the central or peripheral nature of the industry (Bibb and Form, 1977; Beck et al., 1978); and on the size of the employing organization (Stolzenberg, 1978). Are there also aspects of spatial structure which affect relationships between individual characteristics and income, or is the distribution of income across and within communities and areas simply a reflection of the spatial division of labor, with migration serving to integrate the structure of opportunities?

This question has received little theoretical or empirical attention and the evidence is unclear. Income levels depend on location as well as individual attributes (Duncan and Reiss, 1956; Hanushek, 1973; Parcel, 1979), but this could simply reflect areal differences in the cost of living. Studies of relationships between indi-

---

* Direct all communications to: David A. McGranahan; Department of Rural Sociology; University of Wisconsin; Madison, WI 53706.

Revised version of a paper presented at the annual meeting of the Midwest Sociological Society, 1979. Research supported by the National Institute of Mental Health (Grant No. ROL MH 25266–01, E. A. Wilkening, P. I.), and the Research Division of the College of Agricultural and Life Sciences, University of Wisconsin-Madison. I thank D. V. McGranahan, E. A. Wilkening, V. Lambert, D. Featherman, and J. G. Williamsen for commenting on earlier versions of this paper.

vidual characteristics and income status within communities indicate that there is little variation among the largest U.S. northern cities (Mueller, 1974), that there is no systematic variation by community size (Hochbaum et al., 1955; Lane, 1968; Hauser and Featherman, 1977; Curtis and Jackson, 1977), but that there may be differences among smaller communities (Curtis and Jackson, 1977). The more recent of these studies have generally concluded that communities do not have economic stratification systems which are separate from that of the larger society. Given that the annual intercounty migration rate is about 10% for people aged 20–44 (U.S. Department of Commerce, 1977), this conclusion would seem reasonable.

The question of how location and income are interrelated may be approached from a different perspective, however. For instance, migration is a highly selective process. Research has consistently shown that the propensity to migrate increases with education and decreases with age, until the age of retirement (Ritchey, 1976). The effects of location on income may be greater for those segments of the labor force with relatively little geographical mobility. Thus, instead of asking how relationships of individual characteristics to income may be influenced by community or area context, it may be useful to look at how the importance of location on income varies, depending on individual characteristics. This point is important because the above community studies are based on very small or pooled samples of communities,[1] selected or combined on the premise that communities or areas have contained stratification systems, with city size (rather than growth or decline, for instance) as the major contextual variable.

This paper develops a model of income distribution in rural regions, focusing on ways in which incomes may vary by location, depending on individual characteristics. In the first sections of the paper, the differential effects on incomes of metropolitan spread and community commercial centrality are discussed with reference to education, age, and proprietorship; and the implications of these differential effects for community income levels and inequality are examined. Education and age are key variables in stratification (and mobility) research. While the distinction between proprietary incomes and wage and salary incomes is usually treated only peripherally (but, see Wright and Perrone, 1977), it is especially important in rural regions where many of the businesses are individually or family owned. Evidence for the model is then presented, using individual and community level data from northwestern Wisconsin. The discussion section briefly relates the model to the differentiation of interests in community growth and change (see Molotch, 1976). It should be stated that the argument developed here is not meant to apply directly to larger centers, where major businesses are less likely to be owned by individuals or families, and where unionization may have an important bearing on incomes. Nor is it the intention of this paper to provide a complete explanation of income distribution at the individual or community level.

METROPOLITAN SPREAD AND RETURNS TO EDUCATION AND EXPERIENCE

Research on income inequality using areal units such as states (Al-Sammarie and Miller, 1967; Aigner and Heins, 1967; Sale, 1974; Chiswick, 1974), counties (Farbman, 1973; Gunther and Ellis, 1977), and cities (Murray, 1969; Frech and Burns, 1971; Betz, 1972) has consistently shown that where incomes are relatively high they are more equally distributed.

In general, it has been impossible to tell whether these results are anything more than a reflection of demographic, educational, and/or occupational variations resulting from a spatial division of labor. However, Chiswick (1974), using census tabulations of income by education for males over 25, derived a measure of returns to education for each state and found that this indicator, which reflects percentage increases in income per year of schooling, was highly associated with state income inequality (r = .77), while the interstate correlation between inequality in schooling and income inequality was relatively low (r = .48). This result was also found when southern states were excluded. Chiswick (1974:69–71) suggests that the greater mobility of people with more education means that they participate in a national labor market, and that their incomes are relatively equal across areas. On the other hand, people with less education, being less mobile geographically (but perhaps more mobile occupationally), are subject to local labor market conditions. Where their incomes are low, average incomes are low and income inequality high.

---

[1] Apart from Mueller (1974) who studied 15 cities, the maximum number of areas or communities used has been 6.

A similar argument may be made with respect to age. Geographic mobility tends to decline as people get older. The social costs of migration increase and, because there are fewer years left in the labor force for older people, larger gains in income are required to make migration economically worthwhile (see Ritchey, 1976). The incomes of older members of the labor force may therefore be more affected by spatial shifts in the economy than the incomes of younger people. In general, incomes increase with age or experience, at least until the age of 40 or 50. However, in poorer areas with declining economies, where there has been little chance of improving one's position over time and where people have been forced to shift occupations, losing expected gains from experience, the effect of age on income may be relatively small.

This suggests a relationship between areal economic conditions and income inequality which is more complex than that envisioned by Chiswick (1974), yet consistent with his results. Because older people tend to have had less schooling, returns to experience and returns to education tend to counterbalance each other in their effects on income inequality—those with less education have the benefit of greater experience, and vice versa. However, in areas of economic decline, greater experience does not compensate for less education and the result is a steeper effect of education on income and a tendency for income inequality to be greater in poorer areas.

Geographers and planners following earlier work by sociologists (e.g., Bogue, 1949) have come to see rural area development to be a function of proximity to growing metropolises. Positive spread effects of centers depend on the vitality and the size of the center and diminish with distance from the center (Berry, 1970; Peet, 1978). Negative or "backwash" effects also occur, involving the flow of capital and younger, better educated workers out of more peripheral areas (Richardson, 1976). Farms on the periphery are more labor intensive and less affluent, as alternative employment opportunities are lacking and capital is less available (Nicholls, 1961). The net effect has been reflected in lower median incomes and lower absolute growth in incomes over time as one moves away from metropolitan centers, up to a distance of 100 miles (Lamb, 1975). Spread effects have increased over the past 25 years as areas on the periphery have become part of the "urban field," specializing in industry, recreation, and other urban-oriented functions, but a large intermetropolitan periphery has remained (Friedmann and Miller, 1965).

This pattern of spread and backwash suggests that the incomes of the young and highly educated should be roughly equal throughout a region, but that older people with low education, caught in declining sectors in peripheral areas and unable to command the wages prevailing in areas of metropolitan spread, should have much lower incomes away from growing centers.

Nonfarm proprietary incomes might be expected to be higher near major cities, but there are factors which equalize these incomes across a region, at least for larger businesses. Thus, while proprietors near a major city are in a richer area, they must also pay higher wages and compete with urban businesses in terms of prices, a factor which results in a low level of retail services relative to population for communities near large cities (Johansen and Fuguitt, 1979). In general, the major effects of metropolitan spread and backwash should be on the incomes of those persons who are older and who have less schooling, tending to make incomes not only lower but also more unequally distributed in areas remote from expanding metropolises.

Commercial Centrality and Proprietary Incomes

Central place theory is concerned with the spatial arrangement of market activities. Places that are commerically more important have functions additional to those found in less central places, and serve fairly well-defined market areas that include these "lower-order" places (Christaller, 1966). While much attention has been devoted to the organization of central places by size, location, and differentiation, (see Berry, 1967; Berry and Pred, 1961), there has been little interest in the patterns of income distribution generated by central places in rural regions.

The thesis put forward here is that greater commerical centrality has relatively little effect on wage and salary incomes but permits larger proprietary incomes, thus tending to make incomes higher and more unequally distributed in commercially more central communities. For the highly educated—because of their participation in regional, state, and even national labor markets—neither community nor area should have a large effect on incomes. For wage earners with less schooling, because of the possibility of commuting, conditions in the labor market area are more important than the commercial importance of their communities. However, proprietary incomes are likely to increase with community commercial centrality for several reasons. First, greater centrality permits larger businesses with economies of scale (Hawley, 1941; Berry and Garrison, 1958) and consequently larger proprietary incomes. Second, commerce is expanding at a faster rate in more central places (Clements, 1978; McGranahan, 1980), contributing to the greater profitability of their businesses. Finally, businesses such as banks, which develop branches, are more likely to have their central offices in places of commercial centrality and their branches in less central places.[2]

This argument does not apply to incomes in health, education, and related government activities, where proprietary incomes are generally not involved. Here, community income effects should be largely compositional, depending on the personnel requirements of hospitals, schools, and government bureaucracies. Although larger units may bring occupational differentiation, communities specializing in social services should have less income inequality, due to the absence of proprietary incomes.

The effects of industry on income distribution would appear to depend on the type of industry. According to the dual economy perspective (see Averitt, 1968; Beck et al., 1978), industry may be divided into a center or core sector—comprised of large, oligopolistic, unionized firms (in which wages are set internally), and a periphery sector—comprised of small, competitive, nonunionized firms (where wages are determined largely by local supply and demand). Thompson (1965), in an extensive discussion of urban income distribution, suggests that, at least where the labor market is tight, high-wage manufacturing not only affects incomes for those employed in these plants but also induces an area wage "roll out," which drives up wages in the rest of the local economy.[3] However, since people with high education are relatively mobile geographically, the area roll out may have less effect on their incomes than on others'. For this reason, and others suggested by Thompson (1965) (such as extensive absentee ownership in high-wage industry), towns and cities dominated by high-wage manufacturing should have higher, more equal income distribution.

This argument is more relevant to urban than rural areas, however. First, the distinction between center and periphery industry has a spatial dimension, with much of the industry in rural regions composed of small, low-wage firms and plants. Second, the wage roll-out effects may depend on a tight labor market, which is generally present only in areas proximate to growing metropolises. And, third, because of intercommunity commuting in rural regions, wage effects, if any, are likely to be diffused throughout an area. Hence, in many rural regions, there may be little effect of industry on wages, either directly or through wage roll outs. However, manufacturing communities may still have lower income inequality than other communities because much of the industry tends to be absentee owned and because of wage uniformity within plants.

---

[2] Thompson (1965) in his discussion of community income distribution also suggests that profits are constrained by market area. He sees size (centrality) as operating on income inequality by affecting the variation in proprietary incomes. Since even in small towns the vast majority of people work for others, the variation between proprietary and wage incomes would seem to be more important.

[3] Some indirect evidence of this is presented in Parcel (1979).

To summarize some of the basic elements of the model: metropolitan spread and backwash have historically resulted in growth near metropolitan centers and decline in peripheral areas. The incomes of those least mobile geographically—the older, less-educated members of the labor force—have been most affected. This has resulted in a tendency for distance from expanding metropolitan centers to be associated with lower incomes for the bottom end of the distribution, making average incomes lower and income distributions more unequal in peripheral places. At the same time, commercial centrality within an area permits higher proprietary incomes while having relatively little effect on wages and salaries. These increases to the top end of the income distribution tend to make incomes higher and more unequal in places of higher centrality.[4]

The following section, an application of the model to northwestern Wisconsin, serves to illustrate the model's relevance for one rural region. The region was not chosen to be representative of all rural regions. In fact, the relevance of the model should vary from region to region, depending on the size and vitality of the proximate metropolises, the natural resources of the region and the location of satellite centers and transportation routes. At the same time, the developmental history of the northwestern Wisconsin region is not unlike others in the Midwest, and there is little reason to expect locational influences on income distribution to be unique to the region.

INCOME DISTRIBUTION IN NORTHWESTERN WISCONSIN

The Region

The study region is a 17-county area comprising the northwest quarter of Wisconsin. It is bounded on the north by Lake Superior, on the west by Minnesota, and on the south by an interstate highway connecting Minneapolis-Saint Paul (lying just off the southwest corner) to Milwaukee and Chicago. The expansion of Minneapolis-Saint Paul (SMSA population, 1970: 1.8 million) has been the dominant force in the region, drawing off business from Superior-Duluth, a small declining metropolis, in the northwest corner of the region, and spreading out along the interstate highway, promoting the growth of the satellite center of Eau Claire-Chippewa Falls (population, 55,000). In accord with the spatial patterns of spread and backwash, three subregions have been delineated: The north, which historically has been an area of heavy outmigration;[5] the east, an intermediate

---

[4] The effects of metropolitan spread and commercial centrality on returns to age, education and proprietorship are not the only factors affecting community income levels and income inequality. Kuznets (1976) shows how demographic composition can have an important bearing on income distribution. One might expect poor areas to have a relatively high proportion of elderly people, which would result in lower incomes and higher inequality. Lincoln and Friedland (1978) propose that intracommunity differentiation both increases with community size and, due to intercommunity specialization, decreases with metropolitan accessibility, and that more differentiated communities should have greater income inequality. (See also Thompson, 1965; Betz, 1972). Thus, the pattern of community income inequality predicted here may result from demographic and occupational composition as well as from the differential effects of location on incomes.

It should also be noted that, although it is assumed here that community income characteristics depend on commercial centrality, metropolitan proximity, and other attributes, these relationships may not be strictly recursive. High incomes, for instance, may induce commercial development but inhibit industrial expansion over time. The major effects, however, would seem to be in the direction specified, across space if not across time.

[5] Superior-Duluth is an exception to the rule that metropolises grow and spread. Due to the decline in

Table 1. Demographic and Economic Indicators for Three Subregions of Northwestern Wisconsin and the Entire State*

	Population 1970 (1940=100)	Median Age 1970	Value Farm Products Per Farm 1974 ($1,000)	Average Weekly Manufacturing Wage 1974	Unemployment Rate 1974
North[a]	79	36.3	12	124	7.3
East	90	39.9	22	143	6.2
South	116	27.2	23	166	4.8
State	141	27.2	25	204	4.6

* These data were gathered at the county level. Because of the influences of the large cities on county statistics, Douglas County (containing Superior) and Eau Claire County have not been included. Sources include United States Census of Population and Agriculture and reports of the Wisconsin Department of Industry, Labor and Human Relations.

[a] North includes Ashland, Bayfield, Burnett, Douglas, Iron, Sawyer and Washburn counties; east, Barron, Clark, Price, Rusk, and Taylor counties; and south, Chippewa, Dunn, Eau Claire, Polk, and St. Croix counties.

area; and the south, which includes the counties lying along the interstate (Table 1). In all three regions, slightly over one-fifth of the workers are employed in manufacturing.

Individual Level Analysis

The first question to be addressed is whether the effects of metropolitan spread and backwash on income have been greater for older people and those with less education than for younger people and those with more education. Using survey data from the region,[6] Table 2 presents the results of regressing the log of personal income on years of schooling, age (in log terms because returns to experience decline with age), dummy variables representing north and east subregions, and interaction terms formed by multiplying education and age by the regional variables.

Note first that, controlling for age and education, there are considerable differences in average incomes among the subregions (Equation 2). The antilog equivalents of the coefficients for north (−.30) and east (−.23) are .74 and .79 and are the estimated ratios of incomes in these subregions to incomes in the south. However, people have not been affected equally. The interaction effects between education and subregion are significant, large, and positive (Equation 3), indicating that the percentage differences in income between those with high and low education are much greater in the north and east than in the south. Age shows the reverse pattern (Equation 4): incomes vary much more by age in the south than elsewhere. The inclusion of the two sets of interaction terms does not improve the prediction, however, since relatively high returns to education and low returns to age are interrelated phenomena (Equation 5).

The importance of these findings can best be seen by comparing the predicted incomes of people very likely to be affected by region—people aged 60 with eight years of education—with the incomes of people likely to be affected only minimally by region—people 30 years of age with sixteen years of education. The predicted incomes using Equation 5:

---

importance of lake transportation and the increase in the importance of Minneapolis-Saint Paul, this SMSA actually lost population from the 1920s until the mid-1970s, and it has generated no apparent spread effects in the northern section of the region.

[6] The data are from 1973 and 1974 surveys on personal well-being in the region, both of which drew samples from all 3 subregions. Together, 13 of the region's 17 counties were covered (2 of the 7 counties in the north and 1 of the 5 in each of the other 2 subregions were omitted). Because part- and full-time employment were not distinguished in the surveys, only employed men between the ages of 21 and 65, living outside of places of 10,000 or over, and with incomes of at least $2,000, are included here. Farmers are excluded because their incomes may be affected by a number of factors other than wage levels (although when they are included the results are strengthened). The (reduced) sample size is 345, of which 63 are self-employed.

Table 2. Personal Income in Northwest Wisconsin by Area, Education, Age and Interactions of Subregion with Education and Age for Men, Ages 21–26, with Nonfarm Employment (N=345)

	Regression Coefficients of Equations Predicting Log of Personal Income*				
	(1)	(2)	(3)	(4)	(5)
Education	.065	.060	.037	.057	.040
Age (Ln)	.226	.263	.276	.426	.396
North Subregion[1]		−.30	−.30	−.30	−.31
East Subregion[1]		−.23	−.20	−.24	−.23
Interaction Terms					
North × Education			.036		.025^{NS}
East × Education			.030^{+}		.036^{NS}
North × Age				−.377	−.305^{NS}
East × Age				−.211^{NS}	−.211^{NS}
R^2	.14	.20	.22	.22	.22
$\bar{R}^2$	.13	.19	.20	.20	.20

[1] Coefficients for subregion represent deviation from south, for people with average age (40) and education (12).

* = All coefficients significant at .05 level unless otherwise marked.

$^{+}$ = P < .10.

	Age: 60 *Education: 8*	Age: 30 *Education 16*
North	$ 6,900	$10,900
East	7,800	11,500
South	11,800	12,300

The subregional differences in estimated incomes for men aged 30 with sixteen years of education are quite small relative to those differences for men aged 60 with eight years of education; this is further reflected in relatively small differences between the age/education groups within the south—especially as compared with the north. Age and education have a correlation of −.33 in this sample. In the south, the effects of these two would appear to balance each other out, but this is far less true in the other two areas. There may be additional factors (such as age and education composition) contributing to greater inequality in the north than in the south, but these interaction effects are clearly important.

The second question to be dealt with at the individual level is the differential effect of commercial centrality on wage and proprietary incomes. Unfortunately, the only measure of centrality in these data is size of place of residence, which is coded as rural open country, village, or city (over 2,500 population). Whether or not a person said he was self-employed is used as the measure of source of income.

From the first equation in Table 3, it is evident that there is little variation in income by community size, at least for this rather crude size breakdown. This result is unexpected, given that larger places are more likely to have schools, hospitals, and larger businesses. While people who are self-employed do not generally have higher incomes (Equation 1), their incomes do appear to be much higher in places with over 2,500 residents, and the inclusion of the interaction terms adds considerably to the variance explained (Equation 2). The incomes of the self-employed are no less affected by area than are the incomes of wage and salary earners (Equation 3), although this may be less true for the self-employed who own large firms and/or who are highly educated.[7]

Community Level Analysis

Community level analysis is used, first, to see if incomes are lower and more unequally distributed in communities located

---

[7] While the interaction effects of subregion with age and education may in part reflect subregional differences in industrial structure, a division of the study sample into core and periphery sector employment (Beck, et al., 1978) showed no evidence of this. In all three subregions, 44% to 45% of the sample works in the core sector. While there is no variation within the region, this percentage in core sector employment is much lower than the 71% found by Beck et al. (1978) for males in their national sample.

Table 3. Personal Income in Northwest Wisconsin by Subregions, Size of Place of Residence, Self- or Wage-Employment, and Interactions of Self-Employment with Size of Place and Area for Men, Ages 21–65, Nonfarm Currently Employed

	Regression Coefficients of Equations Predicting Log of Personal Income ($1,000s)*		
	1	2	3
North Subregion	−.30	−.30	−.28
East Subregion	−.28	−.27	−.29
Residence Open Country	−.05^{NS}	.08^{NS}	−.06^{NS}
Residence Under 2,500 Population	−.02^{NS}	.11^{NS}	−.02^{NS}
Self-employed	−.09^{NS}	.71	−.06^{NS}
Interaction Terms			
Open country × self employment		−.88	
Under 2,500 × self employment		−.88	
North × self employment			−.12^{NS}
East × self employment			.03^{NS}
R^2	.10	.14	.10
$\bar{R}^2$	.08	.12	.08

* Significant at .05 level unless otherwise noted.

in the more peripheral subregions and, second, to explore further the relationships among commercial centrality, size of firm, proprietary incomes, and community income characteristics. For the 92 villages and cities in the region having from 200–7,500 residents in 1970, per capita incomes and Gini indices of inequality have been calculated from income distributions furnished by the Wisconsin Department of Revenue. These are based on 1973 personal taxable incomes, husbands and wives combined, net of business expenses. Because nontaxable transfer payments such as social security are exluded, the proportion of population over 65 is included (but not substantively interpreted) in the analyses reported in this paper.

The log of community commercial employment is used as the measure of commercial centrality. This measure, like employment in industry and social services (health, education, and government), is based on unemployment compensation tax returns filed by employers for the first quarter of 1975 (when public agencies were first required to file). For establishments employing over twenty people, the type of ownership—local or absentee—has been determined.[8] Finally, the ratio of employment to population has been calculated to reflect the extent to which a community is an employment—rather than a residential—center. Analysis of individual data showed that there is considerable commuting in the region, and that the self-employed and highly educated were most likely to live near where they worked. Residential communities, generally the smaller villages, can therefore be expected to have lower (and more equal) incomes than places with high employment-to-population ratios.

The results of regressions of these variables on per capita incomes and income inequality are presented in Table 4.[9] The results show, first, that communities in the northern subregion have the lowest and most unequal incomes and that those in the south have the highest and most equal incomes. (Analysis not shown here indicates, in addition, that the ratio of community to open country incomes follows the same pattern, being higher in the north and east than in the south).

---

[8] Overall, 40% of the employment is commercial, 27% industrial, and 33% social service. Employment in large (over twenty employees) commercial establishments comprises 30% of total commercial employment, of which 26% is in absentee-owned firms. For industry, over 90% of the employment is in large firms, and 62% of this employment is in absentee-owned firms. (Cooperatives have been included as absentee owners because profit income does not remain in the community but, presumably, is returned to farmers living in adjacent rural areas.

[9] Because of problems inherent in the use of ratio variables (Schuessler, 1974; Fuguitt and Lieberson, 1974), these models have been tested in a variety of forms. For instance, the sectoral employment measures were expressed as percentages of total employment in some regressions. Although these and other transformations altered the functional form of the model, the results were essentially the same. The models presented here were chosen because their parameters are easily interpretable and multicollinearity is minimized.

Second, the first pair of equations also shows per capita income and income inequality to be highly related to commercial centrality. However, to the extent that a community specializes in manufacturing or social services, incomes are lower and, in the case of industry, inequality is also lower. Communities that have a relatively high employment-to-population ratio—i.e., that are not "bedroom" communities—have higher and more unequal incomes.

The second pair of equations in Table 4 incorporates effects due to size of firms and absentee ownership. With both the total percentage employed in large (over twenty employees) firms and the percentage employed in large *and* absentee-owned firms in the analysis, the effects of the former are net of the effects of the latter. Thus, the coefficients for the percentage employed in large firms represent, in this context, the effects of having large local firms as opposed to small, primarily local firms. While the concentration of local ownership in large firms has little effect on per capita incomes, it is associated with higher inequality as, according to the rationale, the same proprietary incomes are going to fewer people. Commercial centrality is associated with large commercial firms (r = .67), and this explains part of the relationship between centrality and inequality. The results also indicate that, to the extent that ownership of industry and commerce is absentee, both incomes and inequality are lower because proprietary incomes are going out of the community.[10]

This analysis shows that proprietary incomes are an important component in the distribution of incomes in the region. It gives strong evidence that commercial centrality affects community level and inequality by permitting larger proprietary incomes at the top end of the income distribution. This evidence is reinforced by the absence of similar income effects for industry and social services.

DISCUSSION

This paper has explored the question of how economic opportunities may be structured spatially in rural regions. The model developed here appears to be useful for understanding income distribution in northwestern Wisconsin. The basic theme is that location, in itself, does not have a uniform effect but, rather, influences incomes in distinctly different ways—depending on age, education, and proprietary status. The incomes of the more geographically mobile, young, and highly educated appear to vary little across the region, while the incomes of older people with less schooling vary considerably, according to the pattern of metropolitan spread and backwash. Within subregions, wages and salaries appear to vary little by community, while proprietary incomes increase sharply with commercial centrality. These differences are at least contributing factors in the distribution of income within and among communities. Communities in more peripheral areas have lower per capita incomes and greater income inequality; commercial centrality is associated with higher and more unequally distributed incomes.

The relevance of this model for other regions remains to be tested. As suggested earlier, its relevance should vary, depending on the size and vitality of proximate metropolitan centers and on other regional characteristics. Recent industry and population growth (both nationally and in northwestern Wisconsin) indicates that the era of rural decline may be over. If these trends continue, the effects of metropolitan spread and backwash should

---

inequality are twenty percentage points lower. For these small communities, the current model is clearly more useful.

[10] Following Betz (1972) and Lincoln and Friedland (1978), who suggest that inequality is a reflection of differentiation, a Gibbs-Martin index of differentation $[1 - \Sigma X_i^2/(\Sigma X_i)^2]$ was calculated using eight employment sectors. This measure is related to population and employment, and to income and income inequality, but it adds nothing to the variance explained and, when substituted for the employment measures used here, the R^2 for both income and

Table 4. Regressions of Community Income and Income Inequality on Centrality Measures, Subregion, and Absentee and Cooperative Ownership (N=92)

	Equations							
	A1		A2		B1		B2	
	Per Capita Income		Gini Ratio		Per Capita Income		Gini Ratio	
	U[a]	S	U	S	U	S	U	S
North Subregion	−49.3	−.39*	28.0	.30*	−55.4	−.44*	27.9	.30*
East Subregion	−20.7	−20*	21.3	.27*	−26.2	−.25*	21.8	.28*
Commercial Employment (Ln)	24.7	.60*	14.4	.47*	30.4	.74*	11.2	.36*
Industrial Employment (Ln)	−7.1	−.25*	−5.8	−.27*	−4.1	−.14	−7.6	−.35*
Social Service Employment (Ln)	−8.2	−.30*	.9	.05	−10.8	−.40	2.0	.10
Total Employment/Population	11.0	.47*	5.6	.32*	8.9	.38*	3.1	.18
Percent Age Over 65	−24.2	−.23*	17.2	.22*	−18.3	−.18*	23.7	.30*
Percentage Industry in Large Firms					19.6	.16	32.6	.37*
Percentage Industry in Large Absentee-owned Firms					−32.6	−.26*	−28.8	−.31*
Percentage Commerce in Large Firms					14.1	.05	63.1	.31*
Percentage Commerce in Large Absentee-owned Firms					−135.0	−.34*	−74.0	−.25*
R^2		.50		.52		.58		.59
$\bar{R}^2$		.46		.48		.52		.54

[a] U means unstandardized regression coefficients, S means standardized.

* Coefficient greater than twice its "standard error."

eventually become less important.[11] However, other spatial shifts in the economy are occurring and new ones, induced by rising energy costs, for instance, may become pronounced. Again, the income effects should be greater for those least mobile geographically.

The model developed in this paper has implications for the delineation of interests in community growth and change, suggesting that these interests will vary according to education, age, and proprietorship. It is the people with commercial proprietary interests who provide the main force behind communities as "growth machines" (Molotch, 1976), for increased centrality raises their incomes.[12] In contrast, the incomes of people who are young, highly educated, and salaried are little affected by growth in either the community or the subregion. For these people, other concerns such as environmental quality may take precedence. The incomes of wage earners with low education are affected by conditions in the area, but local community centrality is not, in itself, particularly important. Although wage earners and proprietors may generally agree that growth should be promoted, their interests are not necessarily mutual with respect to the kind of growth that occurs. For instance, the development of subregional growth centers may be advantageous to all wage earners in an area, but to only the proprietors in the designated centers.

This paper has been concerned with what may be called the spatial dimension of stratification. While the particular model developed here has been tailored to fit rural regions—and it undoubtedly fits some regions better than others—this study as well as work by Chiswick (1974) and others at the national level suggests that a thorough understanding of the distribution of income in this country will require an understanding of the ways in which opportunities are structured spatially. This, in turn, should lead to a better understanding of community stratification and structure.

REFERENCES

Aigner, D. J., and A. J. Heins
1967 "On the determinants of income inequality." American Economics Review 57:175–84.

Al-Sammarie, Ahmad, and H. P. Miller
1967 "State differentials in income concentration." American Economic Review 57:59–72.

Averitt, Robert J.
1968 The Dual Economy. New York: W. W. Norton.

Beck, E. M., P. M. Horan, and C. M. Tolbert, II
1978 "Stratification in a dual economy: a sectoral model of earnings determination." American Sociological Review 43:704–20.

Berry, Brian J. L.
1967 Geography of Market Centers and Retail Distribution. Englewood Cliffs: Prentice-Hall.
1970 "The geography of the United States in the year 2000." Transactions of the Institute of British Geographers 51:21–53. Reprinted in John Friedmann and William Alonso (eds.) (1975), Regional Policy: Readings in Theory and Applications. Cambridge, Massachusetts: MIT Press.

Berry, Brian J. L., and William L. Garrison
1958 "A note on central place theory and the range of a good." Economic Geography 34:304–11.

Berry, Brian J. L., and Alan Pred
1961 Central Place Studies: A Bibliography of Theory and Applications. Philadelphia Regional Science Research Institute.

Betz, Michael D.
1972 "The city as a system generating income inequality." Social Forces 51:192–8.

Bibb, Robert, and W. H. Form
1977 "The effects of industrial, occupational and sex stratification on wages in blue-collar markets." Social Forces 55:974–96.

---

[11] Featherman and Hauser (1978) present income attainment equations for the United States, North and South, in 1962 and 1973. In 1962, returns to education for whites were higher in the South than in the North, and the effect of age on income much lower, controlling for a number of other factors. These results are consistent with the model developed here, with the northern section of the country historically an area of spread and the South, an area of economic backwash. However, by 1973, these and other North-South differences had largely disappeared, a change attributed to their now more similar industrial structures. According to the logic of the present paper, the process of economic expansion of the South has been a crucial factor in this change. The South is less the economically peripheral, backwash area that it was, and its expansion (relative to the North) has increased the opportunities for southerners with less education, raising their incomes.

[12] Note that this does not extend to owners of industrial property. To the extent that a firm's wages are determined locally and its sales are independent of local demand, economic growth may, through its impact on local wages, be inimical to industrial interests. Industry and commerce may, therefore, have quite different interests in the community.

Bogue, Donald J.
1949 The Structure of the Metropolitan Community. Ann Arbor: University of Michigan Press.
Chiswick, Barry R.
1974 Income Inequality: Regional Analysis within a Human Capital Framework. New York: Columbia University Press.
Christaller, Walter
1966 The Central Places of Southern Germany. Englewood Cliffs: Prentice-Hall.
Clements, Donald W.
1978 "Utility of linear models in retail geography." Economic Geography 54:17–25.
Curtis, Richard F., and Elton F. Jackson
1977 Inequality in American Communities. New York: Academic Press.
Duncan, Otis Dudley, and Albert J. Reiss, Jr.
1956 Social Characteristics of Urban and Rural Communities, 1950. New York: Wiley.
Farbman, Michael
1973 "Income concentration in the southern United States." Review of Economics and Statistics 55:333–40.
Featherman, David L., and R. M. Hauser
1978 Opportunity and Change. New York: Academic Press.
Frech, N. E., and L. S. Burns
1971 "Metropolitan interpersonal income inequality." Land Economics 47:102–4.
Friedmann, John, and J. Miller
1965 "The urban field." Journal of the American Institute of Planners 31:312–9.
Fuguitt, G. V., and S. Lieberson
1974 "Correlation of ratios or difference scores having common terms." In Herbert Costner (ed.), Sociological Methodology, 1973–1974. San Francisco: Jossey-Bass.
Gunther, William D., and C. M. Ellis
1977 "Income inequality in depressed areas: a principle component analysis." The Review of Regional Studies 5:42–51.
Hanushek, Eric A.
1973 "Regional differences in the structure of earnings." The Review of Economics and Statistics 55:204–13.
Hauser, Robert M., and David L. Featherman
1977 The Process of Stratification: Trends and Analyses. New York: Academic Press.
Hawley, Amos H.
1941 "An ecological study of human service institutions." American Sociological Review 6:629–39.
Hochbaum, G., J. Darley, E. Monachesi, and C. Bird
1955 "Socioeconomic variables in a large city." American Journal of Sociology 61:31–8.
Johanson, H., and G. V. Fuguitt
1979 "Population growth and retail decline—conflicting effects of urban accessibility in American villages." Rural Sociology 44:24–38.
Kuznets, Simon
1976 "Demographic aspects of the size distribution of income: an exploratory essay." Economic Development and Cultural Change 25:1–94.
Lamb, Richard
1975 Metropolitan Impacts on Rural America. University of Chicago, Department of Geography Research Paper No. 162.
Lane, Angela
1968 "Occupational mobility in six cities." American Sociological Review 33:740–9.
Lincoln, James R., and R. Friedland
1978 "Metropolitan accessibility and socioeconomic differentiation in nonmetropolitan areas." Social Forces 57:688–96.
McGranahan, David A.
1980 "Changing central place activities in northwestern Wisconsin." Rural Sociology. Spring.
Molotch, Harvey
1976 "The city as a growth machine: toward a political economy of place." American Journal of Sociology 82:309–32.
Mueller, Charles
1974 "City effects on socioeconomic achievements: the case of large cities." American Sociological Review 39:652–67.
Murray, Barbara B.
1969 "Metropolitan interpersonal income inequality." Land Economics 45:121–5.
Nicholls, William H.
1961 "Industrialization, factor markets and agricultural development." Journal of Political Economy 69:319–40.
Parcel, Toby L.
1979 "Race, regional labor markets and earnings." American Sociological Review 44:262–79.
Peet, Richard
1978 "Rural inequality and regional planning." Antipode 7 (December): 10–24.
Richardson, Harry W.
1976 "Growth pole spillovers: The dynamics of backwash and spread." Regional Studies 10:1–9.
Ritchey, P. Neal
1976 "Explanations of migration." Pp. 363–402, in A. Inkeles, J. Coleman and N. Smelser (eds.), Annual Review of Sociology. Palo Alto: Annual Reviews.
Sale, Tom S., III
1974 "Interstate analysis of the size distribution of income: 1950–1970." Southern Economic Journal 40:434–41.
Schuessler, Karl
1974 "Analysis of ratio variables: opportunities and pitfalls." American Journal of Sociology 80:379–96.
Stolzenberg, Ross M.
1978 "Bringing the boss back in." American Sociological Review 43:813–28.
Thompson, Wilbur R.
1965 A Preface to Urban Economies. Baltimore: Johns Hopkins Press.
U.S. Department of Commerce
1977 Social Indicators 1976. Washington, D.C.: U.S. Government Printing Office.
Wright, Erik Olin, and Luca Perrone
1977 "Marxist class categories and income inequality." American Sociological Review 42:32–55.

COMMENTS

COMPARING STRATIFICATION THEORIES*

(A COMMENT ON ROBINSON AND KELLEY, ASR, FEBRUARY, 1979)

In a recent article Robert Robinson and Jonathan Kelley (1979) attempt to integrate Marx's and Dahrendorf's theories of social stratification with the Blau-Duncan approach, through a quantitative examination of class in the United States and Great Britain. This transfusion of European stratification theory into the dominant SES tradition could be very important, both theoretically and empirically. It is doubly unfortunate, therefore, that Robinson and Kelley's work presents errors of both theory and method which lead them to underestimate the power of the European theories vis-à-vis the Duncan-Blau model, and, hence, understate the utility of the Marxian and Dahrendorfian approaches.

In the following sections we identify errors in three areas of their analysis: specification of the sample; operationalization of the independent variable (class); and operationalization of the major dependent variable (income). Based on a reanalysis of some of the data, we argue that Robinson and Kelley's procedures distort the class models which prevents a useful test of them. Their procedures also generate noncomparable data for the United States and Britain which casts doubt on the validity of their cross-national findings.

1. Sample

To compare three contrasting approaches to stratification in two different countries we need samples that represent the entire spectrum of classes in each society. For the Marxian model, the issue of representativeness is particularly acute because Marx discusses classes in capitalism in the context of polarization between the majority—the proletariat—and a numerically small group of capitalists who are disproportionately represented at the upper tail of the income distribution. Thus Marx's model focuses upon extremes of the income distribution.

Robinson and Kelley select a United States sample, however, which excludes unemployed workers, workers on strike, vacationing and ill workers, and part-time workers. This decision distorts their sample in two ways: First, the unemployed and underemployed occupy a critical position in Marx's analysis of wage-labor; excluding them eliminates an important element in this class model. Second, the exclusion of both part-time and unemployed workers lops the bottom off the income distribution, that is, it eliminates a large proportion of the poor from Robinson and Kelley's sample. Table 1 shows that part-time workers in Robinson and Kelley's survey have a much lower mean family income than full-time workers. This also holds for individual incomes of part-time versus full-time workers.

Adding the unemployed, strikers, etc., to part-time workers increases this divergence in income between those proletarians whom Robinson and Kelley include in their sample and those whom they exclude. In short, Robinson and Kelley propose to test Marx's class theory after first excluding poorer individuals from their sample. Since their major test is to regress income on class, this exclusion of lower-income workers from the sample is quite inappropriate, and biases the test against a Marxian model.

A second issue in sampling concerns the comparability of the United States and British samples. Robinson and Kelley include both full-time and part-time workers in the British sample, while allowing only full-time workers in the United States sample. Not only does this mean that these two samples are not equivalent, it will also, ceteris paribus, lead the Marxian model to perform better on the British data than on the United States data, due to Robinson and Kelley's artifact of attenuating the United States sample. These problems of cross-national noncomparability are compounded by other decisions Robinson and Kelley make, as we show later.

2. Independent Variables

Robinson and Kelley (1979:39) initially define the Marxian class of capitalists as those

---

* Direct all communications to: Paul Attewell; Institute for the Study of Social Change; University of California; Berkeley, CA 94700.

This research was supported, in part, by NIMH grant #5T32 MH 14640–03, and the Institute for the Study of Social Change. We would like to thank Carol Hatch for her helpful suggestions. The authors are solely responsible for the content of this comment.

Table 1. Groups Included in and Excluded from Robinson and Kelly's United States Sample

Group	N	Mean Family Income	S.D.	T Value	Degrees of Freedom	2-Tail Significance
1976						
A. Full-Time Workers	591	16,076.98	8271.68	5.40	708	<0.001
B. Part-Time Workers	119	11,630.52	7815.52			
A. Full-Time Workers	591	16,076.98	8271.68	7.06	795	<0.001
B. Other Proletarians	206	11,468.44	7472.06			
1974						
A. Full-Time Workers	591	14,703.89	7713.47	4.17	691	<0.001
B. Part-Time Workers	102	11,279.41	7283.36			
A. Full-Time Workers	591	14,703.89	7713.47	5.40	787	<0.001
B. Other Proletarians	198	11,282.82	7721.07			

Note: In each case, group A was included in Robinson and Kelley's sample while group B was excluded. "Other Proletarians" includes workers on strike, or laid off, or looking for a job, part-time workers, ill workers, workers on vacation, and unemployed workers, but does not include students or homemakers or retirees.

Source: National Opinion Research Center, General Social Survey. See NORC (1972).

who own the means of production and who purchase the labor-power of others. They add:

> . . . we will show that capitalists have high incomes *not only* because they own the means of production but also because they exercise authority. The true effects of owning the means of production can only be discerned by controlling (in the usual statistical sense) for authority by making ownership one variable and authority a second variable and using standard multivariate techniques for separating their effects. (Robinson and Kelley, 1979:41)

While theoretically sound, this passage is curious because the NORC data which Robinson and Kelley use for their United States sample contain no questions about ownership of the means of production or about the purchase of others' labor-power. The 1973, 1974, and 1976 NORC surveys contain information only on supervision. Supervision is not synonymous with either ownership of the means of production or employment of others, yet Robinson and Kelley proceed to define Marx's capitalist class in terms of supervision. Having argued for the theoretical separation of authority from ownership, Robinson and Kelley adopt procedures that obfuscate this separation.

In their operationalization of Marx's capitalist class, Robinson and Kelley shift from the notion of ownership of the means of production to control of the means of production. They argue that some managers control the means of production and are therefore capitalists. Robinson and Kelley then generalize that anyone who supervises others and is not him/herself directly responsible to a supervisor is ipso facto a capitalist. This final operationalization is not in any sense equivalent to Marx's concept of the capitalist as owner of the means of production.

For example, if we apply Robinson and Kelley's definition of capitalists (persons who are not themselves directly responsible to a supervisor, but who do supervise others) to equivalent 1977 NORC data, we find that 44% of Robinson and Kelley's "capitalists" are employees, rather than self-employed.

Secondly, Robinson and Kelley's definition of the capitalist class (N = 161) categorizes individuals from an extraordinary assortment of occupations as capitalists. Of Robinson and Kelley's "capitalists" 18.6% are in the census category of professional, technical and kindred workers. These include 7 lawyers, 4 registered nurses, 2 painter/sculptors, and various others. An additional 11.8% of "capitalists" were craftsmen and kindred workers, including 3 carpenters, 2 foremen, 2 automobile mechanics, etc. Eight clerical workers, including 3 secretaries, 2 clerical supervisors, a keypunch operator and others constitute another 5% of the capitalists. Twelve sales workers, including 7 insurance agents or brokers and various salesmen/sales clerks, account for 7.4% of the capitalist class. Service workers including 3

Table 2. Percentage Self-Employed by Class (Full-Time Workers Only)

Class Position*		
Bourgeois	Petit Bourgeois	Proletarian
55.9%	38.3%	2.0%
(68)	(47)	(638)

Source: NORC General Social Survey, 1977. See NORC (1972).

* As defined by Robinson and Kelley (1979:41, 43).

cooks, 2 janitors, and a nurse's aide, represent 4.4% of the capitalist class, and so on.

In short, over 50% of Robinson and Kelley's "capitalists" worked in census occupations other than management, and (in the 1977 NORC survey) 44% of the "capitalists" were wage-laborers employed by others. This does not prevent Robinson and Kelley, who lack any United States data describing ownership of the means of production (e.g., stock ownership) or data describing employment of others' labor, from declaring that these individuals are capitalists, solely on the basis of their responses regarding direct supervision.[1]

Their operationalization of the petite bourgeoisie is even stranger. Marx used the term petite bourgeoisie to refer to individuals who were self-employed and who lived primarily from their own labor. Typical petit bourgeois occupations are lawyers, doctors, and shopkeepers. Robinson and Kelley (1979:41) operationalize this as persons who are not directly responsible to a supervisor and who do not directly supervise others. Applying Robinson and Kelley's definition of the "petite bourgeoisie" to 1977 NORC data, we find that 61.7% of their petite bourgeoisie are *not* self-employed, despite the fact that self-employment is central to Marx's definition of the petite bourgeoisie. These are the kinds of nonsensical results obtained if Marx's notion of ownership of the means of production is operationalized in terms of one ambiguous survey question concerning direct supervision.

Having constructed their version of Marx's class model, Robinson and Kelley proceed to contrast the class structure of the United States and Britain. They assure their readers that for the British data: "Control over the means of production and authority are operationalized in a manner identical to our classification in the U.S. except that only a dichotomous version of authority is available" (Robinson and Kelley, 1979:51).

Despite this statement, Robinson and Kelley use different operationalizations of Marxian classes for the United States and British data. In the United States data, a capitalist is someone who is not directly responsible to a supervisor but who does supervise other persons. A capitalist from the British survey, however, is someone who is self-employed and employs other persons. These two are not equivalent; as we have seen, there is only a 56% overlap between self-employment and Robinson and Kelley's United States "capitalists." Many managers, foremen, and supervisors in the United States sample are categorized as capitalists, yet persons in these same professions are categorized as *workers* in the British sample unless they were also self-employed and employers of others.

Since the British operationalizations allow for a much closer approximation[2] of Marx's class categories than do the United States operationalizations, it is not surprising that the Marxian class model is empirically stronger for the British data. This cross-national artifact becomes even worse when we consider the major dependent variable, income.

3. The Dependent Variable

A major part of Robinson and Kelley's article is devoted to a comparison of the three stratification theories to account for the observed variance in personal income. We have no objections in principle to this approach, but we differ with Robinson and Kelley's measure of income. Marx argued that proletarians have as their sole source of income the sale of their own labor power; capitalists, however, do not depend solely on their own work because they also appropriate the fruits of others' labor. Robinson and Kelley, though, choose a measure of income for the United States which includes only income from full-time employment. Hence they exclude from their United States analysis the various kinds of non-job-related *capitalist* income—from stocks, bonds, rents, etc. Capitalists and workers are compared solely on their income from full-time jobs. We believe this procedure bears little relation to Marx's theoretical framework, and is bound to weaken the predictive strength of his model.

The extention of this income analysis to the British data is again marred by a lack of comparability. The British survey that Robinson and Kelley used asked for the amount of personal income, with no qualification that such income had to be job-related. One may therefore assume that the various sources of capitalist income were included in addition to salaries and wages.

---

[1] This same survey question distorts Robinson and Kelley's treatment of Dahrendorf. Dahrendorf's concept of an "obey" class makes no distinction between those directly responsible to a supervisor and those only indirectly responsible. Yet for Robinson and Kelley the former become "obey class" members, while the latter are candidates for the "command class." Dahrendorf simply distinguishes between those who wield authority and those who lack it; whether one is directly or indirectly responsible to a supervisor is of no consequence.

[2] Robinson and Kelley do not allow for the existence of capitalists who do not personally employ or supervise workers, but who are capitalists by dint of stock ownership. This important sector of the capitalist class falls through Robinson and Kelley's operationalizations and are lost to their analyses.

Thus at three levels—the sample, the operationalization of class, and the operationalization of income—the United States and British data are quite different. In each case the British operationalizations are theoretically much closer to those required by the Marxian model. Not unexpectedly, then, the Marxian model performs quite well for the British income data, while in its very different United States operationalization it performs less favorably. Robinson and Kelley (1979:51), however, reify this artifact of their data manipulations, and conclude that "Britain is more of a classical capitalist society than the U.S."

In conclusion, while we endorse Robinson and Kelley's appeal for a theoretical and empirical comparison of Marxian, Dahrendorfian, and SES approaches to stratification, we sincerely hope that any such attempt is performed with more theoretical clarity than is demonstrated in Robinson and Kelley's "first step." Given the theoretical and methodological distortions in their analyses, it is doubtful whether their findings should be given any credibility.

Paul Attewell
Robert Fitzgerald
University of California, Berkeley

REFERENCES

National Opinion Research Center (NORC)
1972– Codebook for the Spring 1972 (1973, 1974,
1977 1976, 1977) General Social Survey. Chicago: National Opinion Research Center.

Robinson, Robert V. and Jonathan Kelley
1979 "Class as conceived by Marx and Dahrendorf: effects on income inequality, class consciousness, and class conflict in the United States and Great Britain." American Sociological Review 44:38–58.

SYNTHESIS AND COMPARISON OF STRATIFICATION THEORIES: A REPLY*

In their comment on our article (Robinson and Kelley, 1979) Attewell and Fitzgerald argue that, by defining and operationalizing the Marxian class model as we do, we "underestimate the power of the European theories vis-à-vis the Blau-Duncan model" in the United States data and, thus, bias our comparison with the British data. They do not, however, show that our results are, in fact, distorted. In this reply, we defend our theoretical position and offer a reanalysis which shows that the Marxian model has not been slighted by our definitions. In the course of the discussion, we also reply to their objections to the specification of our sample and our measurement of income.

DEFINITION AND OPERATIONALIZATION OF CONTROL OF THE MEANS OF PRODUCTION

Attewell and Fitzgerald claim that we initially (Robinson and Kelley, 1979:39) defined the Marxian capitalist class as those who both own the means of production and purchase labor power, but later (p. 41) shifted to a definition of the first criterion in terms of "control of the means of production." In fact, from the beginning, our definition is in terms of control. Immediately following our first mention of the Marxian model (p. 39), we state:

> In a modern economy, Marx's criterion of ownership of the means of production may reasonably be expanded to include all forms of control of the means of production, whether they stem from legal ownership or formal control. Thus we include in the capitalist class managing directors who control but do not own the means of production since in practice they have effective control over their firms, can use them to further their own interests, and often own some stock in them. . . .For terminological simplicity we will refer to both owners and managing directors as "controlling the means of production."

Our conceptualization of control encompasses the owner's legal control and the managing director's de facto control. In operationalizing this we employed the following question: "Do you have a supervisor on your job to whom you are directly responsible?" We reasoned that individuals who have no supervisor, or boss, control the means of production, while those who do have a supervisor, or boss, do not. We cautioned (Robinson and Kelley, 1979: fn. 15) that our operationalization was preliminary and that "a small proportion of persons who responded that they have no supervisor may not actually control the means of production but may instead simply be autonomous workers."

Employing this definition and operationalization was preliminary and that "a small proclass as those who control the means of production and exercise authority (that is, have employees or subordinates) and the petite bourgeoisie as those who control the means of production but do not exercise authority

---

* Direct all communications to Robert V. Robinson; Department of Sociology; Indiana University; Bloomington, IN 47405.

We thank Wendell Bell, Nancy Davis, Larry Griffin, Bernice Pescosolido, Patricia Koos, and J. L. P. Thompson for their comments. Sole responsibility for any errors rests with us.

Table 1. Standardized Regression Coefficients for Models Predicting Personal Job Earnings, for Various Samples of U. S. Men, 1977

	Full-Time Men Only		Full- and Part-Time Men[a]	
Variables	Control (1)	Ownership (2)	Control (3)	Ownership (4)
Marx: Control (cols. 1 and 3) or Ownership (cols. 2 and 4)	.147*	.117*	.155*	.113*
Dahrendorf: Authority	.184	.210*	.217*	.237*
Father's education	−.048	.003	−.056	−.043
Father's occupational status	.004	.011	.033	.021
Education	.044	.044	.058	.058
Occupational status	.311*	.302*	.277*	.270*
Percentage of variance explained, R^2	25.1%	24.4%	25.0%	24.0%
Number of cases	297	297	332	332

[a] Includes full-time and part-time persons; and persons on strike, on vacation, and temporarily ill.
* Regression coefficient is significant at $p < .05$.

(Robinson and Kelley, 1979:41:Table 1). Attewell and Fitzgerald object that both classes include a sizable proportion of persons who are not self-employed—that is, who are not owners of a business. Yet this can hardly be surprising since the concept was intended to encompass those who had de facto control as well as those who owned firms.

One might ask whether we have, in fact, underestimated the importance of Marx's model by including in the capitalist class those who control—but do not own—the means of production. A reanalysis suggests that the answer is no, and that, if anything, our operationalization shows a somewhat greater effect of Marx's model than obtains if ownership is used, instead. The 1977 NORC General Social Survey data (the same data employed by Attewell and Fitzgerald) include a question on self-employment[1] as well as the questions from which our operationalizations of control and authority were derived. These data permit two alternative operationalizations of class in Marx's sense—one based on ownership (operationalized by self-employment as suggested by Attewell and Fitzgerald) and the other on control (as defined in our article). Regressions for U.S. men[2] are shown in Table 1, columns 1 and 2. A comparison of column 1, Table 1, presented here, to Table 3 in our article (Robinson and Kelley, 1979:48), shows that the 1977 data, despite the small number of cases (N = 296 full-time men) compared to our analysis based on the combined NORC 1973, 1974 and 1976 GSS, are generally similar although the effect of education is smaller and that of occupational status is larger. This is apparently due to the greater instability of coefficients in small samples.

Since Attewell and Fitzgerald direct their critique at our comparison of stratification theories, we present standardized coefficients, which show the relative effects of Marx's, Dahrendorf's, and the status models. A comparison of the coefficients in columns 1 and 2 suggests that our operationalization of Marxian class as "control" (.147) has, if anything, a slightly greater impact on men's earnings than does Attewell and Fitzgerald's operationalization as "ownership" (.117). To the extent that Attewell and Fitzgerald's preference for ownership is to be accepted, we have slightly *over*-estimated the importance of Marx's model by our extension.

OCCUPATIONAL COMPOSITION OF THE CAPITALIST AND PETIT BOURGEOIS CLASSES

Attewell and Fitzgerald claim that, because we defined capitalists in terms of control rather than ownership (operationalized as self-employment), our "capitalist class categorizes individuals from an extraordinary assortment of occupations as capitalists." Again, this is not surprising. We (Robinson and Kelley, 1979:42) advanced such a distinction between class and occupation on theoretical grounds:

> Although it is often assumed that occupational standing and class are virtually identical, in fact there is little overlap between them, as both Marx (1893:862–3) and Dahrendorf (1959:138–40) stress. Occupations may be performed in a variety of class contexts. Lawyers, plumbers, or cooks, to name only a few examples, may work on their own account and hire assistants (and so be capitalists in

---

[1] Data on self-employment were unavailable in the 1973, 1974 and 1976 NORC General Social Surveys that we analyzed.

[2] Time and space limitations preclude a reanalysis of women. Our original United States-Britain comparison was made on the basis of samples of men in the two countries.

> the command class), or may work for a large company without any subordinates under them (and so be workers in the obey class), or may work for a large company and have subordinates (and so be workers in the command class), or may work alone on their own account (and so be petit bourgeois and classless).

In Table 2 of our article we (Robinson and Kelley, 1979:46) showed that class and occupation are, in fact, "empirically as well as conceptually distinct." Attewell and Fitzgerald object to our including in the capitalist class such occupations as lawyer, registered nurse, painter, or carpenter. Apparently they have missed our point about occupation and class—that they are virtually unrelated. We also reject Attewell and Fitzgerald's claim that "typical petit bourgeois occupations are lawyers, doctors, and shopkeepers," as another example of their confusion of class and occupation.

It is perhaps necessary to emphasize that the empirical distinctiveness of class and occupation does not arise from our operationalization of control. As Table 2 shows, the distribution of occupations within a capitalist class operationalized as those who are self-employed with employees, as suggested by Attewell and Fitzgerald (see also Wright and Perrone, 1977), includes as wide a range of occupations—for example, lawyer, pharmacist, clergyman, funeral director, sales manager, radio and TV repairman, as obtains when control is used in operationalizing class.[3] Moreover, operationalizing class as ownership, 50% of capitalists fall into occupations other than management, a figure virtually identical to what they find using our measure of control. Furthermore, Wright and Perrone (1977:36) and Kalleberg and Griffin (1980) have also demonstrated the distinctiveness of occupation and ownership (as defined by self-employment) and the first author of this reply has supported this point on data from seven societies (Robinson, 1979:chap. 3).

Underlying Attewell and Fitzgerald's critique is, we believe, a serious misunderstanding of class as Marx conceived it. Their reasoning suggests that they do not view occupations (such as lawyer, plumber, painter, or mechanic), which are not directly engaged in the production of commodities, as belonging to the capitalist class. Yet Marx did not distinguish between productive and nonproductive capitalists or workers in this way:

> The only worker who is productive is one who produces surplus-value for the capitalist, or in other words contributes towards the self-valorization of capital. If we may take an example from outside the sphere of material production, a schoolmaster is a productive worker when, in addition to belabouring the heads of his pupils, he works himself into the ground to enrich the owner of the school. That the latter has laid out his capital in a teaching factory, instead of a sausage factory, makes no difference to the relation. (Marx, 1867, as quoted by Wright, 1976:14)

If individuals produce a service and then sell it at a profit, they are no less capitalists than if they were producers of some material commodity such as automobiles.

Table 2. List of Occupations Comprising the Capitalist Class as Defined by Self-Employment with Employees. Full-Time U. S. Men and Women Only, 1977

Occupation	Percentage of Capitalist Class
Lawyer	2.4%
Dentist	2.4
Pharmacist	4.8
Clergyman	7.1
Funeral director	2.4
Restaurant, cafeteria, and bar manager	2.4
Sales manager and department head, retail trade	2.4
Managers and administrators, not elsewhere classified	42.9
Real estate agent and broker	4.8
Salesman and sales clerks, not elsewhere classified	4.8
Bookkeeper	2.4
Radio and TV repairman	2.4
Painter, construction, and maintenance	2.4
Truck driver	2.4
Freight and material handler	2.4
Farmer (owners and tenants)	7.1
Hairdresser and cosmetologist	4.8
Total	99.5%
Number of cases	(42)

RESTRICTION OF THE SAMPLE

Attewell and Fitzgerald argue that, because we excluded persons who worked part time, or who were on strike, on vacation, ill, or unemployed, we underestimated the effect of the Marxian model since these excluded persons have lower incomes than those we included in the analysis. Although we would have liked to include the unemployed, we had stronger reasons for excluding them. Since our overriding purpose was to synthesize stratification dimensions, a reasonable way of classifying the unemployed had to be found for Dahrendorf's model and for the Blau-Duncan status model. In Dahrendorf's scheme, we thought that such individuals might be included in the classless group since they are not currently part of the industrial authority hierarchy. In the status model, however, Duncan (1961) did not provide an SEI score for unemployed persons, nor did Featherman et al.'s (1975) update which we used in coding occupational status. We rejected the possibility of assigning unemployed persons the status of their usual occupation because it would not reflect their current disadvantaged circumstances. Thus, there was no way of classifying the unemployed in terms of status and, consequently, such individuals had to be excluded. Moreover, while we do not wish to understate the severity of unemployment, the unemployed constitute a relatively small proportion of the total labor force (roughly 6% at the time our data were collected). Thus, we suspect that this omission does not bias the analysis to the extent that Attewell and Fitzgerald claim.

---

[3] Since Attewell and Fitzgerald include both male and female capitalists in their remarks, we have analyzed both of these in Table 2.

Although we were unsuccessful in solving the problem of how to treat the unemployed, Attewell and Fitzgerald provide no solution to this either. They mention no way of handling the unemployed in either the Dahrendorf or status model. Regarding Marx's model, they adopt procedures which muddle an important conceptual distinction in Marx's work. In Attewell and Fitzgerald's Table 1, unemployed persons are grouped with part-time, striking, vacationing, and ill persons and labelled "other proletarians." Yet Marx did not classify the unemployed with part-time workers nor did he classify them with the worker class, as Attewell and Fitzgerald's discussion implies they should have been. Rather, he treated them as a separate class—the *lumpenproletariat,* which he referred to as "refuse of all classes" (Marx 1852:480). An approach which is more consistent with Marx's class analysis would treat the unemployed (but not part-time workers) as a fourth class. Even this solution, however, leaves unsolved the problem of how to treat the unemployed in the Dahrendorf and status models.

Aside from the problems with the unemployed, we agree that, generally, it is best to impose as few restrictions on the sample as possible in conducting an analysis. The selection of a sample must, however, be determined by the purposes of the analysis. Because we (Robinson and Kelley, 1979:fn. 14) wished to compare men and women in the United States data, and because a disproportionate number of women worked only part time, we decided to analyze full-time workers only, though we agree that, generally, part-time and other workers should be included in analyses. In making their arguments, however, Attewell and Fitzgerald overstate the case that the Marxian model would be slighted by our exclusion of these persons. There is just as much reason to believe that members of Dahrendorf's obey class (who exercise no authority) will be underemployed. Consequently, some of the effect of excluding these people should accrue to Dahrendorf's model.

In fact, as a comparison of columns 3 and 4 of Table 1 with columns 1 and 2 shows, adding these excluded persons (but not the unemployed) does not substantially affect the coefficients for the Marxian model (defined as either ownership or control), but it does increase the impact of the Dahrendorf model slightly and decrease the influence of occupational status somewhat. We conclude that the Marxian model is not slighted by our having restricted the sample, and none of our substantive conclusions is altered by this reanalysis.

MEASUREMENT OF INCOME

Attewell and Fitzgerald claim that, by employing a measure of job earnings in the U.S. data, we have biased the results against the Marxian model. They argue that a substantial part of capitalists' income derives from nonjob earnings such as stocks, bonds, rents, etcetera, and that we have underestimated the impact of Marxian class by ignoring such income. We agree that use of such a variable would have been preferable but, unfortunately, it did not exist. Attewell and Fitzgerald fail to mention, however, that in addition to capitalists' income, several sources of workers' income—welfare payments, unemployment compensation, disability payments, and earnings from second jobs—were also excluded. Thus, to a certain extent the excluded sources of income for workers may have counterbalanced those excluded for capitalists.

In fact, this appears to be the case. It is possible to ascertain the extent to which Attewell and Fitzgerald's objection has practical consequences by analyzing the 1977 NORC data. These data include the *personal*-earnings variable, which we employed, as well as a *family*-income variable which asks about income from all sources: "In which of these groups did your total *family* income, from *all* sources, fall last year before taxes, that is?" (italics in original). By analyzing only those men who reported that they were the sole earner in their family, it is possible to employ the family-income variable as a measure of personal income from all sources. This income measure should be slightly inflated because it will include some nonjob income of spouses and children, but this will work in Attewell and

Fitzgerald's favor if their argument is correct. That is, the difference between personal job earnings and personal income from all sources will be even greater because some income from other family members is included. Because of the limited nature of this sample (only about 40% of the men analyzed in columns 3 and 4 of Table 1 fall into this sample) it is inappropriate to compare results from it with those in Table 1. However, by comparing regressions on earnings with those on income *within* this sample, we can ascertain the extent to which the results using income might have differed from those using earnings. Before proceeding to such an analysis, we note that the correlation between job earnings and income (from all sources for men working full or part time, or on strike, on vacation, or ill) is fully .931. Moreover, comparing the mean annual job earnings ($13,891) with the mean annual income from all sources ($14,678) shows that we had excluded only 5.4% of personal income by using a job earnings measure. In Table 3 we present the results of a regression analysis of earnings and income. In the models employing control (columns 1 and 2), though the Marxian model increases slightly and the Dahrendorf model decreases slightly, when income is considered, other variables (education and father's SES) show even larger changes in effect. In the models employing ownership (columns 3 and 4), the Marxian model is more effective on income than on earnings, although the difference in the coefficients for the two models (.039) is far from remarkable—especially in samples of this size (N = 142). Dahrendorf's model decreases slightly (.027), while other variables (education and father's education) experience larger shifts. We conclude that, although there is some truth to the argument that income better captures economic differences between classes, the improvement is far from dramatic and none of our substantive conclusions is altered.

UNITED STATES-BRITAIN COMPARISON

Attewell and Fitzgerald conclude (on the basis of their objections previously discussed) that we have biased the comparison with Britain in such a way that Marxian class appears to have a greater effect in Britain than in the United States. Specifically, they argue that (1) the United States sample did not include part-time persons while the British sample did; (2) the United States class variable was operationalized using an inferior measure of class as control; and (3) the United States income variable excluded nonjob earnings. We will respond to each point in turn.

(1) We included part-time men in the British data because we had no variable which allowed us to exclude them in order to make the British data strictly comparable to the United States data. Since only 9% of United States workers work part time we did not think that this should seriously affect the analysis. As our reanalysis of the United States data has shown (compare columns 3 and 4 of Table 1 with columns 1 and 2), the effect of Marx's model is not increased when part-time men are added. Consequently, the exclusion of part-time men cannot explain the differences we found.

(2) The British data did not allow a perfect replication of our operationalization of control in the United States data and we were forced to operationalize class as ownership in these data. However, as we showed above, ownership is not a superior operationalization of Marxian class; thus, if anything, differences between the countries would have been slightly larger

Table 3. Standardized Regression Coefficients for Models Predicting Personal Job Earnings and Personal Income from All Sources, for U.S. Men Who Are the Sole Earners in Their Family[a]

	Models for Control		Models for Ownership	
Variables	Earnings (1)	Income (2)	Earnings (3)	Income (4)
Marx: Control (cols. 1 and 2) or Ownership (cols. 3 and 4)	.138*	.153*	.115	.154*
Dahrendorf: Authority	.199*	.178*	.223*	.196*
Father's education	.084	.054	.080	.048
Father's occupational status	.039	.034	.023	.015
Education	.072	.146*	.091	.171*
Occupational status	.193*	.197*	.173*	.173*
Percentage of variance explained R^2	20.6%	23.6%	20.3%	23.8%
Number of cases	142	142	142	142

[a] Includes full-time and part-time persons; and persons on strike, on vacation, and temporarily ill.

* Regression coefficient is significant at $p < .05$.

had we used ownership in the United States data.

(3) It is true that the United States earnings measure did not capture income from nonjob sources. In the United States data, however, we have shown that there is little real difference between the results on earnings and those on income. Differences between the United States and Britain (of the magnitude we found) would not disappear when income is used instead of earnings.

To conclude this point, our finding (Robinson and Kelley, 1979:51) that Britain was a more classical capitalist society rested on the fact that "control of the means of production is more important than authority in Britain," meaning that the classical Marxian model in terms of relations of production is more important than Dahrendorf's more recent model in terms of authority. The standardized coefficients reported in Table 3 of our article (p. 48) show that Marx's model (.225) was relatively more important than Dahrendorf's (.133) in Britain, but that Dahrendorf's model (.218) was relatively more important than Marx's (.150) in the United States. In none of the reanalyses of United States data in Tables 1 and 3 does the dominance of Dahrendorf's model over Marx's disappear. Since the British results are unchallenged by Attewell and Fitzgerald, we see no reason to abandon our original conclusion that Marxian class is relatively more important in Britain than in the United States.

CONCLUSION

Attewell and Fitzgerald have raised some interesting conceptual issues which have enabled us to offer further support for our conclusions and for the synthesis of stratification dimensions which we proposed. Reanalyses—using an alternative operationalization of the Marxian model, different samples, and a different economic measure—resulted in only minor discrepancies from our reported results and altered none of our conclusions. On some points, however—particularly the inclusion of part-time workers and the use of income from all sources—we agree with our critics and urge that future research take these considerations into account.

In closing, we would emphasize that, as we clearly stated throughout the article, we were attempting a *merger* of the class and status traditions. To a considerable degree, Attewell and Fitzgerald have misinterpreted us as having been more concerned with a *comparison* of these. Our primary intention was not so much to claim to establish the superiority of one of these theories over the others as it was to show that much is to be gained by integrating all three. From this point of view it is noticeable that Attewell and Fitzgerald fail to discuss the implications of their objections in practice for the Blau-Duncan status model and refer to the Dahrendorf model only in a footnote.[4] Instead, they continually object that the Marxian model in particular has been slighted. We have shown that their objections on this point are not as substantial as they imagine. We believe that more might have been gained by solving the unresolved questions that arise from attempts at synthesis such as our article provides.

Robert V. Robinson
Indiana University
Jonathan Kelley
The Australian National University

REFERENCES

Duncan, Otis Dudley
1961 "A socioeconomic index for all occupations." Pp. 109–38 in A. J. Reiss, Jr., O. D. Duncan, P. K. Hatt, and C. C. North (eds.), Occupations and Social Status. Glencoe: Free Press.

Featherman, David L., M. Sobel, and D. Dickens
1975 "A manual for coding occupations and industries into detailed 1970 categories." Center for Demography and Ecology, University of Wisconsin, Madison.

Kalleberg, Arne, and Larry J. Griffin
1980 "Class, occupation, and inequality in job rewards." American Journal of Sociology. In Press.

---

[4] It is revealing that in their one reference to Dahrendorf's model they are confused with regard to the question we employed in operationalizing this. They remark that:

> This same survey question distorts Robinson and Kelley's treatment of Dahrendorf. Dahrendorf's concept of an obey class makes no distinction between those directly responsible to a supervisor and those only indirectly responsible. Yet for Robinson and Kelley the former become "obey class" members while the latter are candidates for the "command class."

In fact, we distinguished between the command and obey classes not on the basis of responsibility to a supervisor but on the basis of an entirely different question:

"In your job, do you supervise anyone who is directly responsible to you?" (pp. 44, 46:Table 2).

This confusion on their part may be the basis of their objection that "having argued for the theoretical separation of authority from ownership, Robinson and Kelley adopt procedures which obfuscate this separation."

Marx, Karl
1852 "The eighteenth brumaire of Louis Bonaparte." Pp. 436–525 in R. Tucker (ed.), The Marx-Engels Reader. New York:Norton.
1867 Capital: A Critique of Political Economy, Vol. 1. Chicago: Kerr.
Robinson, Robert V.
1979 Ownership, Authority, and Occupational Prestige: A Synthesis and Cross-National Comparison. Ph.D. dissertation, Department of Sociology, Yale University.
Robinson, Robert V., and Jonathan Kelley
1979 "Class as conceived by Marx and Dahrendorf: effects on income inequality and politics in the United States and Great Britain." American Sociological Review 44:38–58.
Wright, Erik Olin
1976 "Class boundaries in advanced capitalist societies." New Left Review 98:3–41.
Wright, Erik Olin, and Luca Perrone
1977 "Marxist class categories and income inequality." American Sociological Review 42:32–55.

EMPIRICAL VERIFICATION AND BLACK'S *THE BEHAVIOR OF LAW*[*]

(COMMENT ON GOTTFREDSON AND HINDELANG, ASR, FEBRUARY, 1979)

Donald Black's (1976) *The Behavior of Law* was a major contribution to sociology because of the integrative way it brought together propositions about stratification, morphology, culture, organization, and social control to explain variations across time and space in the quantity of law. At last the sociology of law had a genuine theory which could be a focus for formerly directionless empirical work. The strength of *The Behavior of Law* lay in the fact that because it was in explicit propositional form, it was eminently testable.

Sadly, however, in his recent contribution to *ASR*, Black (1979), out of his own mouth, compromised the value of his theory. Incidentally, in his effort to defend *The Behavior of Law* against a nonsupportive empirical test by Gottfredson and Hindelang (1979a), Black has also neutralized the considerable worth of some of his earlier work (Black, 1970; 1971). This comment is, inter alia, a plea for sociologists to continue to see value in Black's work by ignoring his comment in *ASR*.

Gottfredson and Hindelang (1979a) have used victim survey data to test whether, in accord with Black's theory, the wealthy, the employed, the married, are more likely to report to the police events regarded as crimes than are various other groups. The test seemed reasonable as Black states clearly that "a complaint to a legal official, for example, is more law than no complaint" (Black, 1976:3). However, in his *ASR* comment, Black rejects Gottfredson and Hindelang's use of victim survey data because such surveys require subjects to report only incidents they consider to be "crimes." Hence, the data show only whether people called the police when they considered an incident a crime, not whether some incidents were more or less likely to be interpreted as crimes in the first place. While victim survey data certainly ignore the crucial first-stage labelling process, either the second-stage decision to call the police, given the labelling of the event as a crime, must be relevant to the theory or the theory must become less general than we thought it to be. Gottfredson and Hindelang were able to show that Black did intend the theory to have this level of generality; that he did intend the theory to make predictions about the quantity of law conditional upon the incident having reached some prior stage of processing; and that he did intend victim survey data to be relevant to testing the theory (since Black himself cites such data in support of it).

Black's reversal to the position that his theory cannot explain increments in the quantity of law from one stage of criminal processing to another both limits its generality and renders it untestable in any practical sense. Black (1979) suggests that a relevant test will gather "data based on direct and unobtrusive observation" to ascertain whether any particular type of incident will be more often dealt with legally by one group than another. Black does not address himself to the problem of which types of incidents will be selected for "direct and unobtrusive observation," and how such selection might fundamentally affect conclusions drawn.

In practical terms, we can never get into the offices, the bedrooms, and the boardrooms in order to observe an unbiased sample of the quantity of law arising from "the universe of incidents people experience in their everyday lives" (Black, 1979:20). Through his *ASR* comment Black has transformed what was a testable general theory about increments in the quantity of law into an untestable collection of ideas.

Gottfredson and Hindelang's examination of reporting of crimes to the police uncovered little support for Black's theory as originally stated. What they did find was that the "seriousness" of the offense, as measured by the Sellin-Wolfgang (1964) index was strongly predictive of the quantity of law. Black (1979) correctly retorted that perceived seriousness is a matter of value, not fact; a response to crime, not a characteristic of it. Gottfredson and Hindelang have therefore simply explained one response to crime with another. Black makes much of this. He likens it to explaining the response to an idea with its "truth." Black seems to be suggesting that there is something inherently unsound about this. If one were to say that Donald Black never wrote a book called *The Behavior of Law*, sociologists would reject this assertion because it is not true. In this case, explaining the rejection of the idea with its lack of truth would not only be sound, but it would also be the best explanation possible. The problem with Gottfredson and Hindelang's explanation is not that it is unsound, but that it is banal.

---

[*] Direct all communications to: John Braithwaite; Australian Institute of Criminology: Canberra; P.O. Box 28; Woden, A.C.T. 2606; Australia.

It is hardly telling for Gottfredson and Hindelang to denigrate the predictive power of Black's theory by suggesting that the gravity of the infraction against legal norms is a better predictor than Black's stratification, morphology, culture, organization, and social control. Imagine a critique of a theory about what training method is best for weightlifters which points out that how heavy the weights are is a better predictor than are training methods used.

Black's more general point is that Gottfredson and Hindelang's preoccupation with seriousness is evidence that the sociology of law has yet to escape the common sense of the people it studies. This point is well taken. However, the blanket rejection by Black of the use of the seriousness concept, to the point of repudiating his own studies on the severity of sentencing controlling for seriousness (Black, 1970; 1971), goes too far. In the weightlifting analogy above, while it is banal to use weight as an *explanation*, it would be negligent not to use weight as a *control*. Similarly, a study of sentencing which fails to control for the perceived seriousness of the offense is methodologically unsound since the purpose of such work is to determine whether incidents which are perceived as equally serious when decontextualized are dealt with as incidents of unequal seriousness when placed in certain social contexts.

Black persuasively hammers home the seriousness question because it is the weakest part of Gottfredson and Hindelang's (1979a) analysis. However, Gottfredson and Hindelang (1979b:4) correctly point out that even if seriousness variations were deemed irrelevant to Black's propositions, then the validity of the propositions should be assessed by collapsing across the rows (representing different seriousness levels) in their tables. When this is done, the fact remains that the data provide little support for the propositions.

Taking Black's theory in its "proximally conditional" form, data from the Australian crime victims survey will now be used to confront the same propositions selected by Gottfredson and Hindelang for test on American data. The importance of testing the theory of law in a variety of cultures hardly needs to be argued. The methodology of the survey has been described in detail elsewhere (Australian Bureau of Statistics, 1979; Braithwaite and Biles, in press), and is not substantially different from that of the American surveys.

The data have not been disaggregated by seriousness, but have been disaggregated by offense in tables which are not presented in the text. In an attempt to strengthen the generalizability of Gottfredson and Hindelang's empirical test, this replication is on a wider selection of offense types—motor vehicle theft, robbery with violence, theft, break and enter, rape and attempted rape, assault, and nuisance telephone calls, as well as fraud, forgery, and false pretenses.

The first proposition tested by Gottfredson and Hindelang was that the quantity of law varies directly with social rank. Their data, contrary to the theory, show no relationship between family income and reporting to the police. Table 1, part A, shows that in the Australian data there is also no association between household income and reporting to the police. This remains true for seven of the eight offense types when totals are disaggregated by offense. The notable exception is the category of fraud, forgery, and false pretenses which shows a markedly higher rate of reporting to the police among high-income victims.

The second key proposition addressed by Gottfredson and Hindelang is that law is inactive among intimates and is used increasingly as relational distance increases. Black has predicted that since, up to a point, relational distance will increase with city size, the quantity of law will be positively associated with city size. Gottfredson and Hindelang found that, if anything, the reverse was slightly the case. From Table 1, part B, it can be seen that, in Australia also, the data do not support the prediction that reporting to the police will be greater in larger cities. Assault was the only individual offense type which showed a trend in support of the prediction. Assault was twice as likely to be reported to the police in cities over a million than in cities and towns under 50,000.

A more direct way of tapping the relational distance aspect of morphology than that tested

Table 1. Percentage of Victimizations Reported to the Police, by Selected Predictors

A. Weekly Household Income

<$80	$80–139	$140–199	$200–259	$260–339	>$340
34%	31%	43%	38%	34%	41%

B. City Size

500–50,000	50,000–100,000	100,000–500,000	500,000–1,000,000	>1,000,000
33%	43%	41%	38%	37%

C. Residential Mobility

Low	Medium	High
37%	36%	39%

D. Victim's Knowledge of Offenders

Knew at least one offender	All strangers
26%	42%

E. Relationship between Victim and Offender (Where offender known)

Family member	Other relative	Close friend	Speaking acquaintance	Knew by sight
20%	50%	23%	26%	34%

F. Marital Status

Never married	Now married	Widowed	Separated or divorced
34%	36%	38%	44%

G. Employment Status

Not in the work force	Unemployed	Part-time	Full-time
28%	30%	35%	42%

H. Education Level

Still at school	Never attended or left school under 15	Left school over 15	Trade or technical	Tertiary
31%	21%	38%	39%	41%

I. Number of Offenders Involved in Incident

1 only	2	3, 4 or 5	6 or more
26%	45%	52%	70%

J. Time of Day at Which Incident Occurred

6 a.m.–Noon	Noon–6 p.m.	Unspecified daytime	6 p.m.–Midnight	Midnight–6 a.m.	Unspecified nighttime
35%	34%	34%	38%	58%	39%

K. Location of Incident

Inside home	Near home	At work	Inside public areas	Outside public areas	In a motor vehicle	Other
35%	30%	55%	40%	41%	49%	56%

by Gottfredson and Hindelang is through the residential mobility of the respondent in the years preceding the interview. Respondents who have been geographically mobile should be at a greater relational distance from those around them than people who have lived in the same spot all of their lives. Table 1, part C, shows no relationship between an Australian Bureau of Statistics' residential mobility index and reporting to the police. Nor was there a tendency for high residential mobility to be associated with high reporting to the police on any of the eight individual offenses.

Relational distance is clearly greater in the situation in which offender and victim are strangers than in one in which they are acquainted. While Gottfredson and Hindelang showed only a moderate tendency for offenses to be more likely to be reported when the offender was a stranger, this tendency was quite strong in the Australian data (Table 1, part D). Moreover, the Australian data clearly suggest (Table 1, part E) that where the offender is more intimately known to the victim (family member, close friend) the offense is less likely to be reported than where the offender is less intimately known (speaking acquaintance, knew only by sight).

The next proposition tested is that law varies directly with integration into the center of social life. Black specifically proposes that married and employed people are more integrated than unmarried and unemployed people. Gottfredson and Hindelang found support for the prediction that married people would be more likely to report offenses, but they found no support for higher reporting by those employed compared with those unemployed. The support for Black's theory on the marital status question was not replicated on the Australian data. Apart from a tendency for separated or divorced respondents to report more victimizations to the police, there was little variation according to marital status (Table 1, part F). The one exception by offense was with assault. Respondents who had never been married reported assault victimizations to the police at only half the rate of both those who were married and those who were separated or divorced.

On the employment variable, the Black prediction is supported by the Australian data. People working part time were more likely to report offenses than the unemployed, and people working full time more likely to do so than both people in part-time employment and the unemployed (Table 1, part G). In the present study, persons keeping house were classified as "not in the work force," for the purposes of testing Black's prediction about morphology, whereas Gottfredson and Hindelang classified these people as "employed." Clearly, if persons keeping house had been classified as employed in the present study, the trend supportive of Black would have been greatly diminished.

Black's proposition that law varies directly with culture was operationalized through the indicator of culture suggested by Black: level of education. Gottfredson and Hindelang found a weak positive association between education and reporting to the police. This trend receives strong confirmation in the Australian data (Table 1, part H), and there were no individual offense types which provided significant exceptions to the aggregate trend.

Black hypothesized that "law is greater in a direction toward less organization." In operational terms, Black suggested that more organized offenders (a group of offenders) are more likely to have the law used against them than less organized offenders (a lone offender). Moreover, more organized victims are more likely to use the law than less organized victims. Data are not available from the Australian victims survey on number of victims. However, data are available on the number of offenders. Contrary to Black's prediction, the Australian data show a strong tendency (consistent across offense types) for recourse to the criminal justice system to increase as the number of offenders increases (Table 1, part I). This contrasts with only a very slight tendency in the same direction in Gottfredson and Hindelang's data.

The theory of law asserts that, in settings which permit people to continuously observe and react to each other's conduct, law is less important as a mechanism of social control. "When people go to sleep, for instance, most social control relaxes as well and law increases" (Black, 1976:110). Consistent with this prediction, for both the American and Australian data there were clear tendencies for reporting to the police to be greater for offenses occurring between midnight and 6 A.M. (Table 1, part J).

More crucially, Black maintains that in private settings more nonlegal social control exists, so less law is required than in public settings. Gottfredson and Hindelang produced the surprising finding that their most public setting ("on the street") evidenced a lower rate of reporting to the police than their most private setting ("own home"). Table 1, part K, shows that in the Australian survey the expected finding—that "inside home" and "near home" had lower reporting rates than "outside public areas"—applied. However, "at work"—a more private setting than "outside public areas"—showed the highest reporting rate of all settings.

In summary, the data from the Australian national crime victims survey are almost as discouraging as data from the United States survey in the way in which they refute most of the predictions which have been derived from Black's *The Behavior of Law*. These tests—even though they are on more massive data sets than would be available for any alternative type of verification—are only very partial tests of a theory meant to apply to many other domains. The theory is relevant to the social construction of the event as a crime which occurs prior to a decision about reporting to the police, to a number of stages of processing through the criminal justice system which are subsequent to reporting to the police, and to an infinitely wider range of justiciable types of conduct than the eight which are the subject of this paper and the four which are the subject of Gottfredson and Hindelang's paper. It is a pity that Black has so counterproductively erected the barricades at the first discovery of disconfirmatory data. It may be that his theory can accommodate them.

John Braithwaite
David Biles
Australian Institute of Criminology

REFERENCES

Australian Bureau of Statistics
1979 1975 General Social Survey: Crime Victims. Canberra: Australian Bureau of Statistics Ref. No. 4105.0.

Black, Donald
1970 "Production of crime rates." American Sociological Review 35:733–48.

1971 "The social organization of arrest." Stanford Law Review 23:1087–111.
1976 The Behavior of Law. New York: Academic Press.
1979 "Common sense in the sociology of law." American Sociological Review 44:18–27.

Braithwaite, John, and David Biles
In press "Overview of findings from the first Australian National Crime Victims Survey." Australian and New Zealand Journal of Criminology.

Gottfredson, Michael R., and Michael J. Hindelang
1979a "A study of 'The Behavior of Law.'" American Sociological Review 44:3–18.
1979b "Theory and research in the sociology of law." American Sociological Review 44:27–36.

Sellin, Thorsten, and Marvin E. Wolfgang
1964 The Measurement of Delinquency. New York: Wiley.

TRITE BUT TRUE*

(RESPONSE TO BRAITHWAITE AND BILES)

Braithwaite and Biles support our critique of Black's (1979) response to our test (Gottfredson and Hindelang, 1979a) of his theory found in *The Behavior of Law* (1976). They find value in, and essentially replicate, our empirical work.[1] They also see the difficulty in reconciling Black's original presentation of his theory with his subsequent defense of it, and they agree with our argument that to accept Black's "explication" is to accept the conclusion that his theory is not falsifiable.[2] For all these reasons we quite naturally agree with the bulk of Braithwaite and Biles's comment on our paper.

With respect to our use of the concept, "seriousness," however, Braithwaite and Biles restate the position adopted by Black in his comment on our paper. This position, central to the labelling approach to law, is that legal reactions can be explained without reference to individual conduct. Black (1976; 1979) adopts this labelling premise, and when we invoke a measure of conduct and its consequences (the Sellin-Wolfgang [1964] seriousness scale) as a rival hypothesis he counters with the argument that we have explained one response to crime with another (Black, 1979:25). Braithwaite and Biles repeat this assertion, telling us that the seriousness measure we used is "a response to crime, not a characteristic of it" (1980:335).

This interpretation of our use of seriousness has little connection with what we did. However, a theory that denies the relevance of conduct, but which is confronted with evidence of its superior predictive power relative to propositions derived from the theory, is left with few alternative strategies other than to define the rival hypothesis as synonymous with the criterion.[3] Thus, Black (1979) and now Braithwaite and Biles (1980:335) argue that seriousness cannot be used to explain calls to the police because, in effect, seriousness is the same thing as calls to the police (and therefore is a "banal" explanation).[4]

---

* Direct all correspondence to: Michael R. Gottfredson; School of Criminal Justice; State University of New York; 135 Western Avenue; Albany, NY 12222.

[1] We are, however, prohibited from appreciating the full significance of their data for Black's theory because they are not fully displayed. It is usually standard practice to indicate the numbers of cases upon which percentages are based when offering data as a test of ideas. In the absence of these numbers, and in the absence of a measure of association, we are not able to judge the relevance of some of the tables they present to the theory. (The total sample may be sufficiently large for some of the tables presented, but we are, quite frankly, unsure about those which rely on small parts of their data—e.g., Table 1, part "E".)

[2] Braithwaite and Biles (1980) suggest that Black's (1976) theory, when viewed in its "proximally conditional" form (Gottfredson and Hindelang, 1979b), is of research interest and therefore sociologists should ignore Black's (1979) discussion of his theory in relation to our work. Although we agree with their premise, it is difficult to abide by their conclusion. After all, the theorist must be the ultimate arbiter of "what the theory means" in such jurisdictional disputes. Thus, we are forced to conclude that the inconsistencies (apparent to us and to Braithwaite and Biles) between Black's presentation and defense of his theory are resolved by Black's (1979) response. If his response is accepted, then, for the reasons stated in Gottfredson and Hindelang (1979b), Black's collection of ideas is not falsifiable; in any event, it can hardly be ignored, as Braithwaite and Biles suggest (1980:334).

[3] A priori, it would have seemed that this strategy would be more persuasive when the theory being tested also is predictive of the criterion. In such cases the field must choose between empirically established, but theoretically incompatible, predictors. But apparently this strategy is also useful in cases—such as this—in which one of the competitors has virtually no demonstrated explanatory power with respect to the criterion (i.e., Black's [1976] theory, in relation to calling the police).

The labelling perspective has other strategies that are useful for coping with the problem that conduct is a major correlate of legal reaction. Some of these strategies—such as invoking the claim that even a trivial amount of explained variance is of theoretical interest—have been discussed in the literature (e.g., Tittle, 1975).

[4] Braithwaite and Biles confuse us with their attempt to mediate the dispute between ourselves and

But, as we pointed out when Black (1979) attempted this definitional substitution, we did not use seriousness in the way suggested. The measure we used, the Sellin-Wolfgang scale, was not based on judgments of the respondents in our surveys about the victimization they experienced. If the survey victims had evaluated the seriousness of their own victimizations, there would be *some* truth to the statement (Black, 1979; Braithwaite and Biles, 1980:335) that we "explained one response to crime (the victim's decision to call the police) with another (the *victim's* seriousness estimate). But we did not use a measure like that. The one we used has two components.

The first component relates to the objective characteristics of the crime: how much injury was inflicted, how much financial loss was involved, whether a weapon was used, etc. This component distinguishes incidents that differ with respect to conduct. As we have shown elsewhere (Hindelang and Gottfredson, 1976), by themselves these differences predict calls to the police. That is, even divorced from their association with value judgments, these differences—such as whether or not a gun was present, whether or not injury occurred, or whether or not there was a loss of property in the victimization—are associated with reports to the police. Black (1979), and now Braithwaite and Biles (1980), refer to these differences in conduct as "a matter of value, not fact." In our view, whatever label one chooses to pin on these objective differences in criminal events, the empirical finding that conduct differences predict calls to the police poses a fundamental challenge to all theories (*The Behavior of Law* included) that claim such differences are irrelevant to understanding legal reactions. *That* is why they are important empirical findings, albeit "obvious" ones.

Furthermore, the argument that these offender conduct differences are synonymous with legal reactions (e.g., calling the police, arrest and sentencing) cannot be consistent logically with the theoretical position that they are also uncorrelated with legal reactions. For example, Black's (1976:31) theoretical contention that his principles "... apply whatever the actual behavior of the lower ranks . . . since their conduct is more likely to be defined as illegal no matter what they do," is logically incompatible with his argument that offender conduct differences are just another way of conceiving of the criterion. In the former case we expect no correlation between legal reaction and conduct, in the latter a substantial correlation.

The second component of the Sellin-Wolfgang scale, the scale we used to measure seriousness, is a series of weights applied to these conduct differences. The origin of these weights, and the general intergroup consistency with respect to rank ordering that has been found, are well known (see Gottfredson and Hindelang, 1979a for citations).

Once it is admitted that conduct is a competitor to theories of legal reaction, the question arises as to how it is to be measured and tested against the criterion. As we pointed out (1979b) there are far too many combinations of conduct differences in the data we used, to control them all simultaneously. The weights provided in the Sellin-Wolfgang scores provided a solution to this problem. They permit the scaling of some of these differences along a single dimension—consensually defined seriousness. Thus, from an empirical point of view, these scale values organize the data. From a theoretical point of view, we argued that the gravity of the infraction against legal norms should condition the victim's belief that the conduct warranted the invocation of state social control systems.[5]

The findings that seriousness predicts calls to the police and other legal reactions are central data for the sociology of law. They are also

---

Black, when they state that, whereas seriousness should not be used as an explanatory variable, it should be used as a control variable—otherwise the study will be "methodologically unsound." Why a variable that is not explanatory—in some sense—should be controlled in these types of studies is quite unclear to us. (The answer, it seems, is that when a variable is obvious—i.e., "what everyone knows"—sophisticated sociological *theory* should ignore it, but *researchers* testing these theories better not.) It is also a mystery to us why Braithwaite and Biles would argue that seriousness should be controlled in studies of legal reaction when they themselves define it earlier as a "response to crime, not a characteristic of it." If "seriousness" and "legal reaction" are in some sense synonymous, as Braithwaite and Biles appear to argue, then their suggestion that it would be "negligent" not to control for seriousness is a suggestion that in tabular analyses we restrict the range on our criterion variable.

[5] Contrary to Black (1979:4)—and now Braithwaite and Biles (1980:335)—we define, measure, and use the concept "seriousness" apart from the dependent variable. *They* are thus in the position of arguing that since seriousness is the same thing as the criterion it must be perfectly related to it. We see it as an explanatory concept, but not as a sufficient cause, and thus can tolerate the lack of perfect association. In fact, our data indicate there is much left to be explained after seriousness is taken into account, a finding that constitutes a major challenge for the sociology of law.

"obvious," "banal," and "commonsensical." Denying the relevance of the obvious may be fashionable among some sociologists, but it is not necessarily the best way to construct useful theory. Denying the relevance of conduct to the explanation of legal reactions has led to the construction of a body of theories that do not work very well to predict the phenomena of interest. It is our guess that such poor prediction will continue to be the case in this field, so long as some sociologists choose to claim that behavioral differences cannot be measured and that "what everybody knows" necessarily lacks the profundity to rise to the level of science.

Michael R. Gottfredson
Michael J. Hindelang
State University of New York, Albany

REFERENCES

Black, Donald
1976 The Behavior of Law. New York: Academic Press.
1979 "Common sense in the sociology of law." American Sociological Review. 44:18–27.
Braithwaite, John and David Biles
1980 "Empirical verification and Black's 'The Behavior of Law.'" American Sociological Review, 45:334–8.
Gottfredson, Michael R. and Michael J. Hindelang
1979a "A study of 'The Behavior of Law.'" American Sociological Review 44:3–18.
1979b "Theory and research in the sociology of law." American Sociological Review 44:27-36.
Hindelang, Michael J. and Michael R. Gottfredson
1976 "The victim's decision not to invoke the criminal justice process" in W. McDonald (ed.), The Victim and the Criminal Justice System. Beverly Hills: Sage.
Sellin, Thorsten and Marvin Wolfgang
1964 The Measurement of Delinquency. New York: Wiley.
Tittle, Charles
1975 "Labelling and crime: an empirical evaluation" in W. Gove (ed.), The Labelling of Deviance. New York: Halsted Press.

IS THE STATUS STRUCTURE IN THE UNITED STATES REALLY MORE FLUID FOR WOMEN THAN FOR MEN?*

(COMMENT ON GLENN, ROSS, AND TULLY, ASR, OCTOBER, 1974)

Three mid-1970s articles in the *American Sociological Review* reported research in which men's intergenerational mobility was measured in the usual way (by comparing the occupational prestige of fathers and sons) and married women's mobility was measured by comparing the occupational prestige of their fathers and husbands (Tyree and Treas, 1974; Glenn et al., 1974; Chase, 1975). All three studies found greater mobility among the women than among the men. Stated differently, status of origin correlated more highly with status of destination among the men (Glenn et al., 1974: Table 3)[1]—an indication of greater intergenerational transmission of status among the men, at least along the dimensions of status taken into account by the studies.

A statement (written by the first author—who is also the first author of this comment) in the conclusions section of the Glenn et al. (1974) article asserts that, if there are not substantial errors in the data reported in the article, "the status structure in the United States is substantially more fluid than the male data alone would indicate. . . ." This conclusion has been widely quoted and paraphrased in textbooks and other publications. However, the data reported by Glenn et al. (1974) (and in the other publications cited above) do not adequately support that conclusion. As Glenn et al. (1974) point out earlier in their article, women may experience intergenerational mobility in two important ways: through marriage, and through their own achievements (or lack of such).[2] The latter kind of mobility is in no way measured by the three studies. Furthermore, mobility studies which use only the occupational prestige of the family head as the indicator of the status of the family of orientation and the family of procreation fail to deal with several important dimensions of status, the most important of which are perhaps the economic dimensions. Therefore, the conclusion quoted here, for which Glenn is responsible, is premature. The status structure may well be

---

* Direct all communications to: Norval D. Glenn; Department of Sociology; University of Texas at Austin; Austin, TX 78712.

The data reported in this paper are from the 1973 through 1977 General Social Surveys conducted by the National Opinion Research Center with grants from the National Science Foundation. The authors are solely responsible for the tabulations, analyses, and interpretations presented here.

[1] Of course, the correlation of origins with destinations is not in all cases an accurate indicator of the relative amounts of mobility experienced by two compared populations, but it is accurate if the origins and destinations distributions are the same for the two populations and is fairly accurate if the distributions are similar.

[2] Of course, men may also experience mobility through both of these means, but marriage has traditionally been a less important means of mobility for men than for women.

Table 1. Median Family Income, by Sex and Level of Origin, Married White Persons Ages 25–54, Combined Data from United States National Surveys, 1973–1977

Level of Origin (Father's Occupational Prestige)*	Median Family Income				
	Males	(N)	Females	(N)	Difference
0–19	$12,500	(76)	12,000	(108)	500
20–29	15,278	(146)	14,353	(179)	925
30–39	15,617	(275)	14,677	(341)	940
40–49	14,449	(416)	15,100	(454)	−651
50–59	16,500	(155)	17,721	(201)	−1,221
60 and up	16,250	(62)	22,692	(82)	−6,442

* Hodge-Rossi-Siegel Occupational Prestige Scale.

Source: The 1973–1977 General Social Surveys (James A. Davis, principal investigator).

less fluid for women than for men when mobility through personal achievement and mobility through marriage are both taken into account for both sexes and when dimensions of status not accurately measured by the occupational prestige of the male family head are considered.

In this comment, we report evidence which very strongly suggests, although it does not absolutely prove, that among whites in the United States, there has been greater intergenerational transmission of economic status among women than among men. The evidence is less than absolutely conclusive, because intergenerational economic mobility cannot be directly measured by use of the kind of retrospective survey data which has been used for occupational mobility studies.[3] It is unlikely that many adults are able to report even roughly accurate information on the income and other dimensions of the economic status of their families of orientation when they were children and adolescents. Even if they could do so, income data for different years are incomparable, and thus it would be very difficult to do a meaningful income mobility study with an age heterogeneous sample.

However, it is possible to make a reasonable inference about the rank order of men and women in amount of intergenerational economic mobility without measuring the mobility for either sex. This is possible because the males and females in each birth cohort whose fathers had the same occupations, or had the same level of occupational prestige, must have had, as aggregates, families of orientation which had very nearly the same distributions on economic status. Therefore, if the patterns of intergenerational economic mobility of males and females have been the same, or nearly so, males and females in the same birth cohorts whose fathers had about the same occupational prestige should have, as adults, about the same distributions on economic status. Furthermore, the correlation of father's occupational prestige with measures of current family economic status should be about the same for both sexes, and the regression of the latter variable on the former should not vary by sex.

We have investigated the relationship between father's occupational prestige, as indicated by the Hodge-Rossi-Siegel Occupational Prestige Scores (Siegel, 1971), and reported family income by use of data from the 1973 though 1977 General Social Surveys conducted by the National Opinion Research Center (Davis, 1978).[4] In Tables 1 and 2, here, we report aspects of the relationship, by sex, for married whites ages 25–54.[5] The data are restricted to married persons, since "family income" has a different meaning for married and unmarried persons, being the same as personal income for most unmarried persons. However, since one might suspect that differential selection of males and females into (and retention in) marriage might produce differences in the male and female data, we have conducted supplementary tabulations and analyses including persons of all marital statuses.[6] The resulting

---

[3] Very few studies of intergenerational economic mobility have been attempted. (For the results of one attempt, see Huber and Form, 1973:Appendix 2).

[4] The cumulative GSS file provides data gathered by the 1972 and 1978 surveys, which we did not use because the 1972 income data are coded differently and because we had reservations about pooling income data gathered over a period of six years.

[5] We excluded nonwhites as there are reasons to suspect that sex differences in patterns of intergenerational mobility may differ between blacks and whites and as an insufficient number of black respondents precluded a separate study of blacks.

[6] It is important that the distributions of father's occupational prestige were very similar for the males and females. For instance, among married persons, the means were 39.7 and 39.6 for males and females, respectively, and the standard deviations were 12.2 and 12.6. The distributions of family income were also very similar (see the further discussion in this text).

Table 2. Relationship of Father's Occupational Prestige* (X) to Family Income (Y), by Sex, Married White Persons Ages 25–54, United States, 1973–1977

	Product Moment Correlation					Regression of Y on X		
Data Set	Males	(N)	Females	(N)	Significance of Male/Female Difference**	Males	Females	Significance of Male/Female Difference**
1973	.120	(191)	.342	(244)	.06	64.34	196.15	.05
1974	.130	(180)	.323	(248)	.41	79.42	149.72	.37
1975	.166	(211)	.304	(249)	.23	97.00	181.22	.20
1976	.142	(185)	.164	(223)	.87	93.10	99.51	.94
1977	.058	(184)	.169	(208)	.40	40.96	101.41	.48
1973–1977 Combined	.113	(951)	.246	(1,172)	.02***	70.11	159.60	.02****

* Hodge-Rossi-Siegel Occupational Prestige Scale.

** Two-tailed.

*** The two-tailed probability computed making use of information from all five independently drawn samples is .00013.

**** The two-tailed probability computed making use of information from all five independently drawn samples is .00011.

Source: The 1973–1977 General Social Surveys (James A. Davis, principal investigator).

data are not reported in tabular form but are summarized briefly below.

FINDINGS

The data in Table 1 show a male/female difference in the family incomes of white persons whose fathers had similar occupational prestige. Among persons whose fathers had low occupational prestige, the women had the lower median family incomes; among persons whose fathers had high occupational prestige, the women had the higher median family incomes. In other words, it appears that females who had origins both high and low in the economic hierarchy were more likely to remain near their level of origin than were the males of the same origins. High-status families, as a whole, were apparently better able to confer high economic standing on their daughters than on their sons, and low-status families apparently tended to confer a greater economic handicap on their daughters than on their sons.

From the data in Table 1 alone, one cannot be confident that the observed male/female differences did not result from sampling variability. However, the correlation and regression data in Table 2, to which we apply significance tests, corroborate the differences shown in Table 1. The data from each General Social Survey conducted from 1973 to 1977 show a stronger relationship for women than for men and, thus, indicate greater intergenerational transmission of status for the women. The data from the combined 1973–1977 file show the regression of family income on father's occupational prestige to be more than twice as great for females as for males and show the percentage of variance explained to be almost five times as great for females (6.1) as for males (1.3).

By making use of the fact that the direction of the differences is the same in the data from each of the five independently drawn samples, we were able to compute a joint two-tailed probability (significance level) for the male/female differences in the correlation and regression coefficients.[7] The probabilities, reported in the notes to Table 2, are both lower than .0002. Therefore, one can be highly confident that the relationship was stronger for women than for men in the population.

The analyses conducted with respondents of all marital statuses yielded results very similar to those shown in Tables 1 and 2 and, thus, it does not seem likely that sex differences in the mobility-proneness of persons selected into marriage account for the observed male/female differences. For instance, in the data for persons of all marital statuses, the difference in median family income of persons from the lowest and the highest levels of origin is $9,138 for females but only $3,339 for males. The correlation of father's occupational prestige with reported family income in the data from the combined 1973–1977 file is .092 for males and .205 for females, and the corresponding regression coefficients are 58.07 and 130.85.

---

[7] The joint probability is computed by multiplying across the one-tailed probabilities obtained for the separate surveys and doubling the product to obtain the two-tailed significance level. To illustrate, the probability that two independently drawn samples would both show differences in the same direction significant at the .05 level or lower (one-tailed), if there were no difference in the population, is $.05^2 \times 2 = .005$. By the same logic, the probability that five independently drawn samples would all show like-signed differences significant at the .05 level is $.05^5 \times 2 = .0000006$.

We multiplied all standard errors by 1.3 as an adjustment for the cluster design of the samples (see Kish, 1965).

Another possible problem grows out of the fact that family income is normally higher for middle-aged persons than for young adults and, thus, for our purposes is not comparable for persons of different ages.[8] Although this study is restricted to persons ages 25–54, one might suspect that age differences account, to some degree, for the male/female differences in Tables 1 and 2. However, the age distributions of the male and female respondents were quite similar, the means for the married persons being 38.4 and 37.8 years respectively for the males and females and the standard deviations being 8.7 and 8.6 years. Furthermore, holding age constant produced partial correlations between father's occupational prestige and family income of .132 for males and .256 for females—obviously very similar to the zero-order correlations.

Still another reason for reservations about the validity of the male/female comparisons is a possible sex difference in the accuracy of reports of income. Several studies (e.g., Ballweg, 1969; Ferber, 1955) have indicated that there may be such a difference and that the reports of males may be the more accurate. If so, however, and if the errors in reporting are random, or nearly so, the effects on the results of this study would be to depress the female correlations relative to the male ones (Blalock, 1972:413–5) and, thus, to lower the measured male/female differences. In fact, any sex differences in the accuracy of reporting family income appear to have been largely random, because the distributions of reported family income were nearly the same for males and females, as they must have been in the population.[9] For instance, the means were $16,459 and $16,607 for males and females, respectively, and the standard deviations were $7,540 and $7,698. Thus, it is highly unlikely that male/female differences in the accuracy of reporting family income account to any important degree for the male/female differences found by this study.

CONCLUSION

We believe that the data reported here make very clear the prematurity of the conclusion that "the status structure in the United States is substantially more fluid" when both sexes are taken into account than it appears to be when males alone are considered. In fact, the results of this study suggest that the opposite may well be true. We cannot be sure, of course, since we have not measured all aspects of the status of origin or the status of destination of either males or females (nor is anyone else likely to devise a meaningful summary measure of all aspects of the intergenerational transmission of status).

In any event, it is important if, as the data strongly suggest, there has been greater intergenerational transmission of major dimensions of economic status among females than among males; research is needed to investigate the causes and consequences of that difference.

Norval D. Glenn
University of Texas at Austin
Sandra L. Albrecht
University of Kansas

REFERENCES

Ballweg, John A.
1969 "Husband-wife response similarities on evaluative and nonevaluative survey questions." Public Opinion Quarterly 36:249–54.

Blalock, Hubert M., Jr.
1972 Social Statistics. (2nd ed.) New York: McGraw-Hill.

Chase, Ivan D.
1975 "A comparison of men's and women's intergenerational mobility in the United States." American Sociological Review 40:483–505.

Davis, James A.
1978 Comulative Codebook for the 1972–1978 General Social Surveys. Chicago: National Opinion Research Center.

Ferber, R.
1955 "On the reliability of responses secured in sample surveys." Journal of the American Statistical Association 50:788–810.

Glenn, Norval D., Adreain A. Ross, and Judy Corder Tully
1974 "Patterns of intergenerational mobility of

---

[8] A finding not directly relevant to the purpose of this study but which has implications for the "double standard of aging" (Sontag, 1972) is that, among the married respondents (1973–1977 file), the correlation of age with family income was .284 for men but only .128 for women. This difference must have resulted from the marriage of a good many younger women to affluent older men and/or from the marriage of a good many of the older women to retired men.

[9] Of course, the distributions of family income were identical in the total populations of married men and women, and they had to be quite similar among whites in the age range of 25–54.

females through marriage." American Sociological Review 39:683–99.
Huber, Joan, and William H. Form
1973 Income and Ideology: An Analysis of the American Political Formula. New York: Free Press.
Kish, Leslie
1965 Survey Sampling. New York: Wiley.
Sontag, Susan
1972 "The double standard of aging." Saturday Review: The Society. October:29–38.
Siegel, Paul S.
1971 "Prestige in the American occupational structure." Ph.D. dissertation, University of Chicago.
Tyree, Andrea, and Judith Treas
1974 "The occupational and marital mobility of women." American Sociological Review 39:293–302.

THE IMPACT OF OUTLIERS ON INCOME INEQUALITY*

(COMMENT ON HEWITT, ASR JUNE, 1977; STACK, ASR FEBRUARY, 1979; AND HEWITT, ASR FEBRUARY, 1979)

Hewitt (1977) claimed to have found support for Lenski's (1966:313–25) argument that, within industrial societies, strong socialist parties have effected a considerable redistribution of income in favor of the "lower classes." At the same time, he was unable to find any support for the argument that political democracy, more broadly conceived, has a nonspurious effect on income inequality. More recently, however, Stack has alleged that "an operationalization of political democracy which stresses political participation, as opposed to the structural elements of democracy, is a better predictor of the level of income inequality than social democracy as measured by socialist party strength" (Stack, 1979:171). Hewitt's response is that, despite some problems, Stack's "main point is a good one" and his result "interesting," although Hewitt believes that the issue is complicated, given "the high intercorrelations between the political variables" (Hewitt, 1979:172). As I shall show here, both of these authors' claims are unwarranted, as were the claims concerning the effects of socialist party strength which Hewitt advanced in his original paper.

Stack criticizes Hewitt for placing insufficient emphasis on rates of political participation in his analysis—a reasonable point, given the concern with participation in Lenski's theory. Stack's (1979:170) point—about the need to measure the "overall degree of income differences" rather than to focus solely on income received by the most wealthy—is also reasonable, as Lenski and others were concerned with redistribution from the most wealthy to the least wealthy (the lower classes). Less reasonable is Stack's assertion that, once these two issues are taken into account, participation has a major effect on inequality while socialist party strength does not.

ANALYSIS

Following the suggestion of Stack and others, my measure of income inequality focuses on both ends of the income distribution. Specifically, I use the ratio of the share of income received by the most wealthy population quintile to the share received by the poorest 40% of the population. This measure has three desirable properties. First, it is readily interpretable (and higher scores imply more inequality). Second, it responds to Lenski's concern with both the top *and* bottom ends of the income distribution (in contrast to measures that refer only to the most wealthy). At the same time, the denominator includes the poorest *two* population quintiles in order to capture Lenski's discussion of the lower classes in a way that is not too restrictive. Third, and finally, this measure is more responsive to differences in each end of the distribution—in contrast to the more commonly used Gini ratio, which is most sensitive to differences in the middle range (cf. Allison, 1978:869). However, I should emphasize that the results I report here are not unique to this measure: as interested readers can verify for themselves, similar results are obtained with other ratios, including the Gini ratio.

My data are taken from the same sources that Hewitt and Stack employ.[1] Information on income inequality is drawn from Paukert's (1973) figures, and I adopt the minor modifica-

---

* Direct all communications to: Robert W. Jackman; Department of Political Science; Michigan State University; East Lansing, Michigan 48824.

I would like to thank Mary R. Jackman and Brian D. Silver for their helpful comments.

[1] Hewitt (1977:455) also employs as a second measure of income inequality the spread of earnings between high- and low-paid workers. Unfortunately, as Hewitt acknowledges, this measure completely ignores unearned income and therefore understates the degree of inequality. I therefore do not employ it in the following analysis (note also that it is not discussed in the Stack–Hewitt exchange). However, given that this is the only measure he uses that begins to address both ends of the income distribution, it is especially interesting to observe that even Hewitt's (1977:461–2) results show neither socialist party nor democratic performance effects on the spread of earnings.

Table 1. Regressions of the Ratio of Income Received by the Wealthiest Population Quintile to That Received by the Poorest Two Quintiles on Socialist Strength, Turnout, and Energy Consumption Per Capita

	Constant	Socialist Strength	Turnout	Energy Cons. P.C.	R^2	$\bar{R}^2$	F
South Africa included N = 18	10.471*	.727 −2	−.859 −1*	−.313 −3*	.710	.647	11.41
	(1.250)	(.171 −1)	(.179 −1)	(.147 −3)			
	5.186*	−.420 −1)		−.182 −3	.234	.131	2.29
	(.922)	(.215 −1)		(.277 −3)			
	10.311*		−.813 −1*	−.304 −3*	.706	.666	18.00
	(1.159)		(.139 −1)	(.142 −3)			
South Africa excluded N = 17	5.950*	−.729 −2	−.263 −1	−.194 −3)	.209	.027	1.15
	(2.379)	(.167 −1)	(.320 −1)	(.143 −3)			
	4.051*	−.159 −1		−.144 −3	.168	.049	1.41
	(.553)	(.129 −1)		(.128 −3)			
	6.457*		−.350 −1	−.211 −3	.197	.083	1.72
	(2.017)		(.242 −1)	(.133 −3)			

* Starred coefficients are at least twice their standard errors, which appear in parentheses below them. Scientific notation is used to avoid endless strings of zeros.

tions suggested by Atkinson (1975:248). Socialist party strength is measured in terms of socialist legislative presence, 1945–65, as reported by Hewitt (1977:459). Figures on turnout (the proportion of the adult population voting) are from Taylor and Hudson (1972:54–6), except for three countries for which information comes from Russett et al. (1964:84–5). Finally, like Hewitt and Stack, I take data on energy consumption per capita from Taylor and Hudson's data set. However, since the income inequality data are for circa 1962, I take energy consumption figures for 1960, rather than 1965, as reported in ICPR (1971).

The top row of Table 1 reports a set of estimates similar to those reported by Stack (1979:Table 1), and based on the same 18 countries that both he and Hewitt analyzed.[2] Consistent with his results, the estimates for turnout and energy consumption per capita are both of the expected sign and have t-ratios greater than 2.0. In addition, the estimate for socialist party strength is smaller than its standard error. Hewitt (1979) suggests, however, that this may not resolve the issue due to the "intercorrelation" between the "political variables" (the relevant correlation, noted by Stack, is .59). To pursue this question, the second and third rows of Table 1 report the effects of the political variables separately, net of energy consumption rates. These figures suggest that Hewitt's complaint may have some basis, in the sense that the t-ratio for socialist party strength in the second row is 1.96. Offsetting this is the fact that the equation as a whole accounts for only 13% of the variance, and has an F-ratio that is not statistically significant at even the .10 level. In contrast, the estimates in the third row appear to provide stronger support for Stack's claim, since the coefficient for turnout has a t-ratio of 5.84, while the equation as a whole accounts for two-thirds of the variance in income inequality and has a high F-ratio.

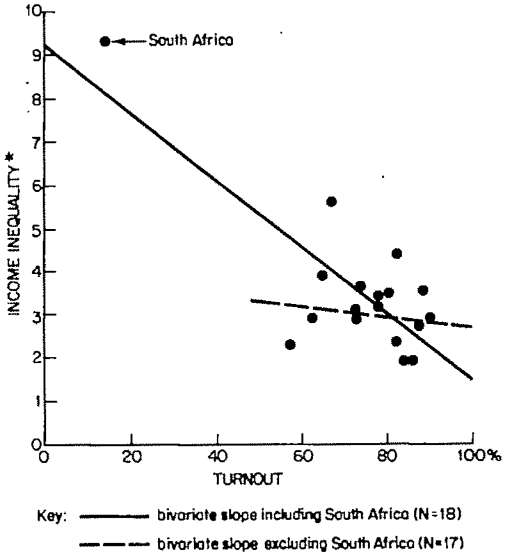
And there the matter would seem to rest. Turnout has a pronounced effect on inequality, while the impact of socialist party strength is marginal. However, it is generally advisable to examine the scatterplots that give rise to estimates such as these, especially when the number of cases analyzed is small (cf. Blalock, 1972:381–3; Anscombe, 1973; Tufte, 1974). Accordingly, Figure 1 arrays income inequality (as defined here) against turnout. Not the least interesting feature of this graph is the presence of an outlier in the northwest corner. Common sense suggests that a check of the robustness of the results reported in the first three rows of Table 1 is warranted. Clearly, we should not rely too heavily on these estimates unless we are sure that they are not a function solely of the inclusion of this single outlier in the analysis.

---

[2] Some readers may think that, as Hewitt (1977:Tables 4–6) does not identify the number of cases analyzed, his analysis of Paukert's data is based on the 25 countries listed in his Tables 2 and 3. This is not, however, the case.

That my results are not identical to Stack's is attributable to two factors. First, my measure of income inequality is slightly different (although, as I have already noted, I obtain the same pattern with Gini ratios in the analyses which follow). Second, both Hewitt and Stack use energy-consumption data for 1965 (I use data for 1960), despite the fact that the data for income inequality are for circa 1962. The sole reason for my focus on the same 18 countries examined by Hewitt and Stack is to maximize the comparability of my results with theirs. I should point out, however, that the inequality data for one of these countries (Italy) are for 1948, which predates the independent variables by a huge margin. Hewitt and Stack blithely ignore this problem, but note that my results below do not depend on the inclusion of Italy in my analysis. Finally, Hewitt's (1977:fn. 1) definition of "modern industrial" identifies the wealth of countries but not the source of that wealth, and is therefore too inclusive (Kuwait in 1965, for example, was not an industrial country!). For a modified definition, see Jackman (1980).

Such a check is particularly important in view of the fact that the outlier is not just any country, but represents the scores for South Africa. Indeed, South Africa does not appear on Lenski's (1966:318–9) list of industrial democracies, for fairly obvious reasons (e.g., nonwhites, an overwhelming majority of the population, do not enjoy basic political rights including the right to vote). Hewitt's (1977:451) original inclusion of South Africa is thus odd, given his claim to be testing Lenski's argument, and given his conclusion, "Strong socialist parties *acting within a democratic framework* appear to have reduced income inequality in industrial societies" (Hewitt, 1977:460, italics added). The location of South Africa within Hewitt's "democratic framework" is certainly questionable, to put it mildly. Stack's inclusion of South Africa is also dubious, as Hewitt (1979:fn. 2) almost notices: the turnout score for South Africa is only 14.3%, which is one quarter the size of the next to lowest figure, and which is three and a half standard deviations below the overall mean turnout figure of 73% for these 18 countries. Looking at the problem another way, the original standard deviation for turnout of 17.4 (when all 18 cases are included) drops to 9.7 when South Africa is excluded (a reduction of 45%). Similarly, the standard deviation for income inequality of 1.7 (N = 18) drops to 0.9 when South Africa is excluded (a reduction of 47%).



*Income inequality is defined as the ratio of income received by the wealthiest population quintile to that received by the poorest 40 percent of the population.

Figure 1. South Africa as an Outlier.

In short, from Figure 1 we learn that there is an extreme outlier in the analysis and that this outlier is having a pronounced effect on the distributions of both the dependent and independent variables. That the case involved is South Africa is of particular interest, given the very restricted political participation in that country. The centrality of race to South African politics makes those politics qualitatively different from the political patterns that prevail in Western industrial democracies.

Accordingly, the bottom three rows of Table 1 repeat the analyses reported in the top half of the table, but with South Africa excluded. The most obvious point here is that excluding just one outlier from the analysis has a dramatic effect on the results. Compare the first and fourth rows of the table. The coefficient for turnout is reduced to one-third its former size; the corrected coefficient of determination plummets from .647 to .027; and the overall F-ratio drops from 11.41 to 1.15. Note that the t-ratio for turnout also drops from 4.79 to less than 1.00. These are drastic changes that indicate that Stack's results can be attributed solely to the inclusion of South Africa in the analysis. (To illustrate these changes graphically, I have drawn the two bivariate slopes in Figure 1.) Indeed, these changes represent a textbook example of the problems that outliers can create. The similarity between Figure 1 and graphs that are explicitly and solely constructed to illustrate the pitfalls of outliers is striking: compare Figure 1 with either Figure 17.10a in Blalock (1972:382) or the top part of Figure 3.18 in Tufte (1974:107).

The fifth row of Table 1 shows that Hewitt's original analysis suffers from the same problem.The coefficient for socialist party strength in this row shrinks to 40% of the size of the corresponding estimate in the second row, and is barely larger than its standard error.[3] The figures in the fifth row also underscore the irrelevance of Hewitt's (1979:172) remark about the possible confounding effects of the correlation between turnout and socialist strength.[4] Finally, the estimates in the bottom row of the table show that, even when socialist strength is excluded from the equation, the outlier effect remains dominant: the F-ratio is less than 2.0 and the $\overline{R}^2$ is .083, while the coefficient for turnout drops to 40% of its original size (and its t-ratio falls from 5.84 to 1.45).

---

[3] While the outlier issue is a severe problem for

CONCLUSIONS

The foregoing analysis clearly demonstrates that there is no justification for any of the conclusions advanced by Stack (1979) and Hewitt (1979). It also suggests that there is little reason to have any confidence in Hewitt's original conclusion. In other words, this analysis is consistent with results reported earlier by Parkin (1971) and Jackman (1975): neither electoral participation nor socialist party strength affects the overall degree of income inequality.

Robert W. Jackman
Michigan State University

REFERENCES

Allison, Paul D.
1978 "Measures of inequality." American Sociological Review 43:865–80.
Anscombe, F. J.
1973 "Graphs in statistical analysis." American Statistician 27:17–21.
Atkinson, A. B.
1975 The Economics of Inequality. Oxford: Clarendon Press.
Blalock, Hubert M., Jr.
1972 Social Statistics. (2nd ed.) New York: McGraw-Hill.
Hewitt, Christopher
1977 "The effect of political democracy and social democracy on equality in industrial societies: a cross-national comparison." American Sociological Review 42:450–64.
1979 "Reply to Stack." American Sociological Review 44:171–2.
ICPR
1971 World Handbook of Political and Social Indicators II: Cross-National Aggregate Data. Ann Arbor: Inter-University Consortium for Political Research.
Jackman, Robert W.
1975 Politics and Social Equality. New York: Wiley-Interscience.
1980 "Socialist parties and income inequality in Western industrial societies." Journal of Politics 42:135–49.
Lenski, Gerhard
1966 Power and Privilege. New York: McGraw-Hill.
Parkin, Frank
1971 Class Inequality and Political Order. New York: Praeger.
Paukert, Felix
1973 "Income distribution at different levels of development: a survey of evidence." International Labour Review 108:97–125.
Russett, Bruce M., Hayward R. Alker, Jr., Karl Deutsch, and Harold D. Lasswell
1964 World Handbook of Political and Social Indicators. New Haven: Yale University Press.
Stack, Steven
1979 "The effects of political participation and socialist party strength on the degree of income inequality." American Sociological Review 44:168–71.
Taylor, Charles L., and Michael C. Hudson
1972 World Handbook of Political and Social Indicators. (2nd ed.) New Haven: Yale University Press.
Tufte, Edward R.
1974 Data Analysis for Politics and Policy. Englewood Cliffs: Prentice-Hall.

Hewitt's (1977) analysis, it is even more crippling for Stack's (1979). This is because in Hewitt's case South Africa represents an extreme score on the dependent variable only, whereas in the Stack case it has extreme scores on both the dependent *and* independent variables.

[4] Elsewhere, I have shown—with these and with more recent and comparable data on income inequality—that socialist parties have not had a marked effect on income inequality. To the extent that they have had any egalitarian effect, their impact has been minimal and has favored middle-income groups, never the least wealthy (i.e., never the poorest 40% of the population), despite the support that these movements receive from this sector (Jackman, 1980).

REPLY TO JACKMAN*

Jackman is able to conclude that Socialism does not affect income inequality because his analysis differs from mine in two respects: first, he examines a different set of countries; second, he uses a different measure of equality.

I agree that its racial situation may make South Africa a special case.[1] I am therefore including a reanalysis omitting South Africa but retaining my original measures.

The results show clearly that even without

---

* Direct all communications to: Christopher Hewitt; University of Maryland Baltimore County; Catonsville, MD 21228.

The author wishes to thank David Jacobs and Roger Lewis for their helpful comments.

[1] I also agree that Kuwait may not be a truly industrial society, but, since I did not include Kuwait in the analysis (due to the absence of any income figures), this is a somewhat minor point.

Table 1. Multiple Regression Analyses of Shares of Income Going to Top 5% Income Category

	Original Sample Excluding S. Africa (N=18)		Sample Excluding S. Africa Plus New Cases (N=23)	
	Unstandardized Regression Coefficient	Beta Coefficient	Unstandardized Regression Coefficient	Beta Coefficient
Socialist Strength	−.1530*	−.4861	−.1495*	−.4577
Energy Per Capita	−.0010	−.3802	−.0012*	−.4196
Intercept	28.8041		29.4206	
R^2	.34		.46	
Multiple Regression Analyses of Shares of Income Going to Top 20% Income Category				
Socialist Strength	−.1458**	−.5525	−.1654**	−.4938
Energy Per Capita	−.0012*	−.5032	−.0015**	−.5082
Intercept	54.5106		56.6688	
R^2	.50		.60	

Significance levels * .05 level ** .01 level

South Africa (that is, with only 18 cases) Socialist Party strength is still significantly associated with a reduction in the share of the top income categories. If additional cases are added, the correlations are higher and more significant.[2] From examination of the scattergrams it is apparent that no outliers are producing these results.

The crux of the disagreement between Jackman and myself does not concern whether or not South Africa is an outlier, but how income inequality should be measured.

Jackman criticizes the measures I used (shares of top 5% and top 20%) and suggests that a measure which captures "the overall degree of income inequality" or "redistribution from the most to the least wealthy" is preferable. I agreee that Socialist governments are expected not only to take from the rich (as I have shown they do), but also to give to the poor. However, we must consider three questions in selecting an appropriate measure: What government policies affect income distribution? At what stage in the policy process is income inequality measured? What is the relation between "class" and income category?

Government policies affect income distribution in three ways:

(1) They alter market forces through monetary policies, affecting employment, inflation and interest rates, or through direct controls.[3]

(2) They tax households both directly and indirectly.

(3) They increase household resources either through direct transfer payments or by providing free or subsidized goods and services.

Since Paukert's (1973) figures are of the distribution of income before tax, they take into account, of course, *only* the effect of the first set of policies plus transfer payments. If we consider income "after tax," the distribution will be more equal; and if we calculate the distribution after the allocation of government goods and services, it will be still more equal. Thus, according to Nicholson's study cited by Atkinson (1975), the Gini coefficient for pretax income in Britain in 1970 was 0.28, but 0.20 after all taxes and benefits were taken into account.

I would argue that interference with the allocation processes of the market economy under Socialist governments will reduce the incomes of rentiers, business and certain professions (for example, physicians). Since such "classes" in the Weberian sense are largely contained within, and equivalent to, the top quintile of income recipients, the decline in this category's share is thereby explained. The lowest quintiles do not gain substantially at this stage because many individuals and households are outside the work force and, hence, unaffected by government economic policies.[4]

---

[2] The additional cases are the next five most highly developed countries listed in Paukert (1973): Greece, Panama, Mexico, Chile and Jamaica.

[3] To take a striking example, physicians in the United States receive the incomes they do because they are able to exploit their market position to its maximum. In the United Kingdom socialized medicine allows the State, as the only effective purchaser of their services, to hold physicians' incomes at a much lower level. Rent control and minimum wage legislation are other examples.

[4] The question of the occupational composition and labor-force status of the income categories needs more study. I am relying upon a chart, *Social Stratification in the U.S.*, which shows that in 1978, managers, self-employed, and professionals constituted more than half of those in the top 20%, while more than two-thirds of these occupations fall into the top quintile. The chart also shows that in the bottom quintile a majority of units had no one in the labor force.

Redistribution to the poor is conventionally assumed to occur through a combination of progressive taxation and welfare state expenditures.[5] Therefore it is necessary, when only pretax figures are available, to make a separate estimate of the redistributive effect of taxation and social service expenditures. I did so in my original study and found that the greatest amount of redistribution occurred in polities with strong Socialist Parties.

I think it probable that Jackman's finding that the poor are not helped by Socialism is explained by the fact that his measure is of inequality before the impact of taxation and social service expenditures.[6] I look forward to reading his forthcoming, more detailed, analysis.

Christopher Hewitt
University of Maryland Baltimore County

REFERENCES

Atkinson, A. B.
1975 The Economics of Inequality. Oxford: Clarendon Press.
Paukert, Felix
1973 "Income distribution at different levels of employment." International Labor Review 108:97–125.
Rose, Stephen and Dennis Livingston
1978 Social Stratification in the U.S. Baltimore: Social Graphic Company.
Washington Post
1979 September 12:21.

---

[5] According to the *Washington Post* (September 12, 1979:21), in Sweden "maintaining the welfare state costs citizens over half their total earnings in national and local taxes, with many above-average wage earners paying much more than that. Swedes, especially those with below-average incomes, receive a lot back in . . . benefits."

[6] It should be noted that in a ratio measure such as Jackman now proposes, the effect of taxation is even more marked than with Gini values. In the United Kingdom in 1967 the top 10% received 28.0% of pretax income while the bottom 30% received 10.4%. The posttax figures were 24.3% and 12.0%, which means that the ratio of top to bottom is 2.7 before tax but only 2.0 after tax (Atkinson, 1975).

IN DEFENSE OF MICROECONOMIC THEORY*

(COMMENT ON COOK AND EMERSON, ASR, OCTOBER, 1978)

In their recent article, "Power, Equity and Commitment in Exchange Networks," Cook and Emerson claim to expand exchange theory through the use of exchange networks. In addition, they argue that their formulation solves two problems that economic theory has not solved: (1) the problem of indeterminacy in the economic study of exchange, and (2) the problem of incorporating the concept of commitment. More specifically, they contend that

> conventional microeconomic theory leaves two-person interaction indeterminate because it is a theory organized around *individual decision making* in an asocial *market* composed of many traders. But when only two parties engage one another in reciprocally tactical interaction, be they husband or wife or organized labor and management, the market mechanism is replaced by *social interaction* and economic theory is left with little to say about the outcome. (Cook and Emerson, 1978:722, emphasis in the original)

We agree that their formulation does seem to advance important notions involved in sociological exchange and equity theory. However, we do not agree that their formulation solves problems heretofore unsolvable by microeconomic exchange theory. This point of contention between ourselves and the authors is an important one: How such a disagreement is resolved will affect the direction of future exchange formulations.[1] If Cook and Emerson are correct in their analysis, then we should more fully develop those concepts suggested by their formulation and disregard economic theory for the study of social relations. If Cook and Emerson are incorrect in their analysis, then we should try to adapt those economic concepts that fit our sociological interests.

---

*Direct all communications to: Jane Sell; Department of Sociology/Anthropology; Texas A&M University; College Station, TX 77843.

[1] Cook and Emerson (1978:721–2) are aware of the importance in distinguishing economic exchange from social exchange and, in fact, emphasize the importance of the distinction:

> There is in Economics a well developed science of exchange. Therefore, if "social" exchange theorists are going to build another science of exchange, it is incumbent upon them to relate their work to economic theory, attending to and highlighting important differences.

The Problem of Indeterminacy in the Economic Study of Exchange

The authors contend that microeconomic theory is indeterminate for predicting exchange outcomes for bilateral monopoly. We believe their contention to be an unfair oversimplification. If the only information available is the indifference curves for actors, it is true that microeconomic theory is indeterminate in its predictions. However, if further theoretical assumptions concerning the nature of the interaction are imposed, the predictions of microeconomic theory are no longer indeterminate. There are numerous rules or norms that can function as constraints upon two-person interactions. If these constraints are given appropriate mathematical expression they allow microeconomic theory to predict the results of two-person interactions.

Coddington (1968) discusses constraints that may be imposed and demonstrates that the outcomes of bilateral monopoly are not necessarily indeterminate. For example, Coddington develops a microeconomic model to predict bargaining outcomes. A series of mathematical constraints are imposed upon the indifference curves for two actors engaged in a bargaining process. These constraints include assumptions concerning the form of actors' bargaining expectations, actors' behavioral adjustment concerning bargaining decisions, and the consistency of actors' decisions over time. The imposition of such constraints allows for the exact prediction of bargaining outcomes in bilateral monopoly.

Thus, it is incorrect to contend that microeconomic theory is indeterminate in its ability to predict the outcomes of two-actor interaction. By imposing mathematical constraints, Coddington (1968), Bowley (1928), and others have provided theoretical solutions to the problem of indeterminacy. When Cook and Emerson state that the microeconomic theory is indeterminate, it is analogous to our stating that social power theory is indeterminate when the equity assumption is not specified. It is not that either theory is indeterminate—it is that both theories require additional theoretical constraints to predict the outcomes of two-actor interactions.[2]

The "Asocial" Market, Rationality, and Commitment

Cook and Emerson maintain that microeconomic theory cannot deal with social interaction because the market concept is "asocial." Their assumption is that microeconomic theory is restricted to the analysis of individuals' decisions regarding material goods. Contrary to their assumption, the market concept in economics may consist of all variables that might affect an actor's decision. Such variables often include material goods and money. However, the market may also include nonmaterial variables such as social approval, status, and respect. Several economists using the concept of market have developed theories about such "noneconomic" behaviors as marriage and child-rearing (Becker, 1973; 1974a; and 1974b); charity (Schwartz, 1970); and suicide (Hamermesh and Soss, 1974). Gramm (1975), for example, formulates a theory in which she assumes that the household derives utility from various commodities. One of the commodities she considers is "childness"—a good which takes time and children to produce. Childness seems an excellent example of a good that is both nonmaterial and directly related to social interaction.

The misconception that economics deals only with material goods in an asocial market may stem from the emphasis placed upon the concept of price. However, price is simply a measure of scarcity and, as such, may have nothing at all to do with material goods and money. In fact, we suggest that the notion of price (what must be given up to obtain an additional unit of a good) is an excellent measure of commitment and has been used in microeconomics in precisely the way in which Cook and Emerson wish to use it. For example, the theoretical measure of the commitment of actor A to actor B is the alternatives A is willing to forgo in order to maintain interaction with B.

The fact that economics deals with rational actors is another reason that Cook and Emerson believe economics cannot deal with social commitment. They believe that the concept of rationality implies that actors seek only to maximize their individual gains, irrespective of other actors. Thus, "exchange partners develop no loyalties or longitudinal commitments to one another (Cook and Emerson, 1978:728)." However, the requirement of rationality is not a dictate about how people act. Rather, it is a heuristic tool for the construction of theories. The notion of rationality can be translated into "consistency." For example, economic theory demands that if an actor made a certain choice under a given set of conditions at one point in time, he or she will make the same choice under exactly the same conditions at subsequent points in time. The choice itself may be considered "irrational" by common standards—for example, the actor may choose to date a man who beats her rather than date another man—but the choice is considered "rational" within the economic framework, if it is consistent over time. Thus, Cook and Emerson are incorrect in their conclusion that the assumption of rationality in economic theory precludes the study of social interactions involving commitments between actors.

---

[2] In the Cook and Emerson formulation, the additional constraint is the equity assumption. In the Coddington formulation, the constraints are several mathematical functions.

We are not saying that the Cook and Emerson article is unimportant. We think it is very important. However, should social power and equity researchers abandon the use of economic theory, as Cook and Emerson would have us do, we feel a serious mistake will be made. It is not that microeconomic theory is fully developed for all our interests (it is not) but, rather, that some of its concepts can yield great theoretical flexibility for sociological theory.

Michael W. Martin
Jane Sell
Texas A&M University

REFERENCES

Becker, Gary S.
1973 "A theory of marriage: part I." Journal of Political Economy 81:813–46.
1974a "A theory of marriage: part II." Journal of Political Economy 82:11–26.
1974b "A theory of social interactions." Journal of Political Economy 82:1063–93.
Bowley, A. L.
1928 "On bilateral monopoly." Economic Journal 38:651–9.
Coddington, Alan
1968 Theories of the Bargaining Process. Chicago: Aldine.
Cook, Karen A. and R. M. Emerson
1978 "Power, equity and commitment in exchange networks." American Sociological Review 43:721–39.
Gramm, Wendy Lee
1975 "Household utility maximization and the working wife." American Journal of Economics 65:90–100.
Hamermesh, Daniel S. and Neal M. Soss
1974 "An economic theory of suicide." Journal of Political Economy 82:83–98.
Schwartz, Robert A.
1970 "Personal philanthropic contributions." Journal of Political Economy 78:1264–91.

DOES MICROECONOMIC THEORY NEED A DEFENSE?

(A REPLY TO MARTIN AND SELL)*

Martin and Sell in their comment on "Power, Equity and Commitment in Exchange Networks" take issue with our treatment of microeconomic theory. They make two basic claims, that: (1) our formulation (while it does "advance important notions involved in sociological exchange theory and equity theory") does not "solve problems heretofore unsolvable by microeconomic theory"; and (2) we imply that "economic theory should be disregarded." Before dealing with the specific issue of indeterminacy in a bilateral monopoly, let us deal with their second claim.

Should exchange theorists "abandon" economic theory? Our discussion of bilateral monopoly was included in order to clarify how our work articulates with and builds upon economic conceptions of exchange. We believe Martin and Sell's claims (1) that we imply in our analysis "economic theory should be disregarded" and (2) that power and equity researchers should "abandon the use of economic theory" are gross misrepresentations of our position. In our article we state: "There is in Economics a well developed science of exchange. Therefore, if 'social' exchange theorists are going to build another science of exchange, it is incumbent upon them *to relate their work to economic theory*" (Cook and Emerson, 1978: 721–2, fn. 3; emphasis added). Our article represents our attempt to relate the two bodies of theory and to develop further social exchange theory grounded upon economic theory.

Is microeconomic theory indeterminate in the case of bilateral monopoly? Martin and Sell also take issue with our statement that "conventional microeconomic theory is indeterminate" in regard to the outcomes of exchanges in a bilateral monopoly. They argue that economic theory is not indeterminate in this case. Many economists would disagree with Martin and Sell. For example, Fellner (1959:10) concludes: "after the establishment of a range of possible values, a *different apparatus* (i.e., something other than economic theory) should be employed in the discussion of what is likely to happen within that range" (emphasis added). That "different apparatus" in our article is social exchange theory.

Essentially, Martin and Sell argue that microeconomic theory can solve the problem of indeterminacy in a bilateral monopoly by including additional assumptions and variables in the theory, such as information on bargaining expectations, bargaining skill, consistency in decision making, and so forth. That the incorporation of additional information (either sociological variables or extra assumptions) resolves the indeterminacy problem is hardly surprising to either sociologists or economists.[1] Coddington (1968:11) agrees with us on this point: "The existence of indeterminacy seems to imply that we cannot achieve a proper understanding of bilateral monopoly without introducing further concepts into the theoretical framework." This is precisely the point we make in our article; if additional assumptions regarding power and equity are specified for two-party exchanges, the problem of indeterminacy is resolved. That is, *with the addition of assumptions* based upon power-dependence theory and equity theory within sociology, derivations can be made regarding the exchange outcomes in what economists refer to as a bilateral monopoly.

---

* Direct all communications to: Karen S. Cook: Department of Sociology; University of Washington; Seattle, WA 98195.

[1] If we are satisfied with making an arbitrary set of

Economists Ferguson and Gould (1975:288) state that "the information the economist has is not sufficient to determine the precise market solution . . . the solution is based on bargaining skills and other personal characteristics *anterior* to the realm of economic analysis" (emphasis added). What is defined by some economists as "anterior" to the realm of economics has in fact been the subject of *extensive* examination within sociology (i.e., social exchange theory, bargaining theory within social psychology, power-dependence theory and equity theory). That economists like Becker (1974) have recently "rediscovered" social interaction is a credit to the field of economics. As Becker (1974:1091) explains it:

> Probably the main explanation for the neglect of social interaction by economists . . . [is due to] excessive attention to formal developments during the last 70 years. As a consequence, even concepts considered to be important by earlier economists, such as social interactions, have been shunted aside.

Economic vs. Social Exchange Theory

Concerning the scope of economic and social exchange theory, Martin and Sell fail to make a distinction between theory and concrete reality or between analytical distinctions and concrete differences. As Cook (1979) points out in her discussion of social vs. economic exchange theory, economists and sociologists tend to use different theoretical assumptions in their analysis of the same phenomenon. The difference resides in the nature of the theories being applied and not in the object of study. For example, economists tend to focus upon single transactions, *abstracted* to a large degree from the social context in which they occur in order to arrive at empirical estimates of important parameters such as price. Economic actors are typically buyers and sellers isolated in theory (i.e., the perfectly competitive market) from organizational contexts and continuing social relations. In contrast, social exchange theory, as formulated by Emerson (1972a; 1972b) and others, treats the exchange relation as the primary unit of analysis, where the exchange relation is defined *as a series of interrelated transactions over time.* Thus sociological theory focuses upon relatively enduring exchange relations among actors which lead to other social processes of theoretical interest (e.g., for Blau, 1964, social differentiation among exchange partners; or for Malinowski, 1922 and Ekeh, 1974, social solidarity and societal integration). The emphasis upon enduring social relations led us to extend social exchange theory beyond the dyad to exchange networks in order to allow for the analysis of exchange principles in networks of social relations. Martin and Sell seem to have overlooked completely the primary purpose and the empirical work reported in our article which analyzes four-party exchange networks, not dyadic exchanges.

The "Asocial" Market, Rationality and Commitment

Martin and Sell infer an erroneous conclusion from the following statement in our article: "Conventional microeconomic theory . . . is a theory organized around individual decision making in an *asocial* market composed of many traders."

They infer that we believe economic theory is restricted to the analysis of material goods. Nowhere in the article do we make this claim. Martin and Sell equate asocial with material goods; we do not. In fact, as Cook (1979) has argued, the primary distinction between economic theory and social exchange theory does not reside in the nature of the actors (i.e., rational or not), the nature of the resources (material or social), or the nature of the markets (perfect vs. imperfect) involved. Further, as Emerson (1976; 1980) and Cook (1979) have argued, the issue of the "rational actor" is fundamentally a question of the psychology of the actors involved and is not a question often dealt with empirically by either economists or sociologists. The notion is usually included in the theory as a "simplifying assumption," as Martin and Sell correctly point out. Emerson's (1972a; 1972b; 1976; 1980) position that the ex-

---

assumptions, as Bowley (1928) did, we can easily resolve the indeterminacy. For example, if we assume that one party can determine both price and quantity (Why not assume this?), of course there's no indeterminacy. What we aim to do, however, is not to select arbitrarily a set of assumptions that are enough to solve the problem but to test the validity of these assumptions and to specify the conditions under which these assumptions hold.

change relation should be the *primary unit of analysis* implies that "rationality" can be treated as a variable attribute of exchange relations or exchange structures and not as an assumption about individual motives. From this perspective, motives, per se, become ancillary to the theory, and debates concerning the motives of the exchange participants are not that informative. Describing consistency in behavior over time as "rational" does not help explain the consistency at all; nor does assuming that people behave consistently over time explain why people do so. Our research attempts to specify the conditions under which exchange actors will behave consistently in one fashion or another regardless of whether we refer to their behavior patterns as rational or not. Our experiment is an attempt to specify empirically the conditions under which power is exercised and commitments emerge within exchange networks. The network structure (degree of power imbalance) can be viewed as an important determinant of the extent to which commitments emerge between exchange partners (regardless of whether or not we view such commitments as "rational" or "irrational").

Karen S. Cook
Mary R. Gillmore
Toshio Yamagishi*
University of Washington

REFERENCES

Becker, Gary S.
1974 "A theory of social interactions." Journal of Political Economy 82:1063–93.

Blau, Peter M.
1964 Exchange and Power in Social Life. New York: Wiley.

Bowley, A. L.
1928 "On bilateral monopoly." Economic Journal 38:651–9.

Coddington, A.
1968 Theories of the Bargaining Process. Chicago: Aldine.

Cook, Karen S.
1979 "Exchange power and equity." Presented at the American Sociological Association Meeting, Boston, August, 1979.

Cook, Karen S. and R. M. Emerson
1978 "Power, equity and commitment in exchange networks." American Sociological Review 43:721–39.

Ekeh, Peter P.
1974 Social Exchange Theory: The Two Traditions. Cambridge: Harvard University Press.

Emerson, R. M.
1972a "Exchange theory, part I: a psychological basis for social exchange." Pp. 38–57 in J. Berger, M. Zelditch, Jr., and B. Anderson (eds.), Sociological Theories in Progress, Vol. 2. Boston: Houghton-Mifflin.
1972b "Exchange theory, part II: exchange relations and networks." Pp. 58–87 in J. Berger, M. Zelditch, Jr., and B. Anderson (eds.), Sociological Theories in Progress. Boston: Houghton-Mifflin.
1976 "Social exchange theory." Pp. 335–62 in A. Inkeles, J. Coleman, and N. Smelser (eds.), Annual Review of Sociology, Vol. 2.
1980 "Social exchange." In M. Rosenberg (ed.), Sociological Perspectives in Social Psychology.

Fellner, W. J.
1959 Competition Among the Few. New York: Knopf.

Ferguson, C. E. and J. P. Gould
1975 Microeconomic Theory (4th ed.). Homewood, Ill.: Irwin.

Malinowski, B.
1922 Argonauts of the Western Pacific. London: Routledge and Kegan Paul.

---

[*] Richard M. Emerson is currently on leave.

ITEMS

■ JAMES C. CRAMER (Fertility and Female Employment) is Assistant Professor in the Sociology Department and Graduate Group in Ecology, University of California at Davis. His current work also includes (a) continuing research on female employment and fertility; and (b) population growth, social impact assessment, and environmental policy analysis.

■ LARRY ISAAC (Political Protest Orientations among Blacks and Whites) is Assistant Professor in the Department of Sociology at Florida State University. He is involved in research which focuses on the determinants of State activity and its relationship to macroeconomic and distributional outcomes. ELIZABETH MUTRAN is a Research Associate, at Duke University, on a National Institute of Mental Health (NIMH) grant, working on the topic, "The Multidimensional Aspects of Widowhood." Her current research extends her interest in socialization experiences within the family context, now primarily focused on the postparental stage. SHELDON STRYKER is Professor in the Department of Sociology and Director, Postdoctoral Training Program in Social Psychology at Indiana University. His book, *Symbolic Interactionism: A Social Structural Version*, is forthcoming from Benjamin/Cummings, Menlo Park. His present research is toward relating commitment, identity salience and role performance.

■ BARBARA LASLETT (Place of Theory in Quantitative Historical Research) is Associate Professor in the Department of Sociology at the University of Southern California. The subject of social change and the family in Los Angeles, California, between 1850 and 1900, occupies her present research.

■ ALAN J. LIZOTTE is Assistant Professor in the Department of Sociology at Emory University. His current research centers on patterns of firearms ownership and use in Illinois, at both the county and individual levels. He is also studying weapon specific homicide trends in Atlanta, Georgia. DAVID J. BORDUA is Professor in the Department of Sociology at the University of Illinois, Urbana-Champaign. He continues his studies of firearms ownership and public policy. With Lizotte, Gary Kleck, and Van Cagle, he coauthored *Patterns of Firearms Ownership, Use and Regulation in Illinois*, Vol. XVIII, a report to the Illinois Law Enforcement Commission.

■ WILLIAM C. RAU (The Tacit Conventions of the Modernity School) is a Visiting Assistant Professor in the Department of Sociology at the State University of New York at Albany. Currently, his research focuses on (1) the development of a structural theory of psychological modernity, (2) clarification of problems in the comparative analysis of value systems, and (3) empirical exploration of the "industrial man" hypothesis in convergence theory.

■ DENNIS P. HOGAN (The Transition to Adulthood as a Career Contingency) is a Faculty Research Associate in the Population Research Center and Assistant Professor in the Department of Sociology, both at the University of Chicago. His book, *Fertility and Family Planning in Rural Northern Thailand*, coauthored with Somphong Shevasunt, is forthcoming from the University of Chicago Community and Family Study Center. He is engaged in further research on the life-course transitions of 20th-Century American males, teenage fertility in Chicago, and fertility levels and trends in southeast Asia.

■ SURESH L. KONDA (An Opportunity Labor Demand Model) is a Ph.D. candidate at Carnegie-Mellon University, where he is researching system models of mobility—incorporating decision-making into stochastic models. SHELBY STEWMAN is Associate Professor in the School of Urban and Public Affairs at Carnegie-Mellon University. His research involves: (1) aging and organizations; (2) longitudinal analysis of the United States Government labor force; (3) development of a methodology for projecting labor demand of construction workers, by craft, in metropolitan areas; and (4) the use of survey methods to examine cognitive processes. . . .

■ CAROL H. WEISS (Truth Tests and Utility Tests) is a Senior Research Associate in the Graduate School of Education, Harvard University. She has two books forthcoming: *Social Science Research and Decision Making* (with M. J. Bucuvalas), from Columbia University Press; and *Making Bureaucracies Work* (with A. H. Barton), from Sage. She is currently conducting a longitudinal study of opinion and practice in the alcoholism treatment field and the extent to which they reflect recent research results. MICHAEL J. BUCUVALAS is a Senior Analyst in the public affairs division of Louis Harris and Associates. There, he is doing survey research on a variety of issues—Vietnam-era veterans, conservation of natural resources, family violence, solar energy applications, and public utility management. He coauthored (with Weiss) the book, *Social Science Research and Decision Making*.

■ DAVID A. McGRANAHAN (The Spatial Structure of Rural Income Distribution) is a Ph.D. candidate in the Department of Rural Sociology, at the University of Wisconsin, Madison. His current research focuses on small-community and regional development as affected by the spatial organization of class and other interests.

■ PAUL ATTEWELL (Comment on Robinson and Kelley) is Assistant Professor, University of California at Santa Cruz, as well as a Postdoctoral Research Fellow at The Institute for the Study of Social Change, University of California at Berkeley. His work presently is in researching economic sociology and complex organizations. ROBERT FITZGERALD is a Ph.D. candidate in the Department of Sociology, also at the University of California at Berkeley. He and Attewell are currently examining various aspects of Marxist stratification theory. He recently completed a study on the proneness of juries to convict when persons opposed to the death penalty were excluded from juries.

■ JOHN BRAITHWAITE (Comment on Gottfredson and Hindelang) is a Criminologist with the Australian Institute of Criminology and is currently researching transnational corporate crime. In press, at the Institute, is his forthcoming book, *Prisons, Education and Work: Towards a National Employment Strategy for Prisoners*, DAVID BILES is Assistant Director of Research at the Australian Institute of Criminology, presently working on the topic of classification of prisoners. His book, *The Management of Long-Term Prisoners in Australia*, written with Grant Wardlaw, is forthcoming.

■ NORVAL D. GLENN (Comment on Glenn, Ross, and Tully) is Professor in the Department of Sociology, at the University of Texas at Austin. His current research deals primarily with (a) aging and attitudinal stability, (b) the noneconomic utility of formal education, and (c) the effects of children on the psychological well-being of parents. SANDRA L. ALBRECHT is Assistant Professor, Department of Sociology, University of Kansas, and her research presently centers on participatory democracy (comparative), social stratification, and the sociology of work.

■ ROBERT W. JACKMAN (Comment on Hewitt, on Stack, and on Hewitt) is Associate Professor of Political Science, Michigan State University. He is doing research on (1) Coups d'etat and related forms of political violence in Third World countries, and (2) the nature of social class identification in the United States.

■ MICHAEL W. MARTIN (Comment on Cook and Emerson) is Assistant Professor, Department of Sociology, at Trinity University, San Antonio, and is also doing research on the factors affecting individuals' choices of equity norms. JANE SELL is Assistant Professor, Department of Sociology, Texas A & M University. She is currently researching the mathematical definition and empirical estimation of reinforcements with varying strengths.

New from Pittsburgh

THEORETICAL METHODS IN SOCIOLOGY: SEVEN ESSAYS **$19.95s**
Lee Freese, Editor
"I shouldn't be surprised if this book becomes the authoritative source for sociologists on axiomatic theorizing. Certainly it clears away several prevalent misconceptions."
—Robert W. Avery

In analyzing various theoretical methods and issues, Freese and his colleagues imply that a cumulative science of sociology would have to ask different kinds of questions than those which typically guide sociological research.

FEMALE AND MALE IN LATIN AMERICA: ESSAYS ***NOW IN PAPERBACK*** **$6.95s**
Ann Pescatello, Editor

This pioneering study of Latin American women is based on current disciplinary methodologies and original field research undertaken by historians, anthropologists, sociologists, demographers, psychologists, political scientists, and literary critics.

University of Pittsburgh Press
Pittsburgh, PA 15260

CONTEMPORARY SOCIOLOGY

A Journal of Reviews

Norval D. Glenn, Editor ISSN 0094-3061

A journal of reviews, criticism, and debate designed to give new thrust and style to reviewing and intellectual exchange within the field.

CONTEMPORARY SOCIOLOGY publishes reviews and critical discussions of recent works in sociology and in related disciplines which merit the attention of sociologists. Since all sociological publications cannot be reviewed, a selection is made to reflect important trends and issues in the field.

Recent issues have featured articles by:

David B. Bills
Theodore E. Long
Amos H. Hawley
Lewis A. Coser
Kenneth C. Boulding
Alex Inkeles
Edward A. Tiryakian
William Julius Wilson

- Members, $10
- Non-members, $15
- Institutions, $20
- Non-member students, $10
- Single issues, $4

Order from: ASA, 1722 N Street, N.W. • Washington, D.C. 20036

SOCIAL PSYCHOLOGY QUARTERLY

(formerly SOCIOMETRY)

A Journal of Research and Theory in Social Psychology
(George W. Bohrnstedt, Editor)

Genuinely interdisciplinary in the presentation of works by both sociologists and psychologists (Quarterly—ISSN 0190-2725)

Recent issues have dealt with:

Alternative Interpretations (Freese & Rokeach)
Motivational Relevance of Educational Plans (Alexander & Cook)
Why a Rejection? (Wiley, Crittenden & Birg)
Bayesian Model for Performance Expectations (Foschi & Foschi)
Explanations of Black Self-Esteem (Taylor & Walsh)
Varieties of Interpersonal Attraction (Segal)

Special rate to ASA members, $8.00
Non-members, $12 **Institutions, $16** **Single issues, $4**

THE AMERICAN SOCIOLOGICAL ASSOCIATION
1722 N Street, N.W. • Washington, D.C. 20036

ORGANIZATION STUDIES

An international multidisciplinary journal devoted to the study of organizations, organization, organizing and the organized in, of and between societies

A New Quarterly

Organization Studies aims to provide a more internationally based forum than has previously existed for ideas and research, and for the exchange of personal experiences, in the study of organization and the organized. The scope of Organization Studies will be broadly rather than narrowly conceived across the diversity of social science approaches and paradigms. It will range from the theoretical, even the speculative, to relevant contemporary problems and empirical research reports.

Editor-in-Chief	David J. Hickson, Univ. of Bradford
Managing Editor	Karen Tkach, The Hague
Editorial Board	F. Agersnap, Copenhagen Sch. of Econ. and Social Science; M.Aiken, Univ. of Wisconsin; G. Benguigui, Univ. of Paris; M. Crozier, Cent. for the Socio. of Organizations, Paris; J. Eldridge, Univ. of Glasgow; F. Ferraresi, Univ. of Turin; J. Hage, Univ. of Maryland; F. Hegner, Int'l Inst. of Manag. and Admin., Berlin/Univ. of Bielefeld; B. Hinings, Univ. of Birmingham; G. Hofstede, Europ. Inst. for Adv. Studies in Management, Brussels/Int'l Inst. for App. Systems Analysis, Austria; J. de Kervasdoué, Polytechnical Sch., Paris; A. Kieser, Univ. of Mannheim; C. Lammers, Univ. of Leyden; R. Mayntz, Univ. of Cologne; J. Olsen, Univ. of Bergen; V. Rus, Univ. of Ljubljana; B. Stymne, Stockholm Sch. of Econ.; H. Thierry, Univ. of Amsterdam; J.-C. Thoenig, INSEAD, Fontainebleau
Subscription rates	Per volume of four issues Libraries and institutions DM 88.–/US$ 45.00 Individuals DM 48.–/US$ 25.00

The first issue will be published in January 1980

Published in collaboration with the
European Group for Organizational Studies (EGOS) by
WALTER DE GRUYTER · BERLIN · NEW YORK

Verlag Walter de Gruyter & Co, Genthiner Strasse 13, D-1000 Berlin 30
Walter de Gruyter, Inc. 200 Saw Mill River Road, Hawthorne, N.Y. 10532

TRADITION and *INNOVATION* for 1980

Focus on Society: An Introduction to Sociology, Second Edition

Norman W. Storer, Bernard M. Baruch College, City University of New York

Comprehensive and penetrating examination of the basic structures and dynamics of society. Clear writing and a carefully-developed presentation of a modified structural/functional perspective provide unity and coherence. In this edition are: new chapters on socialization and changing sex roles, updated "focus" sections on key concepts and ideas, conceptual diagrams, end-of-chapter glossaries, and appendices on the empirical research in the field. Accompanied by an Instructor's Manual with tests.

(07337) 544 pp. paper *Available Now*

Introduction to Social Problems

Leonard Glick and Daniel E. Hebding, both of Community College of Philadelphia

The aim of this well-researched text-reader is to examine the nature and causes of, the interrelationships, and possible solutions to contemporary social problems. Emphasizes the sociological approach to the study of social problems, but also draws on evidence and theory from other fields. Excellent pedagogy includes lists of key terms at chapter-end, margin notes, chapter summaries. Comprehensive Instructor's Manual.

(02600) 544 pp. paper *Available Now*

Gerontology: A Behavioral Science Approach

Richard C. Crandall, Lake Superior State College

Comprehensive and thoroughly documented examination of issues from a variety of perspectives. Includes chapters on the chronological development of gerontology as a science and on the aged in preliterate society. Instructor's Manual with tests.

(01252) c. 496 pp. *Available Now*

The Social Reality of Death: Death in Contemporary America

Kathleen C. Charmaz, California State University at Sonoma

Important new book uses a distinctively sociological approach to integrate the interdisciplinary materials in the field of death and dying.

(01033) 352 pp. paper *Available Now*

For complimentary copy consideration, write to Abigail Winslow at Addison-Wesley Publishing.
Be sure to mention course title, enrollment, and author of current text.

Social Science & Humanities Division
Addison-Wesley Publishing Company
Reading, Massachusetts 01867

New from THE FREE PRESS

THE FAITH AND DOUBT OF HOLOCAUST SURVIVORS

REEVE ROBERT BRENNER

The author asks the survivors of the Holocaust a question never before asked: To what degree, and in what ways, did the experience in the death camps, in the ghettos, or in hiding, affect the religious beliefs of those who survived? The responses of almost 1,000 survivors reveal their conversions, rejections, reaffirmations, doubts, and despairs.

ISBN 0-02-904420-0 *$12.95*

THE FACTS ABOUT "DRUG ABUSE"

THE DRUG ABUSE COUNCIL

The prestigious Drug Abuse Council presents the results of seven years of research on the use and misuse of narcotics and alcohol, criticizing stereotypes and misconceptions that have shaped popular opinion and public policy.

ISBN 0-02-907720-6 *$14.95*

A GUIDE TO THE CULTURE OF SCIENCE, TECHNOLOGY, AND MEDICINE

Edited by PAUL T. DURBIN

This monumental volume covers the history, philosophy, and sociology of science, technology, and medicine. The most up-to-date and comprehensive guide to bridge the gap between the sciences and humanities.

ISBN 0-02-907820-2 *$40.00*

DIVORCE REFORM

Changing Legal and Social Perspectives

LYNNE C. HALEM

Interweaving sociological and historical perspectives, this book insightfully traces the changes in the laws which govern divorce, and the ways in which judges apply those laws, as reflections of changing views on marriage and divorce.

ISBN 0-02-913570-2 *$15.95*

OLD FOLKS AT HOME

ALVIN RABUSHKA & BRUCE JACOBS

The authors show that most of America's senior citizens are *not* ill-housed, frail, or poor, though these notions are leading us to tragically wasteful public programs. "Common sense at last! *Old Folks at Home* redresses errors in stereotyped, sentimental thinking about the elderly. . . .This is research that *tells*!" —Robert W. Adamson, Fordham University.

ISBN 0-02-925670-4 *$10.95*

ALIENATION AND CHARISMA

A Study of Contemporary American Communes

BENJAMIN ZABLOCKI

The definitive and most exhaustive sociological treatment of the American commune to date. The author studied 120 communes in an attempt to produce a general theoretical explanation for the formation and viability of these groups and their ideological success or failure.

ISBN 0-02-935780-2 *$22.95*

Order from:

THE FREE PRESS

A Division of
Macmillan Publishing Co., Inc.
200D BROWN STREET
RIVERSIDE, N.J. 08370

ROUTLEDGE & KEGAN PAUL

9 Park Street, Boston, Mass. 02108

Founding Theory of American Sociology 1881-1915

ROSCOE C. HINKLE *Professor of Sociology, Ohio State University, Columbus*

Based on a comparative study of the theories of such sociologists as Ward, Sumner, Giddings, Ross, Small and Cooley, this is a systematic and rigorous analysis of the main features of earlier sociological theory in the US. Professor Hinkle shows that early theory focused on social ontological interests, the pervasive ontological stance being evolutionary naturalism, within which some sociologists adhered to a social evolutionary theory of change while others preferred a social process theory. Contrasting earlier and later theories, the book provides a basis for interpreting one period in American theory in relation to others. *International Library of Sociology* 135.00

The Material Word

Some Theories of Language and its Limits

DAVID SILVERMAN and BRIAN TORODE *Goldsmith's College, London and Trinity College, Dublin*

The Material Word is an exploration of language as a body of practices. Each section begins with an interrogation of an important theory of language (including Wittgenstein, Husserl, ethnomethodology, J.S. Austin, Barthes, Derrida and Foucault). The results are applied to related texts (Althusser, Locke, Bernstein, Labov, Kafka, Robbe-Grillet, and Perrault). $24.50, paper $14.95

The Inheritance of Inequality

LEONARD BROOM, F. LANCASTER JONES, PATRICK McDONNELL and TREVOR WILLIAMS

'This is the most extensive study of stratification and social mobility in the status attainment tradition thus far reported for any national population other than the United States [Australia] ... carefully done and well integrated with current theoretical development.' — *from the Foreword by Professor William H. Sewell of the University of Wisconsin. International Library of Sociology* $27.50

Organization, Class and Control

STEWART CLEGG and DAVID Dunkerley *Griffith University, Brisbane and School of Behavioural and Social Science, Plymouth Polytechnic, England*

This is the most complete book to have appeared on the subject and is likely to become indispensable to all who study organizations. Its major innovation lies in its reconceptualization of organization as control of the labour process in the context of a particular mode of production, that of capitalism. April *International Library of Sociology* $40.00, paper $20.00

The International Yearbook of Organization Studies 1979

EDITED BY DAVID DUNKERLEY and GRAEME SALAMAN

Contributors: Erik Olin Wright, Chilla Bulbeck, Alexander Kouzmin, Gibson Burrell, Paul Goldman, Donald R. Van Houten, D. Eleanor Westney, Craig R. Littler, Peter Singelmann, James L. Price, Allen C. Bluedorn, Meredith Gould, Stephen Mugford

This is the first volume in a series conceived to bring together contemporary developments in the field of organization studies. $37.50

The Ceremonial Order of The Clinic

P. M. STRONG *University of Oxford*

This study of over one thousand American and Scottish paediatric consultations offers a significantly improved account of medical consultation which can be applied to adult as well as child patients. *International Library of Sociology* $20.00

Ideas and Intervention

Social Theory for Practice

JOE BAILEY *Kingston Polytechnic, England*

This book presents a practical view of the importance of social theorizing. It examines social intervention in three areas — law, social work, urban planning — and concludes by proposing a pluralist view of theory as the best strategy for a sociology relevant to practice. $24.00, paper $12.00

Language and Literacy

The Sociolinguistics of Reading and Writing

MICHAEL STUBBS *University of Nottingham*

This book provides a basis for a socio-linguistic theory of literacy. The author believes that a systematic theory of literacy must be based on an understanding of a number of factors, such as the relationship between spoken and written language, the social, educational and technological pressures on written language, and the variability of spoken language and the relative uniformity of written language. *Routledge Education Books* $25.00, paper $12.50

Disclosures To A Stranger

Adolescent Values in an Advanced Industrial Society

TOM KITWOOD *University of Bradford*

The condition known as 'adolescence' is examined in four broad areas: family life, relations with other adolescents, formal and informal work, and the development of 'self-values.' *Social Worlds of Childhood* $32.50

Now in Paperback

Origin and Significance of The Frankfurt School

A Marxist Perspective

PHIL SLATER *The City University, London*

'A tightly written, thoroughly researched and most thoughtful analysis' — Choice

'A significant and worthwhile work.' — Robert Gogel, *Sociology and Sociological Research* *International Library of Sociology* $7.95

Meaning and The Moral Sciences

HILARY PUTNAM *Professor of Philosophy, Harvard University*

'Putnam is an engaging and tantalizing thinker. I suspect that the book will attain a position within philosophy of the social science which will make it required reading for every aspiring methodologist.' — Philip Pettit, *The Times Higher Education Supplement* *International Library of Philosophy* $4.95

Class Structure in the Social Consciousness

Translated from the Polish by Sheila Patterson

STANISLAW OSSOWSKI *Late Professor of Sociology, University of Warsaw*

'It deals with some of the most important problems of our time with a perceptiveness rarely present in comparable Western studies, the results not only of a perspective widened by historical experience, but also of the author's encyclopedic knowledge.' — *Views and Reviews* *International Library of Sociology* $10.50

The Rules of Disorder

PETER MARSH, ELIZABETH ROSSER and ROM HARRÉ

'A most compelling picture of what has previously been the chaos and the din that the media have faithfully portrayed.' — Anthony Clare, *Psychology Today* *Social Worlds of Childhood* March $7.95

Reissues

Revolution in a Chinese Village: Ten Mile Inn

ISOBEL and DAVID CROOK

First published in 1959, this book is a ten years' history of one Chinese village, covering the period immediately before the setting up of the Chinese People's Republic. The authors describe in vivid detail, based on first-hand observation, the social, political and economic changes involved in passing from the landlord regime to land redistribution under Communist rule. *International Library of Sociology* $20.00

The First Years of Yangyi Commune

ISOBEL and DAVID CROOK

'The Cooks of Peking have done a major service in producing this sequel to their earlier *Revolution in a Chinese Village.* It will rank for many years as the best insight yet offered us into the mobilization of one-fifth of humanity into the communes.' — *The Times Literary Supplement* *International Library of Sociology* $22.50

DORSEY

IMPORTANT NEW TITLES

INTERVIEWING: Strategy, Techniques, and Tactics, Third Edition
Raymond L. Gorden, Antioch College
Presents basic concepts relevant to the interviewing process and richly illustrates them with concrete examples. Provides dozens of skill-building activities in the form of critical questions, laboratory problems, and field problems.

EXPERIENCING SOCIAL RESEARCH, Revised Edition
(paperbound)
John F. Runcle, Development Analysis Associates, Inc.
Combination of text and carefully chosen problems, stressing that the student should learn the more theoretical aspects of research methods and then apply what has been learned in solving the problem. Suggestions are given for the method of approach and the analysis of the gathered data.

THE URBAN ENVIRONMENT *(paperbound)*
Samuel E. Wallace, University of Tennessee, Knoxville
Integrates and synthesizes urban theory and research within a comprehensive perspective of ecology. It analyzes the historical past as it structures the present, the present as urbanites live it, and the future as seen in contemporary patterns.

INTRODUCTION TO SOCIAL WELFARE AND SOCIAL WORK: Structure, Function and Process
Beulah R. Compton, University of Minnesota
Introduces students to the development of social welfare, the profession of social work and the issues involved in the development of both the institution and the profession. Kahn's notion of systems of social welfare as an organizing framework for social welfare and a sociological perspective on the professions as the framework for that consideration is used.

Examination copies available on request

THE DORSEY PRESS
homewood, illinois 60430

THE SOCIAL SYSTEM OF THE PLANET EARTH

by Kenneth B. Boulding, University of Colorado,
Elise Boulding, Dartmouth College, and Guy M. Burgess,
Massachusetts Institute of Technology

Innovative! Engaging! A unique interdisciplinary treatment of the world social system. A science fiction framework postulates the appearance of an extraterrestrial visitor who seeks knowledge of the Earth. Students get involved in organizing a research institute to gather and interpret data on population dynamics, history, religion, science, economics, social stratification, distribution of wealth, the international system, and more for this visitor.

AVAILABLE NOW (00207) paper 1980
Instructor's Manual *(00208)*

For complimentary copy consideration, write to Abigail Winslow. Be sure to mention course title, enrollment, and text now in use.

Social Science & Humanities Division
Addison-Wesley Publishing Company
Reading, Massachusetts 01867

THE HIGHLY ACCLAIMED, HIGHLY REGARDED AND *HIGHLY CONTROVERSIAL* NEW BOOK THAT SETS FORTH A CHALLENGING NEW THEORY OF HUMAN NATURE

"Mary Midgley refutes, in this lively and accessible essay, a host of what she believes are dangerously, sometimes willfully simplified theories about man's nature—on the part of biologists, psychologists and other social scientists, and some of the major philosophers of the past two centuries..."
—*The Atlantic Monthly*

"This is a very important book. Mrs. Midgley is a rare philosopher who has learned enough evolutionary biology to understand its deeper layer of meaning."
—*Iris Murdoch*

BEAST AND MAN
THE ROOTS OF HUMAN NATURE
MARY MIDGLEY

Cutting across many disciplines—psychology, philosophy, ethology, biology, sociology—this is a serious and richly informative new book that deepens our understanding of other species while showing us why we behave the way we do.

Meridian **F529/$5.95**

New American Library Education NAL **1633 Broadway New York 10019**

American National Election Studies Data Sourcebook, 1952–1978

Warren E. Miller, Arthur H. Miller, and Edward J. Schneider

Here, for the first time in book form, is the most basic information describing the recent electorate's partisan loyalties, attitudes on issues, opinions about the American political system, and participation in elections and election campaigns. Anyone with an abiding interest in American national politics will find this invaluable. 456 pp. $25.00

American Social Attitudes Data Sourcebook, 1947–1978

Philip E. Converse, Jean D. Dotson, Wendy J. Hoag, and William H. McGee, III

A unique record of trends in social and economic attitudes held by the American adult population over the past 30 years is available in book form from the Survey Research Center at the University of Michigan. Survey items include social class location, racial attitudes, views on women's roles, work and retirement, life quality, family financial situations, the state of the national economy, government spending and war and peace. 392 pp. $25.00

Injury to Insult

Unemployment, Class, and Political Response

Kay Lehman Schlozman and Sidney Verba

It is commonplace in contemporary American politics for those who experience economic strain to join together and ask the government for help, yet today's unemployed have not done so. Schlozman and Verba look closely at the unemployed and ask why not. The authors consider the political attitudes and behavior of the unemployed, as well as the changing role of social class in America and its implications. $20.00

Organization Without Authority

Dilemmas of Social Control in Free Schools

Ann Swidler

Here is one of the first systematic sociological treatments of the alternative organizations that emerged from the social and political movements of the 1960s and '70s. Based on rich, ethnographic case studies of two free schools, this book describes the distinctive patterns of leadership, social pressure, and social control that emerge in such organizations. $12.50

Harvard University Press

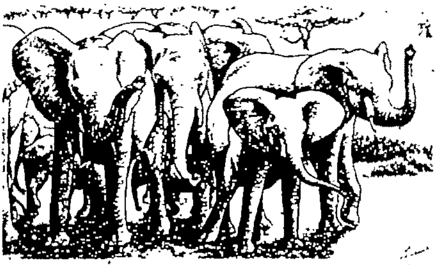
Cambridge, Mass. 02138

Sociobiology

The Abridged Edition

Edward O. Wilson

In this new edition, Wilson trims his monumental work to its essential argument. Eliminating technical discussions and data summaries, he retains the full basic structure of the original work. His controversial chapter on human social behavior is left intact.



Sarah Landry

"It is impossible to leave Wilson's book without having one's sense of life permanently and dramatically widened."
—Fred Hapgood, Atlantic

Belknap   96 illus. by Sarah Landry, 384 pp. $18.50 cloth; $9.95 paper

Themes of Work and Love in Adulthood

Neil J. Smelser and Erik H. Erikson, Editors

Leading experts in adult development explore the stages and crises adults pass through as they attempt to create meaningful lives for themselves. Contributors include: Neil J. Smelser, Erik H. Erikson, Ann Swidler, Robert LeVine, Daniel Levinson, Roger Gould, and Marjorie Fiske. The result is a book that has many unusual rewards. $15.00

Bastardy and Its Comparative History

Studies in the History of Illegitimacy and Marital Nonconformism

Peter Laslett, Karla Oosterveen, and Richard M. Smith, Editors

This is the first historical and sociological examination of bastardy in a variety of settings and over time. Illegitimacy during a four hundred year span in Britain, Western Europe, North America, and other countries is measured and placed in the larger perspectives of sexual conduct, social-cultural behavior, marriage, and economic change. This comprehensive volume will interest social historians, family historians, and all those engaged in writing sociological history. Studies in Social and Demographic History   480 pp. $35.00

Runnin' Down Some Lines

The Language and Culture of Black Teenagers

Edith A. Folb

With a Foreword by Claudia Mitchell-Kernan, Director,
Center for Afro-American Studies, UCLA

Folb spent eight years talking with black teenagers on the streets of south central Los Angeles, the ghetto area that includes Watts. Her skill in recording and interpreting the complex uses of the vernacular, "the lines," has led to this remarkable book on how these young people live—their actions, beliefs, attitudes, and values. More than a language, black teenage talk is a way to survive. Folb's book is an introduction to the dignity and ingenuity of that survival. $15.00

Harvard University Press

Cambridge, Mass. 02138

5 New Winners From Little, Brown

1. **SOCIAL PROBLEMS,** by Burton Wright and John P. Weiss. Cloth/556 pages/$15.95 tentative/with Instructor's Manual and Study Guide. This new introductory text features a unique integrating framework to help students examine 15 contemporary social problems. The authors consistently provide a useful historical background for each problem to show students how societal conflicts arise, are perpetuated, and, ultimately, might be solved.

2. **PRISONS IN TURMOIL,** by John Irwin. Paper/277 pages/$6.95 In his provocative new book, scholar and former prisoner John Irwin offers a fascinating account of how prisoners cope with prison life, including an analysis of the major types of social structures that arise in prisons.

3. **SOCIAL GERONTOLOGY: AN INTRODUCTION TO THE DYNAMICS OF AGING,** by David L. Decker. Cloth/c. 400 pages/$14.95 tentative/with Test Bank. This new text analyzes recent demographic shifts to explore the growing social significance of aging and discusses a wide variety of problems currently facing older Americans.

4. **THE SOCIOLOGY OF HEALTH: PRINCIPLES, PROFESSIONS, AND ISSUES,** by Fredric D. Wolinsky. Cloth/449 pages/$15.95 tentative. This comprehensive text focuses on major principles, professions, and issues of medical sociology and offers an in-depth analysis of the relationship between social factors and health.

5. **FAMILY IN TRANSITION: RETHINKING SEXUALITY, CHILD REARING, AND FAMILY ORGANIZATION,** Third Edition, edited by Arlene Skolnick and Jerome H. Skolnick. Paper/579 pages/$9.95 tentative/with Test Bank. In the Third Edition of this popular reader, **more than 70% of the material is new,** including a major new section on couples with articles on love, divorce, divorce law, and remarriage.

All of these new titles are **now available for examination.**

COLLEGE DIVISION
LITTLE, BROWN AND COMPANY
34 Beacon Street, Boston, Massachusetts 02106

HOOVER

THE UNITED STATES IN THE 1980s
P228 912 pages **$20.00** cloth
Peter Duignan
Alvin Rabushka
editors

THE THIRD CENTURY
America as a Post-Industrial Society
P203 468 pages **$14.95** cloth
Seymour Martin Lipset
editor

TERRORISM
Threat, Reality, Response
P204 480 pages **$14.95** cloth
Robert H. Kupperman
Darrell M. Trent

PRESENT DANGER
Towards a Foreign Policy
P216 170 pages **$12.00** cloth
Robert Conquest

LEBANON
The Fragmented Nation
P227 304 pages **$16.95** cloth
David C. Gordon

POVERTY AND TRANSFERS IN-KIND
A Re-evaluation of
Poverty in the United States
P219 108 pages **$5.95** paper
Morton Paglin

UNILEVER OVERSEAS
The Anatomy of a Multinational
P205 634 pages **$22.00** cloth
David Fieldhouse

HOOVER INSTITUTION PRESS
Dept. A8017 • Stanford University • Stanford, CA 94305
(When ordering, please include $1.50 postage. California residents add 6-6½% tax.)

RECOMMENDED

For national and international institutions whose task it is to organize, plan and finance scientific research
For research workers and their professional associations
For the educated reader in general

an invaluable reference source and teaching aid now complete

Main Trends of Research in the Social and Human Sciences

The result of a study initiated by UNESCO-and carried out in collaboration with national and international organizations and research centers in the social and human sciences, as well as with scores of individual scholars, MAIN TRENDS describes and diagnoses key tendencies in contemporary research and its results.

This three - volume set is a research effort of extraordinary scope and applicability, comprising

new
Part Two **ANTHROPOLOGICAL AND HISTORICAL SCIENCES / AESTHETICS AND THE SCIENCES OF ART / LEGAL SCIENCE / PHILOSOPHY**

The principal research trends in the historical disciplines, the legal sciences, the critical study of art and literature, philosophy, and social and cultural anthropology.
XLVIII + 1592 pp. Cloth. 2 vols.
DM 255.00 / US$ 125.00

Already published
Part One **·THE SOCIAL SCIENCES**

An examination of the present state as well as the perspectives for development of various social sciences disciplines, and of inter- and multidisciplinary dimensions of research in these fields.
XLVIII + 820 pp. Cloth.
DM 115.00 / US$ 55.00

These volumes are also available in French under the title of
Tendances principales de la recherche dans les sciences sociales et humaines.

Please send orders to your regular bookseller, or directly to:
for USA and Canada:
Mouton Publishers, 200 Saw Mill River Road, HAWTHORNE NY 10532
for all other countries:
Walter de Gruyter & Co., P.O. Box 110240, D-1000 BERLIN-11
Prices are subject to change without prior notice.

mouton publishers

The Hague · Paris · New York

SOCIOLOGY OF EDUCATION:

A Journal of Research in Socialization and Social Structure

(Alan C. Kerckhoff, *Editor*)

(Quarterly—ISSN 0038-0407)

Sociology of Education publishes papers on educational processes, and on human development. The research may focus on the individual, institutions, and structural arrangements among institutions bearing on education and human development.

Recent issues have included studies on:

- *Interactive Influences in Spady and Tinto's Conceptual Models of College Attrition*—Pascarella & Terenzini
- *Loose Structure, Retrenchment and Adaptability in the University*—Rubin
- *School SES Influences—Composition or Context?*—Alexander, Fennessey, McDill & D'Amico
- *Academic Sponsorship and Scientists' Careers*—Reskin
- *Three Perspectives on IQ and Social Differentiation*—Guterman, Gordon & Rudert, Eckland

ASA members, $8 **Non-members, $12**
Institutions, $16 **Single issues, $4**

Order from: **American Sociological Association**
1722 N Street, N.W.
Washington, D.C. 20036

CHARLES C THOMAS • PUBLISHER

NEW CRIME AND THE ENVIRONMENT

By **K. D. Harries,** *Oklahoma State Univ., Stillwater.* This concise and pointed analysis focuses on the relationships of human and physical environments to the origin of criminal behavior.

Human environments are analyzed from two perspectives: *macroenvironmental* (national, regional, and urban) influences and *microenvironmental* (intraurban and local) influences. In the macroenvironmental context, the crime of homicide is used to illustrate the considerations which may enter interpretation of criminal behavior on the regional scale. Burglary, rape, assault, and robbery are then critically evaluated within the microenvironmental context. Recent studies also are examined for their strengths and weaknesses, and their essential findings are synthesized.

Later sections focus on the controversial and often misunderstood problems of population density and crowding, the relationship between crime and distance, and the role of environmental considerations in crime analysis and in criminal justice planning. Other topics discuss land use patterns, crime displacement, proximal space, and climate. *'80, 164 pp., 13 il., 21 tables, $14.00*

GAMBLING TODAY *edited by* **David Lester.** Gambling and gambling behavior are herein analyzed for their economic, sociological, criminological, legal, psychological, and psychiatric ramifications. *'79, $12.75*

POLICE OF AMERICA: A Personal View, Introduction and Commentary *by* **Harold K. Becker** *and* **Jack E. Whitehouse.** Topics include the need for police; their evolution, organization, training, and purpose; and reasons why their function is not to secure a crimeless society. *'79, $9.75*

SOCIAL CONFLICT AND MENTAL HEALTH SERVICES *by* **Robert D. Borgman.** Theory is interlaced with intervention strategies to provide a unique approach to such problems as suicide, homicide, school aversion, and adolescent social conflicts. *'79, cloth-$27.00, paper-$20.25*

CAREER DEVELOPMENT AND COUNSELING OF WOMEN *edited by* **L. Sunny Hansen** *and* **Rita S. Rapoza.** This text is concerned with the environmental and psychological variables which inhibit or facilitate all aspects of female development. *'78, $30.00*

SENTENCING: Process and Purpose *by* **Gerhard O. W. Mueller.** The American correctional system is examined, and alternatives to prison are presented. *'77, $18.50*

PUBLIC WELFARE SYSTEMS *by* **J. W. LaPatra.** Topics such as poverty, social policy, welfare, private and public attitudes, economics, politics, justice, and social services are considered. *'75, $16.00*

New! **THE KEEPERS: Prison Guards and Contemporary Corrections** *edited by* **Ben M. Crouch.** Chapters focus on selection and training of guards, styles of guarding, guard turnover, guard-inmate relations, and related subjects. *'80, $22.50*

PLURALISM IN EDUCATION: Conflict, Clarity, and Commitment *by* **Richard Pratte.** This text exposes the negative influence of ideologies that mask outmoded educational policies and practices. *'79, cloth-$15.75, paper-$9.75*

OUR CHILDREN SHOULD BE WORKING *by* **William N. Stephens.** The role of work in shaping the character, individual work habits, and personal initiative of children is analyzed in this text. *'79, $12.75*

PROGRESS IN BEHAVIOR THERAPY WITH DELINQUENTS *compiled and edited by* **Jerome S. Stumphauzer.** Numerous treatment, training, and preventive programs in both institutional and community settings are examined. *'79, cloth-$32.75, paper-$25.00*

CREATING COMMUNITY ACCEPTANCE FOR HANDICAPPED PEOPLE *by* **Roberta Nelson.** The author discusses physical and attitudinal barriers, ways to elicit support from the community, and issues of habilitation and rehabilitation. *'78, $16.25*

CIVIL LITIGATION AND THE POLICE: A Method of Communication *by* **Robert H. Sulnick.** The author shows how civil litigation can be used as a form of communication to elicit institutional policy changes. *'76, $13.75*

Orders with remittance sent, on approval, postpaid

301-327 East Lawrence Avenue • Springfield • Illinois • 62717

GOODYEAR
Sociology for the 80's

AMERICAN SOCIAL PROBLEMS: AN INSTITUTIONAL VIEW
W.G. Steglich, University of Oklahoma
Margaret Konz Snooks
A macro approach to the fundamental problems of society with more concrete examples than any comparable text.
627 pages 7½ x 9½ 1980

URBANIZED SOCIETY
Lee Taylor, University of Texas
All the basics of urban sociology with an international scope, urban planning data, and applications for practitioners.
338 pages 6 x 9 1980

For examination copies please write to Christopher Weyn, Department 80-54, Goodyear Publishing Company, P.O. Box 2113, Santa Monica, California 90401. Please state your course title, the book you're now using, and approximate enrollment figures.

• UNIVERSITY OF GEORGIA PRESS • THE UNIVERSITY OF GEORGIA PRESS •

UNIVERSITY OF GEORGIA PRESS • THE UNIVERSITY OF GEORGIA PRESS • THE UNIVERSITY OF GEORGIA PRESS •

Conflict and Conflict Management

JOSEPH S. HIMES

In a book that will have wide classroom use, Mr. Himes provides an overview of social conflict and its management. Drawing from social science literature, theory, and research, he effectively demonstrates that contemporary social science can provide answers to the most often asked questions about conflict, its causes, its consequences, its prevention, and its prediction. $23.00 *cloth*, $9.00 *paper*

The Concept of Ideology

JORGE LARRAIN

Foreword by Tom Bottomore

The problem of ideology is at the center of many disciplines and interests within the social sciences. Mr. Larrain deals in a comprehensive and original way with the concept and makes important critical comments on a number of problems that are at the center of recent discussions of social theory, especially Marxist theory—among them ideology and science, ideology and linguistic analysis, and the controversy between historicism and structuralism. $18.00

Ethnic Soldiers

State Security in Divided Societies

CYNTHIA H. ENLOE

Professor Enloe investigates an unusual and highly important theme: the relationship between military organization and racial or ethnic identities. Her ethnic perspective on this subject represents a completely new approach to the problem of internal state security and the mobilization of armed forces. Yet the reader of *Ethnic Soldiers* will quickly realize how crucial this relationship is and wonder that it has been so long neglected. The recent renaissance of ethnic sentiments and loyalties coupled with the serious attention being given by scholars to ethnic politics makes this book of immediate concern to a wide spectrum of anthropologists, sociologists, political scientists, philosophers, and historians. $19.00

THE
UNIVERSITY
OF GEORGIA PRESS
Athens 30602

UNIVERSITY OF GEORGIA PRESS • THE UNIVERSITY OF GEORGIA PRESS • THE UNIVERSITY OF GEORGIA PRESS

THE UNIVERSITY OF GEORGIA PRESS • THE UNIVERSITY OF GEORGIA PRESS •

New from Princeton

SOCIAL ORIGINS OF THE IRISH LAND WAR

Samuel Clark

From 1879-1882, thousands of Irish agricultural tenants revolted against their landlords and set in motion the two decades of political struggle that eventually resulted in the demise of Irish landlordism. This thorough and convincing study examines the most formidable challenge to established authority that emerged in Ireland between the Rebellion of 1798 and the Revolution of 1916-1921. Cloth, $25.00. Limited Paperback Edition, $8.95

AFRICAN BUSINESSMEN AND DEVELOPMENT IN ZAMBIA

Andrew A. Beveridge and Anthony R. Oberschall

Many new African states have sought to reduce foreign control of their economies and have created unprecedented business opportunities for black Africans; yet these new entrepreneurs have received little scholarly attention. Beveridge and Oberschall examine the rapid growth of the indigenous African business sector in Zambia, and systematically link this growth to the country's social structure and its "dependent" economic status. $22.50

THE CENTRALIST TRADITION OF LATIN AMERICA

Claudio Véliz

Surveying some five hundred years of history, this intelligent study shows why the European tradition of development cannot apply to the efforts to reform, modernize, or revolutionize Latin America, and offers a coherent appreciation of the centralist tradition that has shaped Latin American society. Cloth, $22.50. Limited Paperback Edition, $9.75

RELUCTANT FEMINISTS IN GERMAN SOCIAL DEMOCRACY, 1885-1917

Jean H. Quataert

Examining the convergence of socialism and feminism in the German labor movement around the turn of the century, Jean Quataert probes the competing identities and loyalties of class and sex and the problems their adherents faced in reconciling the two. By focusing on the women's movement in particular, she expands our understanding of the German Social Democratic subculture and shows that socialist feminism was far more important than previously recognized. Illus. $18.50

PALESTINIAN SOCIETY AND POLITICS
Joel S. Migdal
Relying principally on archival sources and interviews, and using a variety of methodologies and analytic perspectives, the contributors to this volume focus on the changing nature of the Arab population in Palestine and the leadership to which that population looks. *Written under the auspices of the Center for International Affairs, Harvard University*
Cloth, $20.00 Paper, $6.95

SOMEONE, NO ONE
AN ESSAY ON INDIVIDUALITY
Kenelm Burridge
As categories of moral action, the individual and individuality lie at the center of Western civilization. Kenelm Burridge asks if this reflects the development of a universal in human experience, or if in some sense it is tied to a specific cultural tradition. Through the use of ethnographic information in what previously has been an exclusively philosophical discussion, he seeks the roots of Western individuality in cross-cultural and historical contexts. Whereas most studies of individuality begin with psychodynamics, this one examines the ways in which social processes become internalized and imbedded within the self.
$17.50

Princeton University Press
Princeton, New Jersey 08540

Community Power and Political Theory
A Further Look at Problems of Evidence and Inference
Second, enlarged edition
Nelson W. Polsby
Since 1963, when the first edition of this book was published, the field of community power studies has flourished in a variety of ways. In this new edition, Nelson Polsby addresses some of the central issues in philosophy of social science, theory, and methodology engaged by this literature over the last fifteen years.

"**A classic study has been made even greater.** I know of no other single work that embodies the theoretical, the empirical and the methodological in as creative a fashion in the social sciences as does this second edition of *Community Power and Political Theory*. It is Polsby at his best."
—**Robert Nisbet**
Cloth $22.50 Paper $5.95

Also available

Race and State in Capitalist Development
Comparative Perspectives
Stanley B. Greenberg
Cloth $40.00 Paper $11.50

Arrow's Theorem: The Paradox of Social Choice
A Case Study in the Philosophy of Economics
Alfred F. MacKay
$14.50

Yale
Yale University Press
New Haven and London

Central Library

INTERNATIONAL POPULATION CENSUS BIBLIOGRAPHY

Revision and Update, 1945-1977

(Texas Bibliography II)

Compiled by DOREEN S. GOYER

A Volume in the STUDIES IN POPULATION Series

The original *International Population Census Bibliography* (six volumes and one supplement) contained over 14,000 citations of publications from the first census to those taken in the mid 1960's of about 200 countries and territories. The *Revision and Update* has over 13,000 citations of reports from population censuses taken from 1945 through 1977, almost as many publications in the last thirty-two years as there were in the over 200-year period preceding. No other reference work in the field duplicates exactly the scope of these publications. The *Revision and Update* is not just a bibliography that extends the coverage of the original IPCB to the present; it is a reference tool to aid the researcher and guide the librarian through the maze of census publications. For the researcher it presents the results of a country's census production in a logical manner using familiar bibliographic descriptions. It tells where a copy of almost every item listed may be found in the United States for consultation in person or via interlibrary loan. On the surface it may seem to have many more conveniences for the librarian; but if the librarian has control of the literature, the researcher reaps the benefits.

The Table of Contents contains a list of the 211 countries and territories covered in the volume.

1980, 592 pp., $39.50 ISBN: 0-12-294380-5

SOCIOLOGICAL DILEMMAS

Toward a Dialectic Paradigm

By PIOTR SZTOMPKA

CONTENTS: ON THE THEORETICAL CRISIS OF SOCIOLOGY. Critique of Sociology: Traditions and Perspectives. THE DILEMMAS OF DEMARCATION—METHODOLOGICAL. Science of Humanities: Naturalism, Antinaturalism, Integralism. Science of Man or Science of Society: Reductionism, Antireductionism, Separatism. THE DILEMMAS OF SOCIAL COGNITION—EPISTEMOLOGICAL. Knowledge or Action: Cognitivism, Activism, Constructivism. Detachment or Bias: Neutralism, Axiologism, Commitment. Neutralism and Axiologism in the Sociological Tradition. THE DILEMMAS OF SOCIAL REALITY—ONTOLOGICAL. Man as Object or Subject: Passivism, Autonomism, Creativism. Society as a Whole or an Aggregate: Collectivism, Individualism, Structuralism. OUT OF THE THEORETICAL CRISIS. Recapitulation.

References. Author Index. Subject Index.

1979, 384 pp., $19.50 ISBN: 0-12-681860-6

EQUALITY, JUSTICE AND RECTIFICATION

An Exploration in Normative Sociology

By DEREK L. PHILLIPS

CONTENTS: EQUALITY. The Sociology of Inequality. Equality: Pro and Con. Equality and Social Justice. JUSTICE AND PROPERTY RIGHTS. Property Rights: Locke, Nozick, and the "New Property" Theorists. Rights: An Introduction. Natural and Emergent Rights. The Unanimity Problems. Property Rights. THE RECTIFICATION OF INJUSTICES. Rectification: Problems and Prospects. Rectification Programs and Their Justification.' Index.

1979, 336 pp., $28.00 ISBN: 0-12-554350-6

Continuation Orders authorize us to ship and bill each volume in a series, or "Advances" type publication automatically, immediately upon publication. This order will remain in effect until cancelled. Specify the volume number or title with which your order is to begin.

Two volumes in STUDIES ON LAW AND SOCIAL CONTROL
Series Editor: DONALD BLACK

DISPUTES AND NEGOTIATIONS

A Cross-Cultural Perspective

By P. H. GULLIVER

CHAPTER HEADINGS: The Process of Negotiation. The Study of Joint Decision-Making. Some Basic Concepts for the Study of Negotiation. Processual Model of Negotiation, 1: The Cyclical Model—Information Exchange and Learning. Processual Model of Negotiation, 2: The Developmental Model. The Dynamics of Negotiation. Mediators: Triadic Interaction in the Negotiation Process. Two Case Studies. Conclusion. References. Subject Index.
1979, 320 pp., $19.00 ISBN: 0-12-305550-4

THE IMPOSITION OF LAW

Edited by SANDRA B. BURMAN and BARBARA E. HARRELL-BOND

CHAPTER HEADINGS: *S. B. Burman and B. E. Harrell-Bond,* Introduction. *S. M. A. Lloyd-Bostock,* Explaining Compliance with Imposed Law. *V. Aubert,* On Methods of Legal Influence. *H. A. Bedau,* The Death Penalty in the United States: Imposed Law and the Role of Moral Elites. *F. Svensson,* Imposed Law and the Manipulation of Identity: The American Indian Case. *N. Forer,* The Imposed Wardship of American Indian Tribes: A Case Study of the Prairie Band Potawatomi. *P. Fitzpatrick and L. Blaxter,* Imposed Law in the Containment of Papua New Guinea Economic Ventures. *L. Pospisil,* Legally Induced Culture Change in New Guinea. *H. W. O. Okoth-Ogendo,* The Imposition of Property Law in Kenya. *R. L. Abel,* Western Courts in Non-Western Settings: Patterns of Court Use in Colonial and Neo-Colonial Africa. *J. T. Thomson,* Capitation in Colonial and Post-Colonial Niger: Analysis of the Effects of an Imposed Head-Tax System on Rural Political Organization. *A. Sajó,* Compulsory Prosperity: Some Effects of the Regulation of Farmers' Cooperatives in Hungary. *S. D. Anderman,* Attempts to Impose Legal Restrictions on Trade Unions in Britain 1968–1974. *G. R. Rubin,* Wartime Industrial-Relations Legislation and Legal Institutions and Procedures: The British Munitions of War Acts, 1915–1917. *A. Howkins,* Economic Crime and Class Law: Poaching and The Game Laws, 1840–1880. *R. L. Kidder,* Toward an Integrated Theory of Imposed Law. Index.
References appear at the end of each chapter.
1979, 336 pp., $22.00 ISBN: 0-12-145450-9

PASHUKANIS: Selected Writings on Marxism and Law

Edited by PIERS BEIRNE and ROBERT SHARLET
A Volume in the LAW, STATE AND SOCIETY Series
Edited by Z. BANKOWSKI, M. CAIN, M. McINTOSH and W. CHAMBLISS
Translated by PETER B. MAGGS

CONTENTS: Editor's note. Foreword by *J. N. Hazard.* Introduction by *P. Beirne and R. Sharlet.* The General Theory of Law and Marxism 1st Edition 1924. Lenin and Problems of Law 1925. Encyclopaedia of State and Law 1925-1927. a) Leon Duguit b) International Law c) Object of Law. The Marxist Theory of Law and the Construction of Socialism 1927. Economics and Legal Regulation 1929. The Marxist Theory of State and Law 1932. A Course on Soviet Economic Law (Chap. 1 with L. Ia. Gintsburg 1935). State and Law under Socialism 1936. Notes. Selected Bibliography of E. B. Pashukanis. Author Index. Subject Index.
1979, 392 pp., $41.50 ISBN: 0-12-086350-2

Send payment with order and save postage and handling charge.
Prices are subject to change without notice.

Academic Press, Inc.
A Subsidiary of Harcourt Brace Jovanovich, Publishers
111 FIFTH AVENUE, NEW YORK, N.Y. 10003/24-28 OVAL ROAD, LONDON NW1 7DX

ESSAYS IN MEDICAL SOCIOLOGY
Journeys Into the Field

Renee C. Fox, University of Pennsylvania

This outstanding collection of essays explores the complex interrelationships between medicine, science, religion, and magic, set in a variety of medical settings in the United States, Belgium, and Zaire.

(1-27040-7)/1979/548pp./$19.95

DEATH AND EXISTENCE
A Conceptual History of Human Mortality

James P. Carse, New York University

A systematic survey of ten distinct conceptions of death as they are found in the world's major religious and philosophic traditions, illuminating the questions of contemporary persons about the nature of their own mortality. (1-13704-9)

February 1980/approx. 480pp./ $22.50 (tent.)

WELFARE OF THE ELDERLY An Economic Analysis and Policy Prescription

W. Kip Viscusi, Northwestern University

A systematic analysis of the economic needs of the aged, the motivations for government intervention, the government policies that affect the well-being of the aged, and policy reforms that can promote the welfare of the elderly.

(1-01506-7)/1979/251pp./$18.95

TRUST AND POWER

Miklas Luhmann, University of Bielefeld, German Federal Republic

One of Germany's leading social theorists offers a novel, theoretical treatment of these two social relationships, examining their wide-ranging significance in social, political, and economic life.

(1-99758-7)/1980/228pp./$28.95

Rare & Broad Insights & Perspectives.

THEORIES OF ORGANIZATIONS
Form, Process, and Transformation

Jerald Hage, University of Maryland

A systematic statement about organizations, including how the organizational form maintains itself, how it changes, and what factors are involved. The text approaches the problem from the perspectives of power, change, human factors, resources, and the environment. (1-33859-1)

April 1980/approx. 435pp./ $19.95 (tent.)

WILEY-INTERSCIENCE
a division of John Wiley & Sons, Inc.
605 Third Avenue/New York, N.Y. 10016
In Canada:
22 Worcester Road, Rexdale, Ontario

FRIENDSHIP AND SOCIAL RELATIONS IN CHILDREN

Edited by Hugh C. Foot & Anthony J. Chapman, both of the University of Wales Institute of Science and Technology, & Jean R. Smith, Whitchurch Hospital, Cardiff

Leading researchers give accounts of the different facets of friendship throughout childhood. Includes discussion of children's relationships in the classroom, their conceptions of their peers, and the development of friendship cliques. (1-27628-6)

April 1980/approx. 384pp./$34.00 (tent.)

Available at your bookstore or write to
John V. Storojev, Dept. 7565
Prices subject to change without notice.
0-7565

FIND THEM IN SOCIOLOGY BOOKS FROM WILEY-INTERSCIENCE

New from Columbia

Man in the Age of Technology

ARNOLD GEHLEN, TRANSLATED BY PATRICIA LIPSCOMB. With a Foreword by Peter L. Berger. Probing the crisis of human personality in our century, *Man in the Age of Technology* traces the dizzying social after-effects resulting from the loss of the institutions that grew out of and sustained an earlier, agrarian age. First published in Germany in 1949 and translated here for the first time, this seminal work by Arnold Gehlen reflects his lifelong concern with the need of man to stabilize his life by constructing his own world. $15.00

Programmed to Learn

An Essay on the Evolution of Culture

H. RONALD PULLIAM AND CHRISTOPHER DUNFORD. This spirited analysis of the evolution of behavior provides a stimulating new view of the central role of learning in the origin and maintenance of cultures. Offering an alternative approach to the currently controversial topics in sociobiology, the authors argue that cultural evolution owes its origins and rules to genetic evolution, but has a momentum all its own. "Timely, accessible, and very readable."—*Library Journal* $10.00

Announcing:

The Age of Structuralism

Lévi-Strauss to Foucault

EDITH KURZWEIL. This lucid study of the sociological, political, and philosophical currents of structuralism offers introductions to eight prominent French thinkers—five who fueled the movement and three who provided intellectual opposition. Examining the work of Claude Lévi-Strauss, Louis Althusser, Roland Barthes, Jacques Lacan, Michel Foucault, Henri Lefèbvre, Alain Touraine, and Paul Ricoeur, *The Age of Structuralism* provides a lively and clear portrait of a movement whose impact still reverberates today. $20.00 cloth, $6.95 paper

To order send check or money order to Dept. JN at the address below, including $1.30 per order for postage and handling.

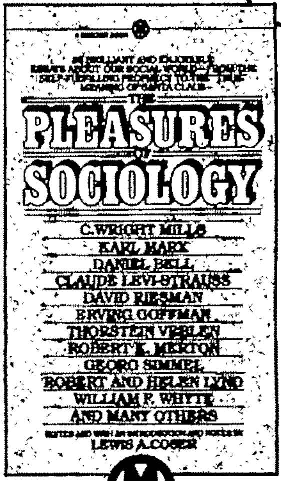
COLUMBIA UNIVERSITY PRESS
Address for orders:
136 South Broadway, Irvington, New York 10533

First Encounters with Eminent Social Thinkers

THE PLEASURES OF SOCIOLOGY is a refreshing, original introduction to sociology under the editorship of Lewis A. Coser, noted author and teacher.

Its 36 remarkable essays include:

- Social "truths" that are really lies
- The dynamics of crime in America
- Labor and alienation
- The conspicuous consumption of the moneyed class
- The sexual code of the slums
- The social roles of the stranger and the fool
- The pioneering lifestyle of modern urban "night people"
- and much, much more



LIMITED SUPPLY OF EXAMINATION COPIES AVAILABLE. To order, make your request on school letterhead indicating course title and estimated enrollment. Send $.50 per copy, checks or money orders only (no cash or C.O.D.'s accepted) to:

NAL, P.O. Box 888, Bergenfield, N.J. 07621

(Allow 4 to 6 weeks for delivery.)

Mentor Original
ME 1825/$2.95

NAL

New American Library
Education
1633 Broadway, New York, N.Y. 10019

JOURNALS PUBLISHED BY THE AMERICAN SOCIOLOGICAL ASSOCIATION

AMERICAN SOCIOLOGICAL REVIEW
(Bi-monthly—ISSN 0003-1224 Rita J. Simon, *Editor)*
(Members, $10; Non-members, $15; Institutions, $30; Single issues, $4)

CONTEMPORARY SOCIOLOGY: A Journal of Reviews
(Bi-monthly—ISSN 0094-3061 Norval D. Glenn, *Editor)*
(Members, $10; Non-members, $15; Institutions, $20; Single issues, $4)

JOURNAL OF HEALTH & SOCIAL BEHAVIOR
(Quarterly—ISSN 0022-1465 Howard B. Kaplan, *Editor)*

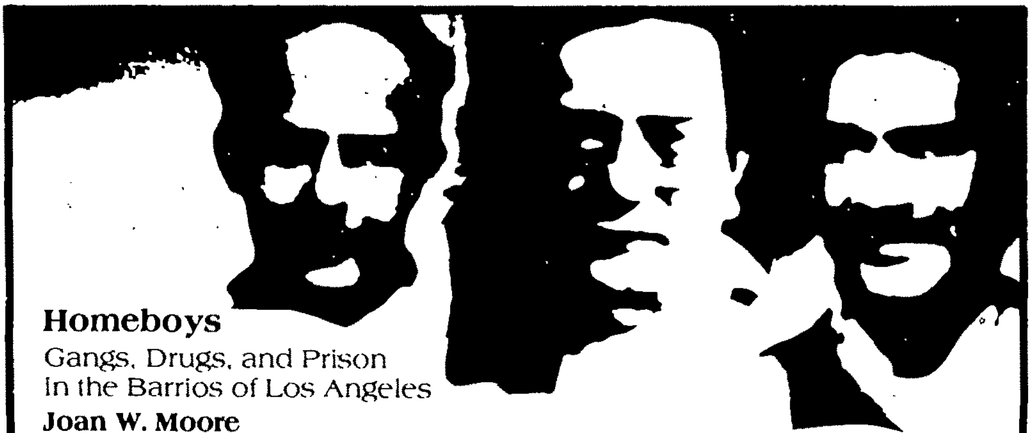
SOCIAL PSYCHOLOGY QUARTERLY
(Quarterly—ISSN 0190-2725 George W. Bohrnstedt, *Editor)*

THE AMERICAN SOCIOLOGIST
(Quarterly—ISSN 0003-1232 James L. McCartney, *Editor)*

SOCIOLOGY OF EDUCATION: A Journal of Research in Socialization & Social Structure
(Quarterly—ISSN 0038-0407 Alan C. Kerckhoff, *Editor)*

Subscription rates for quarterly journals: Members, $8; Non-members, $12; Institutions, $16; Single issues, $4

Order from: American Sociological Association
1722 N Street, N.W. • Washington, D.C. 20036



Homeboys

Gangs, Drugs, and Prison in the Barrios of Los Angeles

Joan W. Moore

Sidney Spivack Fellowship Award winner Joan W. Moore, in collaboration with Chicago pintos (ex-convicts) and community workers, examines the world of the Los Angeles barrios.

"Theoretically illuminating and highly important for policy-makers—if they will listen."—R. A. Schermerhorn

240 pp. 3 maps 1979
cloth ISBN 121-9 $15.00
paper ISBN 114-6 $7.95

Equal Employment Policy for Women

Strategies for Implementation in the United States, Canada, and Western Europe

Edited by **Ronnie Steinberg Ratner**

The first comparative discussion of equal pay and equal opportunity for women in an international context, these original essays assess problems and progress in such areas as legislation, collective bargaining, training, and organization.

544 pp. 1980 ISBN 156-1 $25.00

Inventing the American Way of Death, 1830-1920

James J. Farrell

Approx. 250 pp. June ISBN 180-4 $17.50
In the series, **American Civilization,** edited by **Allen F. Davis.**

From Suburb to Shtetl

The Jews of Boro Park

Egon Mayer

This book is a compassionate examination of the transformation of a community in Brooklyn, New York, from a middle-class suburban neighborhood to the most important Orthodox Jewish community in the United States.

208 pp. 7 illus. 1979 ISBN 161-8 $15.00

Black Testimony

Voices of Britain's West Indians

Thomas J. Cottle

The result of two years spent talking with members of the black community in London, this study chronicles their feelings about themselves, their homes, families, education, work and unemployment, immigration to England, and, particularly, their feelings about their white fellow-citizens.

84 pp. 1980 ISBN 186-3 $12.50 trade

The Resurgence of Race

Black Social Theory from Reconstruction to the Pan-African Conferences

William Toll

288 pp. 1979 ISBN 167-7 $19.50

Publisher's ISBN prefix is 0-87722.

Temple University Press Broad & Oxford Sts. Philadelphia 19122

New from Human Sciences Press

QUALITATIVE SOCIOLOGY

Editors: Barry Glassner, Ph.D. and
Derral Cheatwood, Ph.D.

This journal has been recognized throughout the social sciences as a leading source devoted to the qualitative interpretation of social life. A broad spectrum of topics and research methods are featured including theoretical essays, fieldwork studies, discussions of the philosophy of social science, photographic studies, and qualitative interpretations of quantitative data. Specific subjects which have been examined include the theories of Weber, Parsons, Marx, and other prominent social scientists.

ISSN 0162-0436
Quarterly Vol. 3, 1980
Order 538-6 Individuals $18.00
Order 539-4 Institutions $38.00

Doty, Pamela, Ph.D.

GUIDED CHANGE OF THE AMERICAN HEALTH SYSTEM

Where the Levers Are

This book provides an in-depth analysis of the processes and mechanisms of deliberate and constructive social change. Specifically, four proposals for reforming some aspect of the American health system — each developed by a special study group, commission or task force — are examined. Implicit in each of the proposals is an alternative theoretical perspective for effecting social change. Each plan, therefore, focuses on a different set of variables to provide the "levers" for change.

1980/June 272 pp. (approx.)
0-87705-472-X $22.95

Robin, Stanley S., Ph.D. and
Morton O. Wagenfeld, Ph.D., Editors

PARAPROFESSIONALS IN THE HUMAN SERVICES

Volume VI, Community Psychology Series

This work provides a critical assessment of the effectiveness of paraprofessionals in the human services, and examines the unique problems and potentials of this burgeoning work force. It begins with an historical analysis, and then explores economic, training and recruitment considerations. Various essays document the extent of paraprofessional employment, the nature of the services rendered, and the quality of the paraprofessionals' performance. The authors discuss problems in the establishment of training programs, and difficulties and strategies for research and evaluation of paraprofessional programs.

1980/Aug. 320 pp. (approx.)
0-87705-490-8 $26.95

Tefft, Stanton K., Ph.D., Editor

SECRECY

A Cross-Cultural Perspective

"It offers a broad and thoughtful set of materials which goes far beyond any collection currently available to social scientists. It is only by asking the kinds of analytic and comparative questions raised by the authors that we can begin to understand and intelligently respond to the many issues raised by secrecy." —Gary P. Marx, Ph.D.
Massachusetts Institute of Technology

"This book provides a fascinating combination of theoretical and empirical articles. . . an indispensable work." —E. Pendleton Banks, Ph.D.

1980/Jan. 400 pp. (approx.)
0-87705-442-8 Cloth $19.95
0-87705-443-6 Paper $ 9.95

Kraus, William A., Ph.D.

COLLABORATION IN ORGANIZATIONS

Alternatives to Hierarchy

Most organizations and social systems in Western society utilize hierarchical structures which promote competition as a major value system and an individual behavior pattern. This challenging book introduces the concept and application of collaboration as an alternative value system and organizational structure. The collaborative model reinforces individual development and contribution, and provides a viable framework for organizing a social system.

1980/May 240 pp. (approx.)
0-87705-491-6 $16.95

Humphrey, John, Ph.D. and
Michael Milakovich, Ph.D.

THE ADMINISTRATION OF JUSTICE

Law Enforcement, Courts, and Corrections

This comprehensive text provides students with an overview of the organization and operations of the three major criminal justice agencies: law enforcement, courts, and correctional facilities. The authors examine the relationships among the agencies, and describe the general procedures of each agency at the local, state and federal levels. For those who are considering criminal justice careers, the text explains the roles and responsibilities of professionals within each of the agencies.

1980/Feb. 272 pp. (approx.)
0-87705-448-7 Cloth $22.95
0-87705-449-5 Paper $12.95

[illegible]

HUMAN SCIENCES PRESS
72 Fifth Avenue ● 3 Henrietta Street
NEW YORK, NY 10011 ● LONDON, WC2E 8LU

[illegible] Human Sciences Press [illegible] MONEY-BACK GUARANTEE and RETURN PRIVILEGES

CHICAGO

Since 1891, Publishers of Scholarly Books and Journals

Machine Politics in Transition
Party and Community in Chicago
Thomas M. Guterbock

For three years, Thomas M. Guterbock participated in the daily activities of the Regular Democratic Organization in one North Side Chicago ward in order to discover how political machines win the support of the urban electorate.

Cloth 352 pages $20.00 March

La Chicana
The Mexican-American Woman
Alfredo Mirandé and
Evangelina Enríquez

Very little is known about the contributions or the history of Chicanas. They experience multiple forms of oppression as women, as members of a colonized group, and as members of a culture perceived to be dominated by males. *La Chicana* offers a definitive study of this long neglected group.

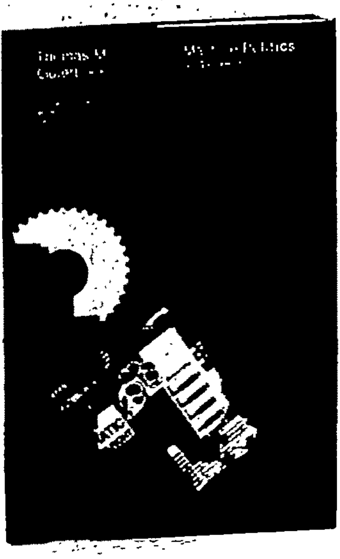
Cloth 281 pages Illus. $19.50. Available

Alexis de Tocqueville
On Democracy, Revolution and Society
Edited by John Stone and
Stephen Mennell

This collection is the first to include excerpts from Tocqueville's two acknowledged masterpieces, *Democracy in America* and *The Ancien Régime and the French Revolution*, as well as excerpts from his extensive correspondence, notebooks, the *Recollections*, and the government reports and newspaper articles that he wrote while a member of France's Chamber of Deputies.

Heritage of Sociology series, edited by Morris Janowitz

Cloth 408 pages $27.50 April



Before the Convention
A Theory of Presidential Nomination Campaigns
John A. Aldrich

Political scientist John Aldrich presents a systematic analysis of presidential nomination politics, based on application of rational-choice models to candidate behavior. He seeks to determine why and how candidates choose to run,, why some succeed and others fail, and to determine what consequences the nomination process has for the general election and, later, for the president in office.

Paper 312 pages $7.95 April
Also available in cloth.

The
University of Chicago
Press
Chicago 60637

Superlative texts to build firm foundations

✶ **William F. Kenkel**
SOCIETY IN ACTION: Introduction to Sociology
Second Edition
Instructor's Manual. Study Guide and Test Bank also available. 624 pages (tentative). February 1980. ISBN 0-06-384486-9.

✶ **Howard J. Sherman & James Wood**
SOCIOLOGY: Traditional and Radical Perspectives
Paper. Instructor's Manual. 413 pages. August 1979. ISBN 0-06-046107-1.

✶ **James William Coleman & Donald R. Cressey**
SOCIAL PROBLEMS
Instructor's Manual. 583 pages. January 1980. ISBN 0-06-041326-3.

✶ **David A. Karp & William C. Yoels**
SYMBOLS, SELVES, AND SOCIETY: Understanding Interaction
Paper. 212 pages. 1979. ISBN 397-47395-8. *J. B. Lippincott.*

✶ **Edwin M. Schur**
INTERPRETING DEVIANCE: A Sociological Introduction
539 pages. August 1979. ISBN 0-06-045811-9.

✶ **William R. Rosengren**
THE SOCIOLOGY OF MEDICINE: Diversity, Conflict, and Change
446 pages. October 1979. ISBN 0-06-045582-9

6 NOV 1980

✶ **Delbert C. Miller & William H. Form**
INDUSTRIAL SOCIOLOGY: Work in Organizational Life
Third Edition
834 pages. 1980. ISBN 0-06-04447-1.

✶ **Chad Gordon & Gayle Johnson**
READINGS IN HUMAN SEXUALITY: Contemporary Perspectives
Second Edition
Paper. 400 pages (tentative). March 1980. ISBN 0-06-042399-4.

✶ **John A. Perry & Erna K. Perry**
CONTEMPORARY SOCIETY: An Introduction to Social Science
Third Edition
Paper. Instructor's Manual. Study Guide available. 624 pages (tentative). February 1980. ISBN 0-06-045135-1.

Harper & Row
New York • San Francisco

To request examination copies, write to Suite 5D (883), Harper & Row, Publishers, 10 East 53d Street, New York, N.Y. 10022. Please include course title, enrollment, and present text.

American Sociological Review

The Boston Anti-Busing Movement 357
Bert Useem

Comparative Measurement of Political Democracy 370
Kenneth A. Bollen

Organizational Properties from Aggregate Data 391
James R. Lincoln and Gerald Zeitz

Social Behaviorism and Symbolic Interactionism 409
Herbert Blumer

Ex Cathedra Blumer or *Ex Libris* Mead? 420
Clark McPhail and Cynthia Rexroat

Education and Fertility . . . Roles Women Occupy 431
Ronald R. Rindfuss, Larry Bumpass, and Craig St. John

Structural Intervention and Differential Association 448
D. A. Andrews

Social Class and Psychological Distress 463
Ronald C. Kessler and Paul D. Cleary

Access to Interorganizational Networks 479
Jon Miller

Research Note
Sociological Analyses of Income Inequality 497
Erich Weede

Comments 502
Charles J. Morgan, Lois M. Verbrugge, David Mechanic, and Ronnie Steinberg Ratner and Paul Burstein

Volume 45 June 1980 Number 3

Notice to Contributors

A processing fee of $10 is required for each paper submitted, except for comments; such fees will be waived for student members of ASA. A check or money order, made payable to the American Sociological Association, should accompany each submission. The fee must be paid in order for the review process to begin.

The length of an *ASR* paper is generally ten (10) printed pages or thirty (30) typed pages. However, the *ASR* is happy to accept manuscripts up to fifty (50) typed pages in length for review. We also are interested in receiving manuscripts for a Research and Notes Section.

To permit anonymity in the review of manuscripts, keep identifying material out of the manuscript. Attach a cover page giving authorship, institutional affiliation and acknowledgments, and provide only the title as identification on the manuscript and abstract.

ASA Multiple Submissions Policy: Submission of a manuscript to a professional journal clearly implies commitment to publish in that journal. The competition for journal space requires a great deal of time and effort on the part of editorial readers whose only compensation for this service is the opportunity to read papers prior to publication and the gratification associated with discharge of professional obligation. For these reasons, *the American Sociological Association regards submission of a manuscript to a professional journal while that paper is under review by another journal as unacceptable.*

SUBMISSION AND PREPARATION OF MANUSCRIPTS

1. Submit four (4) copies and retain the original for your files. Copies may be Xerox, mimeograph or multilith, but not carbons.
2. Enclose a stamped self-addressed postcard for acknowledgment of manuscript receipt. *Manuscripts will not be returned.* Please do not send your original copy.
3. All copy must be typed, doublespaced (including indented material, footnotes and references) on 8½ by 11 inch white opaque paper. Lines must not exceed six (6) inches. Margins must be a minimum of one inch.
4. Include four (4) copies of an abstract of no more than 150 words.
5. Type each table on a separate page. Insert a location note at the appropriate place in the text; e.g., "Table 2 about here." Send copies, retain originals.
6. Figures must be drawn in India ink on white paper. Send copies, retain originals.
7. Clarify all symbols with notes in the margins of the manuscript. Circle these and all other explanatory notes not intended for printing.
8. Footnotes should not be placed in the body of the text. Type them (doublespaced) and attach them as a separate appendix to the text. Number them consecutively throughout the text. Footnotes are to be kept to a minimum and used only for substantive observations. Source citations are made within the text rather than in footnotes.
9. Acknowledgments, credits and grant numbers are placed on the title page with an asterisk.

REFERENCE FORMAT

A. *In the text:* All source references are to be identified at the appropriate point in the text by the last name of the author, year of publication and pagination where needed. Identify subsequent citations of the same source in the same way as the first, not using *"ibid.," "op. cit.," or "loc. cit."* Examples follow:

1. If author's name is in the text, follow it with year in parentheses. [". . . Duncan (1959) . . ."]
2. If author's name is not in the text, insert, in parentheses, the last name and year, separated by a comma. [". . . (cf. Gouldner, 1963) . . ."]
3. Pagination (without "p." or "pp.") follows year of publication after a colon. [". . . Kuhn (1970:71)."]
4. Give both last names for dual authors; for more than two use "et al." in the text. When two authors have the same last name, use identifying initials in the text. For institutional authorship, supply minimum identification from the beginning of the complete citation. [". . . (U.S. Bureau of the Census, 1963:117) . . ."]
5. Separate a series of references with semicolons and enclose them within a single pair of parentheses. [". . . (Burgess, 1968; Marwell et al., 1971; Cohen, 1962) . . ."]

B. *In the appendix:* List all source citations by author, and within author by year of publication, in an appendix titled "References." The reference appendix must be complete and include all references in the text. The use of "et al." is not acceptable in the appendix; list the names of all authors. (See A. 4. for text format.)

If there is more than one reference to the same author and year, distinguish them by the letters, a, b, etc. added to the year. [". . . Levy (1965a:331) . . ."] Give the publisher's name in as brief a form as is fully intelligible. For example, John A. Wiley and Sons should be "Wiley." If the cited material is unpublished, use "forthcoming" with name of journal or publisher; otherwise use "unpublished."

Use no underlining, italics or abbreviations.

Examples follow:

1. *Books:* Jud, Gerald J., Edgar W. Mills, Jr. and Genevieve Walters Burch
   1970 Ex-Pastors. Philadelphia: Pilgrim Press.
   U.S. Bureau of the Census
   1960 Characteristics of Population. Volume 1. Washington, D.C.: U.S. Government Printing Office.
   Bernard, Claude
   [1865] 1957 An Introduction to the Study of Experimental Medicine. Tr. Henry Copley Greene, New York: Dover.
2. *Periodicals:* Conger, Rand
   Forthcoming "The effects of positive feedback on direction and amount of verbalization in a social setting." Pacific Sociological Review.
   Merton, Robert K.
   1963a "The ambivalence of scientists." Bulletin of The Johns Hopkins Hospital 112:77–97.
   1963b "Resistance to the systematic study of multiple discoveries in science." European Journal of Sociology 4:237–82.
3. *Collections:* Davie, M.
   1938 "The pattern of urban growth." Pp. 133–61 in G. Murdock (ed.), Studies in the Science of Society. New Haven: Yale University Press.

See recent issues for further examples.

Revised 1980 ASR

AMERICAN SOCIOLOGICAL REVIEW

Official Journal of the American Sociological Association

JUNE, 1980 VOLUME 45, NUMBER 3

ARTICLES

Solidarity Model, Breakdown Model, and the Boston Anti-Busing Movement .. Bert Useem 357

Issues in the Comparative Measurement of Political Democracy .... Kenneth A. Bollen 370

Organizational Properties from Aggregate Data: Separating Individual and Structural Effects .... James R. Lincoln and Gerald Zeitz 391

Comment: Mead and Blumer: The Convergent Methodological Perspectives of Social Behaviorism and Symbolic Interactionism .... Herbert Blumer 409

Rejoinder: *Ex Cathedra* Blumer or *Ex Libris* Mead? .... Clark McPhail and Cynthia Rexroat 420

Education and Fertility: Implications for the Roles Women Occupy .... Ronald R. Rindfuss, Larry Bumpass, and Craig St. John 431

Some Experimental Investigations of the Principles of Differential Association through Deliberate Manipulations of the Structure of Service Systems .... D. A. Andrews 448

Social Class and Psychological Distress .... Ronald C. Kessler and Paul D. Cleary 463

Access to Interorganizational Networks As a Professional Resource .... Jon Miller 479

RESEARCH NOTE

Beyond Misspecification in Sociological Analyses of Income Inequality ...... Erich Weede 497

COMMENTS AND REPLIES

Comment on Quadagno, *ASR*, February, 1979.
Darwin, Sociobiology and Sociology .... Charles J. Morgan 502

Reply to Morgan .... Jill S. Quadagno 505

Comment on Walter R. Gove and Michael Hughes, *ASR*, February, 1979
Possible Causes of the Apparent Sex Differences in Physical Health ... Lois M. Verbrugge 507

Comment on Gove and Hughes, *ASR*, February, 1979 .... David Mechanic 513

Reply to Mechanic and Verbrugge
Sex Differences in Physical Health and How Medical Sociologists View Illness .... Walter R. Gove and Michael Hughes 514

Comment on Boli-Bennett and Meyer, *ASR*, December, 1978
Ideology, Specificity, and the Coding of Legal Documents: What Do Measures of Constitutional Content Measure? .... Ronnie Steinberg Ratner and Paul Burstein 522

Reply to Ratner-Burstein
Constitutions as Ideology .... John Boli-Bennett and John W. Meyer 525

OTHER

The Editor's Items 528

Editor: RITA J. SIMON

NEW EDITOR

Effective with the February 1981 issue, William H. Form will become the editor of the American Sociological Review. Beginning July 1, 1980, send all manuscripts to Prof. William H. Form, Department of Sociology, 326 Lincoln Hall, University of Illinois, Urbana, IL 61801.

Deputy Editors: CLARK McPHAIL AND JOE L. SPAETH

Associate Editors: PAUL BURSTEIN, ROBERT EVAN COLE, THOMAS J. FARARO, BERNARD FARBER, DANIEL GLASER, MICHAEL J. HINDELANG, GISELA HINKLE, ROSABETH KANTER, JAMES R. KLUEGEL, JOHN LOFLAND, MURRAY MELBIN, MICHAEL MOCH, ANTHONY OBERSCHALL, VALERIE K. OPPENHEIMER, JEFFREY PFEFFER, ALEJANDRO PORTES, SAMUEL PRESTON, BARBARA RESKIN, RICHARD RUBINSON, CHARLES W. TUCKER, JOHN WILSON, MAYER N. ZALD, LYNNE G. ZUCKER.

Copy Editor: ELIZABETH ROBINSON NEUMANN

Managing Editor: LINNA McDADE

Editorial Assistant: HELEN CURLEY

Executive Officer: RUSSELL R. DYNES

SCOPE AND MISSION OF AMERICAN SOCIOLOGICAL REVIEW: The *American Sociological Review* publishes work of interest to the discipline in general, new theoretical developments, results of research that advance our understanding of the most fundamental social processes, and important methodological innovations. Like other publications of the Association, emphasis is given to exceptional quality. Unlike the more specialized journals of the Association, the *American Sociological Review's* primary focus is on whatever has the most general bearing on the knowledge of society.

Concerning manuscripts, address: Editor, *American Sociological Review*, Department of Sociology, University of Illinois, Urbana, Illinois 61801. (217) 333-6515.

The *American Sociological Review* (ISSN 0003–1224) is published at 49 Sheridan Avenue Albany, New York, bimonthly in February, April, June, August, October, and December.

Applications for permission to quote from this journal should be addressed to the Executive Office

Copyright © 1980 American Sociological Association.

Annual membership dues of the American Sociological Association, $20–$120 ($10 of this amount may cover a subscription to the *American Sociological Review*); Associate, $25; International Associate, $25; Student Associate, $12.

Application for membership and payment of dues should be made to the Executive Office. Subscription rate for members, $10; nonmembers, $15; institutions and libraries, $30; nonmember students, $10. Single issue, $4.

New subscriptions and renewals will be entered on a calendar year basis only.

Change of address: Six weeks advance notice to the Executive Office and old address as well as new are necessary for change of subscriber's address.

Concerning advertising, changes of address, and subscriptions, address: Executive Office, American Sociological Association, 1722 N Street, N.W., Washington, D.C. 20036.

Claims for undelivered copies must be made within the month following the regular month of publication. The publishers will supply the missing copies when losses have been sustained in transit and when the reserve stock will permit.

Second class postage paid at Washington, D.C. and additional mailing offices.

MEMBERS OF THE COUNCIL, 1980

Officers of the Association

PETER H. ROSSI, President
University of Massachusetts, Amherst
HELEN MacGILL HUGHES, Vice President
Cambridge, Massachusetts
WILLIAM FOOTE WHYTE, President-Elect
Cornell University
RENEE C. FOX, Vice President-Elect
University of Pennsylvania
JAMES F. SHORT, JR. Secretary
Washington State University
HERBERT L. COSTNER, Secretary-Elect
University of Washington
HUBERT M. BLALOCK, JR., Past President
University of Washington
RUSSELL R. DYNES, Executive Officer
American Sociological Association

Elected-at-Large

PAULINE B. BART
University of Illinois, Chicago Circle
NORMAN BIRNBAUM
Amherst College
ERNEST Q. CAMPBELL
Vanderbilt University
ARLENE K. DANIELS
Northwestern University
IRWIN DEUTSCHER
University of Akron
WILLIAM GAMSON
University of Michigan
HELENA Z. LOPATA
Loyola University of Chicago
THOMAS F. PETTIGREW
Harvard University
MORRIS ROSENBERG
University of Maryland
IMMANUEL WALLERSTEIN
SUNY, Binghamton
CHARLES V. WILLIE
Harvard University
MAURICE ZEITLIN
University of Cal., Los Angeles

(Articles in the REVIEW are indexed in the Social Sciences Index, Psychological Abstracts, Sociological Abstracts, Ayer's Guide, University Microfilms, Abstracts for Social Workers, International Political Science Abstracts, and United States Political Science Documents.)

AMERICAN SOCIOLOGICAL REVIEW

SOLIDARITY MODEL, BREAKDOWN MODEL, AND THE BOSTON ANTI-BUSING MOVEMENT*

BERT USEEM
Center for Research on Social Organization
University of Michigan

American Sociological Review 1980, Vol. 45 (June:357–369)

"Breakdown theorists" assert that actors who are weakly integrated into their community and experience discontent are likely to join a protest movement. These analysts also maintain that disorganization increases discontent. "Solidarity theorists," on the other hand, argue that isolated individuals are less likely to protest than others and that discontent is unrelated to protest. Data from a survey of Boston residents provide support for the solidarity theorists' argument that social cohesion increases protest. The Boston data, however, support the breakdown theorists' hypothesis that discontented individuals are more likely to protest than others. Finally, the data undercut the breakdown theorists' hypothesis that disorganization increases discontent.

Social movement analysts sharply disagree over the reasons for popular participation in social movements. Some researchers advocate a "solidarity" model while others favor a "breakdown" model. The central hypothesis of the solidarity model is that social solidarity, the integration of individuals into community life, facilitates the mobilization process for two reasons. First, social solidarity furnishes individuals with a communication network, a set of common values and symbols around which members can be mobilized, a tradition of participation in group activity, and an authority structure. On the basis of empirical research, solidarity theorists maintain that, unless the particular conditions associated with social solidarity are present, mobilization is unlikely to take place (Shorter and Tilly, 1974; Tilly, 1978).

Second, solidarity makes "bloc mobilization" possible. Movements can expand either by recruiting individuals one by one or by enlisting entire blocs of previously organized people en masse. Mobilization is made much easier when ongoing movements are able to draw previously established groups into their organization. By definition, bloc mobilization is impossible unless some measure of solidarity exists within a community (Oberschall, 1973:125).

Anthony Oberschall (1978:297–8) summarizes the breakdown model:

> (B)reakdown theory points to the dissolution of traditional social formations and communal solidarities as a result of rapid social change. Social disorganization, demographic pressures, and ecological imbalance lead to the accumulation of strains, frustrations, insecurity, and grievances, and the resulting pressure cooker has a tendency to explode in collective violence and civil disorder.

The breakdown model has two variations (see Figure 1). According to the "mass society" version, individuals are most likely to join social movements when they have few personal ties within a community and a weak sense of identification with that community. Social isolation facilitates recruitment by detaching individuals from the structures that normally channel people into conventional political

---

* Direct all communications to: Bert Useem; Center for Research on Social Organization; University of Michigan; 330 Packard Street; Ann Arbor, MI 48109.

I would like to thank Ellen Messing, Mayer Zald, Robert Leibman, Charles Tilly, and Ronald Kessler for their helpful comments on this paper. This research was supported by grants from the National Science Foundation (No. Soc-6800621) and the Society for the Psychological Study of Social Issues.

1. Breakdown Model
   - A. Mass Society Version (e.g., Kornhauser, Coleman)
     Social Disorganization——→Protest
   - B. Discontent Version (e.g., Piven and Cloward, Gurr)
     Social Disorganization→Discontent→Protest
2. Solidarity Model (e.g., Tilly, Oberschall)
   Solidarity——→Protest

Figure 1. Two Versions of Breakdown Model and Solidarity Model on the Relationship among Social Disorganization/Solidarity, Discontent, and Protest.

activity (Coleman, 1957; 1971; Kornhauser, 1959). Mass society theorists assert that secondary organizations, such as fraternal groups and political clubs, are especially important in preventing people from participating in social movements. Thus, in contrast to the solidarity model, proponents of the mass society model argue that socially isolated individuals are more, not less, likely to become involved in social movements.

A second version of the breakdown argument, called the "discontent model," stresses the significance of changes in a population's level of discontent for explaining participation in social movements (Gurr, 1970; Piven and Cloward, 1977). According to this argument, individuals strongly integrated into their community are normally insufficiently discontented to participate in social movements. Individuals with weak bonds to their community, on the other hand, usually experience heightened levels of discontent. The more discontented an individual, the more likely is he or she to participate in a social movement. Thus, the hypothesized process has two steps: social disorganization increases discontent which, in turn, motivates protest. It should be noted that researchers have been particularly interested in the effects of one form of discontent, "relative deprivation." Relative deprivation refers to the feeling that one is deprived of a desired object, in comparison to other persons not being deprived of it.

Solidarity theorists, by contrast, have generally denied the usefulness of focusing on any form of discontent. They maintain that grievances are sufficiently widespread through all societies at all times that, as a constant,[1] they explain little of the variation in social movement participation (Jenkins and Perrow, 1977:266; Oberschall, 1978:298).[2]

Studies have tested both models. One major effort has been the research associated with Charles Tilly (Lodhi and Tilly, 1973; Rule and Tilly, 1972; Shorter and Tilly, 1974; Snyder and Tilly, 1972; Tilly et al., 1975). The Tilly group has focused on testing a solidarity model explanation of why the large-scale transformations that accompanied industrialization changed the preexisting pattern of collective behavior in several European countries. The research group's central conclusion is that industrialization created a proliferation of new organizations capable of contending for power. The establishment of cities, for example, promoted the growth of community organizations, some of which sought new political rights as they matured. Similarly, factory life created a new form of organization, the union, which frequently took political action.

Other work favorable to the solidarity model has shown that group membership may facilitate protest. These studies have revealed a positive association between membership in secondary and primary groups and participation in the Social Credit party in Quebec, Canada (Pinard, 1971); a civil rights sit-in protest in the early 1960s (Von Eschen et al., 1971); the feminist movement in the late 1960s (Freeman, 1973; 1979); and the Pentecostal (Gerlach and Hine, 1970; Heirich, 1977) and Nichiren Shoshu (Snow, n.d.) religious movements.

Other research, for example that of Piven and Cloward (1977), supports the breakdown model. Piven and Cloward examined four poor people's movements that emerged in the United States during the 1930s and the 1960s. They found that prior to each movement's start, a profound social change had destroyed the "regulatory controls inherent in the structure of institutional life" (p. 10). The destruction of these regulatory mechanisms increased discontent which, in turn, led to mass defiance. The Great Depression, for example, dislodged large numbers of people from the regulatory controls inherent in the work situation. As a result, frustration increased, which led to a movement of the unemployed who engaged in spontaneous street demonstrations, rent and food riots, and disruptions of relief centers (pp. 41–92).

---

[1] It should be noted that Jenkins and Perrow argue that one should assume that discontent is constant "especially" when analyzing "deprived groups" (1977:266). They do not discuss whether this assumption is appropriate when studying nondeprived groups, nor do they provide criteria for distinguishing the two.

[2] One solidarity theorist, Tilly (1978:56–72), has recently reformulated his model so as to include a discontent factor (cf. Tilly, 1975). Still, even in the reformulated version, it is assumed that discontent factors are secondary to social organizational factors in accounting for the variance in the level of collective action.

Also consistent with the breakdown model are several studies of the relationship between discontent and participation in the United States urban revolts of the 1960s. Research on riot participation—in three different cities—by Crawford and Naditch (1970), Caplan and Paige (1968), and Bowen et al. (1968) all showed a positive correlation between relative deprivation and involvement in the rebellions. In contrast, however, McPhail (1971:1064) reanalyzed the data from numerous riot studies and concluded that the discontent explanation "receives scant empirical support."[3]

While there are numerous studies testing either the breakdown model or the solidarity model,[4] few have compared the two models using the same data. One exception is the research conducted by the Tilly group mentioned above. Since their research is based on archival material, however, they are forced to rely on highly indirect measures of several key variables. In one study, for example, Snyder and Tilly (1972) measure relative deprivation based on indices of food prices, prices of manufactured goods, and industrial production. Their central finding is that relative deprivation is unable to explain the year-to-year fluctuation between 1830 and 1960 in the number of collective behavior participants in France. It is unclear, however, whether the several indices of objective conditions did, in fact, tap subjectively perceived deprivation.

Thus, although a central debate in social movement theory is between proponents of the solidarity model and advocates of the breakdown model, the available studies have not clearly established which model is best able to account for protest. Research is unsatisfactory because, first, studies have infrequently compared the explanatory power of the two models with respect to the same data. Second, the few exceptions have relied on highly indirect measures of critical variables.

The purpose of this article is to compare the value of the breakdown and solidarity models in explaining support of and participation in the Boston anti-busing movement. We will address the following questions about popular participation in the anti-busing movement. First, did social solidarity promote or inhibit participation in the anti-busing movement, as would be respectively argued by solidarity and mass society theorists? Second, did discontent contribute to involvement in the anti-busing movement, as theorists using the discontent version of the breakdown model would argue? Or is there no relationship between discontent and participation in the anti-busing movement, as the solidarity theorists would maintain? Finally, did social disorganization increase discontent, as the breakdown theorists would predict?

---

[3] Research using aggregate data on the characteristics of riot cities has also produced contradictory results. Spilerman (1970; 1971; 1976) found that measures of black deprivation cannot account for the riots. In contrast, Morgan and Clark (1973) show that grievance levels are strong predictors of the riots.

[4] Additional studies that support the breakdown model include Smelser's (1962) analysis of collective behavior; Huntington's (1968) analysis of political order and revolution in modernizing societies; Gurr's (1970) and Davies's (1962) application of relative deprivation theory to civil strife and revolution; and explanations by Katznelson (1978) and Miller et al. (1976) of the United States urban riots during the 1960s.

Studies that support the solidarity model include Jenkins and Perrow's (1977) study of the unionization of California farmworkers; Wolf's (1969) and Useem's (1977) analyses of peasant participation in revolutionary movements; and Oberschall's (1973:204–41) explanation of the emergence of the Civil Rights movement in the South.

1. Data

The term "Boston anti-busing movement" is here used to refer to the collective, widespread, but not necessarily organized, effort that had the stated goal of preventing busing for the purpose of school desegregation and which used noninstitutionalized means to achieve its goals. The movement emerged in the fall of 1974 and, over the next four years, it mobilized people to join school boycotts; to support neighborhood "information centers"; to form a city-wide organization, Restore Our Alienated Rights (ROAR); and to participate in dozens of mass demonstrations, some of them violent.

Data for the analysis are drawn from a survey of 468 white Boston residents between the ages of 25 and 53 who were United States citizens.[5] To simplify the analysis and to insure that an adequate number of anti-busing activists were in the sample, interviewing was restricted to neighborhoods that were predominantly white, that were heavily affected by the federal court's busing order, and that were locations of anti-busing activities.[6] The sample was drawn from the 1977 City of Boston "Annual Listing of Residents," which provides names, addresses, birthdates, and citizenship information. Respondents were selected in "clusters" of three.

Eleven trained professional interviewers, and the author, conducted the interviews between December, 1977, and April, 1978. Although the interviewing period extended over several months, no important events relating to the busing controversy occurred during this period. Thus, it is unlikely that the delay between the first and last interviews introduced bias into the data. Interviewers were instructed to interview only those respondents who were selected in the sample and to make at least three attempts to interview each potential respondent. Sixty-five percent of the people selected were interviewed.

2. Dependent Variables

This study uses two dependent variables. "Attitudinal support for the anti-busing movement" (SUPPORT) measures the extent to which respondents identified with the anti-busing movement and endorsed its goals and tactics. The scale was derived from two questions. First, respondents were asked how strongly they supported or opposed the following items: school boycotts to protest forced busing, the establishment of private academies for students who refused to be bused, attending anti-busing protest marches, and participation in anti-busing groups. Second, respondents were asked how much "in common" they felt with anti-busing demonstrators and the anti-busing group ROAR. Cronbach's alpha for the SUPPORT scale was .866, which suggests the scale is reliable.[7]

The "PARTICIPATE" scale measures participation in anti-busing activities and groups. The scale is derived from the responses to two sets of questions. First, respondents were asked if they participated in school boycotts, the establishment of private academies, protest marches, or organized anti-busing groups. Second, respondents were asked separately if they had been active in any of nine specific anti-busing organizations. Respondents were scored a "point" each time they mentioned having participated

---

[5] Residents over 53 years old and under 25 were excluded for two reasons. First, evidence from pretest interviewing and attendance at anti-busing rallies and meetings indicated that a very high proportion of the people involved in the anti-busing movement were parents of school age children. Excluding the over-53 and under-25 age groups, therefore, increased the proportion of anti-busers in the sample.

Second, a relatively high percentage of Boston residents are elderly (Tilly, 1965:6). Research in other contexts (e.g., Marsh, 1977) indicates that elderly people infrequently join social movements. In addition, in pretest interviewing it was found that the majority of Boston's elderly are so often overwhelmed by their own personal problems that they are unlikely to join any movement. Thus, since elderly residents were not likely to participate in the anti-busing movement for reasons not relevant to our research interests, they were excluded from the sample.

[6] The three neighborhoods meeting these criteria are South Boston, Hyde Park, and West Roxbury.

[7] See Useem (1979:chap. 2) for a full discussion of the reliability and validity of the dependent variables.

Table 1. Rotated Factor Matrix for Solidarity Items

Variable		Factor 1	Factor 2	Factor 3
a.	Feels a great deal in common with people in neighborhood	.092	.122	.655
b.	Feels strong ties to neighborhood	.098	.139	.700
c.	Long residence in neighborhood	.069	.003	.502
d.	Long residence in Boston	.080	−.028	.585
e.	Owns home	.132	.215	.143
f.	Belongs to secondary organizations	.216	.599	−.082
g.	Attended meeting, dinner, or rally for candidate in recent local election	.084	.706	.195
h.	Worked for candidate in recent local election	−.043	.749	.213
i.	Wore a campaign button or put a campaign sticker on car in recent local election	−.022	.615	.292
j.	Voted in recent local election	.262	.283	.202
k.	Wrote public official giving opinion about something that should be done	.230	.473	−.251
l.	Wrote a letter to the editor of a newspaper giving political opinion	.145	.589	−.286
m.	Talked about politics and public affairs with family and relatives	.751	.024	.033
n.	Talked about politics and public affairs with people at work	.750	.160	.092
o.	Talked about politics and public affairs with friends and neighbors	.829	.110	.102
p.	Talked about politics and public affairs with community leaders, people in politics	.327	.579	.001
	Percentage of total variance:	23.4%	11.1%	9.2%
Percentage of variance accounted for by all 3 factors:	43.7%			

□Indicates a loading above .400.

in an anti-busing activity or organization.[8] A Cronbach's alpha of .828 for the PARTICIPATE scale indicates that it is reliable.

3. *Solidarity Model versus Mass Society Model*

As the first step in assessing the relationship between the dependent variables and social solidarity, indices of social solidarity were developed. "Social solidarity" is defined as the extent to which people participate in organized groups and are objectively and subjectively attached to their community. Two measures of solidarity are, therefore, used. One, labelled "group participation," measures a respondent's participation in various group activities. A second index of solidarity, "community attachment," measures a respondent's bonds with his or her community.

Our questionnaire included sixteen measures of solidarity. To substantiate the hypothesis that the two dimensions of solidarity have distinct empirical referents, the measures of solidarity were factor analyzed. The results of the factor analysis are reported in Table 1.[9] Factor loadings above .400 are circumscribed. The factor extracted in the third column confirms our expectation of the existence of a community attachment dimension. Forming this dimension are questions on feelings of being similar to or different from one's neighbors, feelings of ties to one's neighborhood, and length of residence in Boston and in one's neighborhood.

The factor loading in the first and second columns reveals that group participation has two nearly independent components, rather than the single dimension as originally hypothesized. The factor extracted in the second column appears to represent activity within secondary groups. The items loading highly on this factor are a scale measuring active membership in any of fourteen voluntary organizations (e.g., fraternal lodge, veterans' organization, labor union); a series of questions on nonvoting forms of involvement in a local campaign; a question on writing public officials and newspapers or magazines; and questions on talking with community leaders and people in politics.

---

[8] We created a three-category scale of participation in the anti-busing movement. Respondents were categorized as scoring either 0 (no participation in any of the activities or groups), 1 (participation in one and only one anti-busing activity or group), or 2 (participation in two or more such activities). We truncated the scale since we cannot meaningfully discriminate, for example, between respondents who participated in two activities from respondents who participated in three or four activities. The validity of the recoding strategy is supported by the fact that the correlates of the recoded PARTICIPATE scale are higher than when the full range of values is used.

[9] The solution was computed using a principal component factor procedure and a varimax rotation.

The factor extracted in the first column appears to represent activity within primary groups. The items include the questions on talking about politics and public affairs with family and relatives, with people at work, and with friends and neighbors.

Using the factor scores derived from the factor analysis, we can construct three scales labelled "community attachment," "secondary group participation," and "primary group participation." It should be noted, however, that the questions making up the primary group participation scale are very limited in number and range. The scale has only three questions and they all ask the respondents about "talking about politics." Thus, findings based on the use of this scale should be interpreted with particular caution.

Having constructed measures of solidarity, we now compare the usefulness of the solidarity and mass society version of breakdown theory for explaining anti-busing movement support and participation. Displayed in Table 2 are the correlations between the three measures of solidarity and the dependent variables. The positive signs associated with the correlation coefficients in the first two rows indicate that respondents scoring high on the community attachment scale and on the secondary group participation scale tend to support and participate in the anti-busing movement. These findings favor the solidarity model over the mass society model. Also, the magnitude of the correlations are, in several instances, substantial. Community attachment, for example, explains 25% of the SUPPORT variance, while secondary group activity explains 11% of the variance of PARTICIPATE.

Table 2. Correlations among Community Attachment, Secondary Group Participation, Primary Group Participation and SUPPORT and PARTICIPATE; Partial Correlations among Community Attachment, Secondary Group Participation, Primary Group Participation and SUPPORT and PARTICIPATE, Controlling for Education

	SUPPORT	PARTICIPATE
Zero-Order Correlations		
1. Community Attachment	.502**	.359**
2. Secondary Group Participation	.165**	.327**
3. Primary Group Participation	−.112**	−.088*
Partial Correlations (controlling for education)		
4. Community Attachment	.465**	.322**
5. Secondary Group Participation	.299**	.390**
6. Primary Group Participation	.024	−.011

Note: Double starred and single starred entries are significant at the .01 and .05 levels, respectively. Nonstarred entries are insignificant at the .05 level.

More ambiguous is the meaning of the correlation between primary group participation and the two dependent variables (row 3). The negative direction of the correlations indicates support for mass society theory. The correlations are small, however, explaining no more than 1% of the variance of either dependent variable.

We next control the relationship between the three solidarity variables and the dependent variables with a measure of social class, education.[10] We control for social class because previous research has demonstrated that participation in organizational activities and social class covary positively (Verba and Nie, 1972), and the Boston data indicate that support of and participation in the anti-busing movement and social class covary negatively.[11] Thus, social class may operate as a "suppressor" variable on the relationship between secondary group participation and the dependent variables. The data presented in row 5 of Table 2 bear out this expectation. Controlling for education increases the correlation between secondary group participation and SUPPORT and PARTICIPATE from .165 to .200 and from .327 to .390, respectively.

---

[10] Controlling for two additional measures of social class, family income, and head of household's occupation has virtually no effect on the resulting partial correlations. These data are not presented here.

[11] The correlations between respondents' education and SUPPORT and PARTICIPATE are −.472 and −.273, respectively.

Also of theoretical significance is the effect of controlling for education on the relationship between primary group participation and the two dependent variables (row 6). When education is controlled, the resulting partial correlations between primary group participation and the dependent variables are not significantly different from zero. Thus, the zero-order negative association between primary group solidarity and the dependent variables is a spurious product of their joint dependence on education.

In sum, the evidence clearly undercuts a mass society explanation of anti-busing movement support and participation. Respondents scoring high on the scale measuring community attachment and secondary group participation were more, rather than less, likely to join and support the anti-busing movement. Furthermore, controlling for education increases substantially the correlations between secondary group participation and the two dependent variables. This finding is especially damaging to the mass society model, since that theory stresses the role of secondary organizations in inhibiting social movement participation. Controlling for social class also reveals that the negative zero-order correlation between primary group participation and anti-busing movement support and participation are spurious. Thus, whatever little support existed for mass society theory in the initial set of correlations cannot be considered genuine.

The positive correlations between community attachment and secondary group participation and the two dependent variables demonstrate that social cohesion facilitates, rather than inhibits, support and participation in this protest movement. According to solidarity theory, community attachment increases protest participation by providing citizens with a set of shared symbols, community pride, and other conditions that facilitate the mobilization process.

Why does primary group participation, in contrast to secondary group participation, fail to correlate with anti-busing movement support and participation? One plausible explanation is that the poor quality of the primary group participation scale results in its low correlation with the dependent variable. Given the limitation of the data, however, we cannot test this explanation.

A more substantive explanation for the differing correlations between the dependent variables and primary group participation and secondary group participation can be derived from solidarity theory. As noted above, solidarity theorists argue that one way in which solidarity increases protest participation is by making bloc mobilization possible. The recruitment of entire blocs of previously organized individuals, solidarity theorists hypothesize, greatly facilitates the mobilization process. Given the informal nature of most primary groups in American society, however, it is unlikely that primary groups would provide a basis of bloc mobilization. Thus, an individual scoring high on the primary group participation scale is no more likely than a low scorer on the scale to be subject to bloc mobilization.

That at least several secondary organizations did function as bases for bloc mobilization is supported by historical evidence. For example, many of the anti-busing movement leaders were active in Home and School Associations, a predominantly white parent organization. This link allowed the anti-busers to mobilize several Home and School Associations directly into their movement. The anti-busers also drew into their movement several preexisting civic associations, such as the West Roxbury Residents Association and the South Boston Action Council. Thus, the bloc mobilization of secondary groups, but not of primary groups, appears to account for their differing correlations with the dependent variables.

3. Solidarity Model versus Discontent Model

In this section we will first assess the discontent theorists' argument—that grievances are a significant cause of protest support and participation—by examining the relative deprivation version of discontent theory. Second, we will examine the breakdown theorists' assumption that social disorganization increases discontent.

Relative deprivation here refers to the feeling that one is deprived of a desired object, in comparison to other persons not being deprived of it.[12] We will describe individuals as experiencing "opportunity relative deprivation" if they feel the system provides the opportunities to acquire a desired goal to other individuals or groups, but not to them or their group. "Outcome relative deprivation," on the other hand, arises when individuals feel the rewards they are receiving, such as income, are too few, compared to those received by others.

The existence of two bases for opposition to affirmative action programs illustrates this distinction. Some argue that they unfairly deprive whites of the opportunity to acquire a particular benefit. Others, in contrast, assert that these programs grant blacks a disproportionate amount of the social benefit at hand.

An opportunity relative deprivation scale was built from the responses to two questions:

> I will now read some statements about current issues and relations between the races. Please tell me whether you strongly agree, mildly agree, mildly disagree, or strongly disagree with the following statements. . . .
>
> Many of the government's policies designed to help blacks are a form of unfair "reverse discrimination" against whites.
>
> Irish, Italian, Jewish and many other immigrants overcame prejudice and worked their way up. Blacks should do the same without any special favoritism.

The response from two sets of questions are combined to form an index of outcome relative deprivation. First, respondents were asked to evaluate how well various groups and they themselves had fared economically over the past several years. An introductory statement read:

> Now we'd like your opinion about the economic gains and losses made by different groups over the past 5 years. (HAND CARD D) Suppose those groups which have gained a great deal economically over the past 5 years are at the top of the scale—at point number plus 3. And suppose the groups which have lost a great deal economically over the past 5 years are at the bottom of the scale—at point number minus 3. The middle of the scale, zero, represents groups which have neither gained nor lost over the past 5 years. Where would you place (people in your neighborhood, blue-collar workers, white-collar workers, professionals, black people in general, people in the suburbs outside of Boston, and you yourself) on this scale?

A second set of questions asked the respondents if they thought these same groups were getting their just economic rewards. The question read:

> Next we'd like to know if you think these groups are now getting economically what they deserve—that is, what is fair. (HAND CARD E) All things considered, do you think (people in your neighborhood, etc.) get more than they are entitled to, just what they are entitled to, or less than they are entitled to?

---

[12] Analysts have defined relative deprivation in two substantially different ways. According to one tradition, relative deprivation occurs when individuals perceive a discrepancy between their goals and their current or anticipated level of achievement. The larger the gap between persons' situation and their desired level of achievement, the more likely are they to become involved in a social movement (Gurr, 1970:13; Feierabend et al., 1969). This tradition of research is based on the frustration/aggression theory in psychology (Gurr, 1970:9). Stated simply, the frustration/aggression theory holds that frustrated individuals will tend to act aggressively toward the perceived source of frustration (Berkowitz, 1962). The same psychological mechanism that links frustration to aggression, according to these researchers, also links relative deprivation to political protest and violence.

Drawing on reference group theory, analysts in a second tradition maintain that relative deprivation occurs when persons feel that their own or their group's lot compares unfavorably and unjustly with the situation of other individuals or groups. This definition emphasizes group comparisons and standards of justice (Runciman, 1966; Vanneman and Pettigrew, 1972).

We use the second definition for two reasons. First, social movements are basically attempts to alter or preserve group or "structural" relationships. Thus, the relevant evaluation is whether people feel that the current group relationships operate in their favor or not (Vanneman and Pettigrew, 1972:482). Second, people can feel that the current group relationships fail to operate in their favor and also feel that such a situation is justified and proper. Thus, feelings of deprivation in comparison to others will motivate protest only when that comparison offends people's sense of justice (Moore, 1978:49–116).

Table 3. Stepwise Multiple Regression among Relative Deprivation Variables and SUPPORT and PARTICIPATE

	Simple R	Multiple R	R^2 Change
Relative Deprivation Variables		SUPPORT	
1. Opportunity relative deprivation	.537	.537	.289
2. Neighbors versus blacks	.423	.572	.039
3. Self versus professionals	.285	.586	.015
Relative Deprivation Variables		PARTICIPATE	
4. Self versus blacks	.335	.335	.112
5. Opportunity relative deprivation	.329	.388	.038
6. Whites versus professionals	.298	.426	.031

We derived measures of outcome relative deprivation in the following way: a respondent's score on white/black relative deprivation, for example, is computed by subtracting his/her scores on the two questions about whites from his/her scores on the two questions about blacks, and then combining the two remainders.

Displayed in Table 3 are the results of a stepwise multiple regression equation which included both outcome and opportunity relative deprivation measures. Excluded from further consideration are the outcome relative deprivation measures which produced an increment in R^2 that was smaller than .01 and statistically insignificant at the .05 level. The simple correlations displayed in column 1 of Table 3 show that both a measure of opportunity relative deprivation and several measures of outcome relative deprivation are strongly to moderately related to protest support and participation. Opportunity relative deprivation, for example, explains 29% of the variance of SUPPORT and 15% of the variance of PARTICIPATE. Similarly, the respondents' comparisons between the economic situation of their neighbors with that of blacks "explains" 18% of the variance of PARTICIPATE.

In addition, the R^2 change in column 3 shows that both opportunity and outcome relative deprivation contribute separately to the explained variance of each dependent variable. Finally, the multiple correlations displayed in rows 3 and 6 undercut the solidarity theorists' argument that feelings of deprivation are not a significant cause of protest support and participation. Relative deprivation explained 34% of the variance of SUPPORT and 18% of the variance of PARTICIPATE. Thus, the data support the hypothesis that if a person feels deprived relative to members of another group, s/he is likely to support or join a protest movement.

It should be noted, however, that the data are also consistent with an alternative explanation. Some Boston residents may have become involved in the anti-busing movement for reasons unrelated to relative deprivation. Once they joined, intense exposure to the anti-busing movement's ideology may have bred feelings of relative deprivation. Longitudinal (panel) data are required to test these conflicting explanations.

4. *Disorganization and Discontent*

In this section, we test the breakdown theorists' hypothesis that disorganization breeds discontent (see Figure 1, part 1B). The first step in the analysis is to reduce the number of relative deprivation and solidarity variables, so as to simplify and facilitate the statistical analysis. In the previous section we used stepwise multiple correlation to isolate the five "best" measures for predicting anti-busing movement support and participation. We can construct an index which summarizes the respondents' scores on each of these relative deprivation variables. The "New RD" index is defined as the number of times each respondent scores among the most deprived fifth on each of the relative deprivation measures.[13]

---

[13] Cronbach's alpha for the New RD scale is high (.819). The correlations of the New RD variables with the dependent variables are .477 and .405. These correlations are not much lower than the multiple correlations of all the relative deprivation variables used to make up the New RD scale. This indicates that the New RD variable successfully sum-

We will retain as separate variables both the community attachment scale and the secondary group participation scale. The primary group participation scale will be excluded from further consideration, however, because of its low correlation with the dependent variables.

Table 4 displays the correlations between the solidarity variables and the New RD scale. The positive correlation between the New RD scale and community attachment (r = .282) and secondary group participation (r = .114) means that respondents scoring high on the New RD scale tend also to score high on the two solidarity scales. This finding is of considerable theoretical interest, since it contradicts the breakdown theorists' hypothesis that social disorganization increases the level of discontent. Although the correlations between relative deprivation and the solidarity indices are only moderately large, they indicate that solidarity, not disorganization, breeds discontent.

We can speculate why relative deprivation is a positive function of solidarity. Strong community attachment and high rates of group participation increase an individual's awareness of and concern for others in his or her community. The level of discontent of such an individual would be affected by both grievance-producing events against him or her and grievance-producing events against others in his or her community. Thus, solidarity increases discontent by multiplying the effects of grievance-producing events.

In sum, one version of the breakdown model has two steps: disorganization → discontent → protest. While the Boston data support the second step in the model, they undercut the first step.

5. *Separate Effects of Solidarity and Relative Deprivation*

The additional finding that relative deprivation and solidarity covary, however, raises the possibility that the correlation between relative deprivation and the dependent variables are spurious products of their joint dependence on solidarity. The coefficients in Table 5 reveal the extent to which this is the case. Displayed in row one of the table are the zero-order correlations between New RD (relative deprivation) and the dependent variables. Row three of the same table shows the partial correlations between New RD and the two dependent variables, while controlling for the two solidarity variables. Other data in Table 5 allow us to assess the extent to which the solidarity variables and the dependent variables correlate because of their joint dependence on solidarity. Row two of the table shows the multiple correlation between the two solidarity variables and the dependent variables. Finally, displayed in row four of Table 5 are the multiple-partial correlations between the two solidarity variables and the dependent variables, while we control for New RD.

Table 4. Correlations between Community Attachment and Secondary Group Participation and Relative Deprivation

	Relative Deprivation
1. Community Attachment	.282
2. Secondary Group Participation	.114

Note: Both entries are significant at the .01 level.

Table 5. Correlations, Multiple Correlations, Partial Correlations, and Multiple Partial Correlations among New RD, Neighborhood Solidarity, Secondary Group Solidarity, SUPPORT and PARTICIPATE

	SUPPORT	PARTICIPATE
1. New RD	.435	.403
2. Multiple Correlation, Community Attachment and Secondary Group Participation	.506	.437
3. Partial Correlation, New RD Controlling for Community Attachment and Secondary Group Participation	.351	.334
4. Multiple Partial Correlation, Community Attachment and Secondary Group Participation Controlling for New RD	.439	.279

Note: All entries are significant at the .01 level.

---

marizes the information contained in each of the five relative deprivation variables used to make up the scale.

Comparison of the coefficients in rows one and three of Table 5 reveals that controlling for the two solidarity variables reduced the correlations between relative deprivation and SUPPORT and PARTICIPATE, from .435 to .351 and from .403 to :334, respectively. This indicates that the influence of relative deprivation on the dependent variables is moderately diminished if the confounding effects of the solidarity variables are removed. Each of the partial correlations, however, is moderately strong and statistically significant at the .01 level.

Similarly, comparison of the coefficients in rows two and four of Table 5 shows that controlling for the New RD variable reduces the multiple correlation between the two community solidarity variables and SUPPORT and PARTICIPATE from .506 to .439 and from .406 to .395, respectively. Again, the partial correlations are moderately strong and statistically significant at the .01 level.

In sum, the strength of the four partial correlations in rows four and five of Table 5 suggests that the relationship between discontent, solidarity, and the dependent variables is genuine. These partial correlations are the strongest evidence presented that both relative deprivation and solidarity contribute separately to the variation in anti-busing movement support and participation.

6. *Conclusion*

This paper has examined the reasons for popular participation in the Boston anti-busing movement. The analysis has several implications for the theoretical issues presented at the outset.

The Boston data provide no support for the mass society model. Isolated individuals were less, not more, likely than others to become involved in the anti-busing movement. The evidence instead supports the solidarity theorists' contention that mobilization will not take place unless the conditions normally associated with solidarity are present. Apparently, Boston's neighborhood organization and the residents' strong attachment to their community provided some of those conditions. In addition, historical evidence supports the solidarity theorists' hypothesis that bloc recruitment facilitates mobilization.

The Boston data, however, fail to sustain the solidarity theorists' further argument that discontent plays an insignificant role in accounting for protest. Both solidarity and a form of discontent, relative deprivation, contributed separately and substantially to the variation in anti-busing movement support and participation. This conclusion is tentative, however, since we were unable to establish the extent to which relative deprivation was a product, rather than a cause, of participation in the anti-busing movement. Nevertheless, these findings suggest that the fundamental problem with the solidarity theorists' argument is its premise that "discontent is ever-present for deprived groups" (Jenkins and Perrow, 1977:251). This premise requires empirical proof, and to date no solidarity theorist has seriously attempted to provide the requisite evidence. Indeed, solidarity theorist Anthony Oberschall (1978:298) expresses the premise so ambivalently as to call into question his own commitment to it: "[g]rievances and disaffection are a fairly permanent and recurring [*sic*] feature of the historical landscape." More significantly, carefully conducted research by Moore (1978) indicates that even the most extremely deprived groups do not always experience discontent. Moore's (1978) study shows that some inmates of Nazi concentration camps perceived their suffering to be unjust or morally wrong. Thus, Moore's research as well as the Boston data do not support those solidarity theorists who argue that one should assume, a priori (Jenkins and Perrow, 1977:250), that fluctuations in discontent cannot account for protest.

While the finding (that discontent and protest covary) undercuts the solidarity argument, the Boston data also fail to support the breakdown contention that discontent is the product of social disorganization. Disorganization and discontent were inversely, not directly, correlated. This finding suggests that discontent is generated by social organization and group solidarity.

The finding that disorganization and discontent are inversely related suggests an additional weakness in the breakdown model. Not only are isolated individuals without the resources to engage in protest, as the solidarity theorists would argue, but isolated individuals are also less likely to experience relative deprivation sufficient to motivate protest.

Researchers have traditionally confined their analysis to the consquences of outcome relative deprivation, ignoring the implications of opportunity relative deprivation. Thus, our finding that both opportunity relative deprivation and outcome relative deprivation are separately and substantially related to protest has practical as well as theoretical implications. The belief of whites that they are being deprived of opportunities in favor of blacks may have an independent political impact in other social contexts. Feelings of opportunity relative deprivation may motivate, for example, protest against government and private programs designed to increase opportunities for blacks.

Our analysis of popular involvement in the Boston anti-busing movement leads us to conclude that both the breakdown and solidarity theories help to explain protest. That several different theories helped explain a single phenomenon suggests a need for an integrated theory based on insights from the different models.

REFERENCES

Berkowitz, Leonard
1962 Aggression: A Social Psychological Analysis. New York: McGraw-Hill.

Bowen, Don R., Elinor Bowen, Sheldon Gawiser, and Louis H. Masotti
1968 "Deprivation, mobility, and orientation toward protest of the urban poor." Pp. 187–200 in Louis H. Masotti and Don R. Bowen (eds.), Riots and Rebellion: Civil Violence in the Urban Community. Beverly Hills: Sage.

Caplan, Nathan S., and Jeffery M. Paige
1968 "A study of ghetto rioters:" Scientific American 219:15–20.

Coleman, James S.
1957 Community Conflict. New York: Free Press.
1971 "Community disorganization and conflict." In Robert K. Merton and Robert A. Nisbet (eds.), Contemporary Social Problems (3rd ed.). New York: Harcourt, Brace, Jovanovich.

Crawford, Thomas, and Murray Naditch
1970 "Relative deprivation, powerlessness, and militancy: the psychology of social protest." Psychiatry 33:208–23.

Davies, James C.
1962 "Toward a theory of revolution." American Sociological Review 27:5–19.

Feierabend, Ivo K., Rosalind L. Feierabend, and Betty A. Nesvold
1969 "Social change and political violence: cross-national patterns." Pp. 606–68 in H. D. Graham and T. R. Gurr (eds.), Violence in America: Historical and Comparative Perspectives. Washington, D.C.: National Commission on the Causes and Prevention of Violence.

Freeman, Jo
1973 "The origins of the women's liberation movement." American Journal of Sociology 78:792–811.
1979 "Resource mobilization and strategy: a model for analyzing social movement organization actions." Pp. 167–89 in Mayer N. Zald and John D. McCarthy (eds.), The Dynamics of Social Movements. Cambridge, Mass.: Winthrop.

Gerlach, Luther P., and Virginia H. Hine
1970 People, Power, Change: Movements of Social Transformation. Indianapolis: Bobbs-Merrill.

Gurr, Ted Robert
1970 Why Men Rebel. Princeton: Princeton University Press.

Heirich, Max
1977 "Change of heart: a test of some widely held theories about religious conversion." American Journal of Sociology 83:653–80.

Huntington, Samuel P.
1968 Political Order in Changing Societies. New Haven: Yale University Press.

Jenkins, J. Craig, and Charles Perrow
1977 "Insurgency of the powerless: farm worker movements (1946–1972)." American Sociological Review 42:249–68.

Katznelson, Ira
1978 "The crisis of the capitalist city: urban politics and social change." Pp. 214–29 in Michael Lipsky and Willis D. Hawley (eds.), Theoretical Perspectives in Urban Politics. Englewood Cliffs: Prentice-Hall.

Kornhauser, William
1959 The Politics of Mass Society. New York: Free Press.

Lodhi, Abdul Qaiyum, and Charles Tilly
1973 "Urbanization, crime and collective violence in 19th century France." American Journal of Sociology 79:296–318.

McPhail, Clark
1971 "Civil disorder participation: a critical examination of recent research." American Sociological Review 36:1058–73.

Marsh, Alan
1977 Protest and Political Consciousness. Beverly Hills: Sage.

Miller, Abraham H., Louis Bolce, and Mark R. Halligan
1976 "The new urban blacks." Ethnicity 3:338–67.

Moore, Barrington, Jr.
1978 Injustice: the Social Bases of Obedience and Revolt. White Plains: M. E. Sharpe.

Morgan, William R., and Terry N. Clark
1973 "Causes of racial disorders: a grievance-level explanation." American Sociological Review 38:611–24.

Oberschall, Anthony
1973 Social Conflict and Social Movements. Englewood Cliffs: Prentice-Hall.
1978 "Theories of social conflict." Pp. 291–315 in Ralph Turner, James Coleman, and Renèe C. Fox (eds.), Annual Review of Sociology. Palo Alto: Annual Review.

Pinard, Maurice
1971 The Rise of a Third Party: A Study of Crisis Politics. Englewood Cliffs: Prentice-Hall.

Piven, Francis Fox, and Richard Cloward
1977 Poor People's Movements. Why They Succeed, How They Fail. New York: Pantheon.

Rule, James, and Charles Tilly
1972 "1830 and the unnatural history of revolution." Journal of Social Issues 28:49–76.

Runciman, W. G.
1966 Relative Deprivation and Social Justice. Berkeley: University of California Press.

Shorter, Edward, and Charles Tilly
1974 Strikes in France, 1830–1968. New York: Cambridge University Press.

Smelser, Neil J.
1962 Theory of Collective Behavior. New York: Free Press.

Snow, David A.
n.d. "Differential recruitment to and the differential growth of social movements: toward a microstructural theory." Mimeo, Department of Sociology, University of Texas.

Snyder, David R., and Charles Tilly
1972 "Hardship and collective violence in France, 1830–1960." American Sociological Review 37:520–32.

Spilerman, Seymour
1970 "The causes of racial disturbances: a comparison of alternative explanations." American Sociological Review 35:627–49.
1971 "The causes of racial disturbances: test of an explanation." American Sociological Review 36:427–42.
1976 "Structural characteristics of cities and the severity of racial disorders." American Sociological Review 41:771–93.

Tilly, Charles
1965 "Metropolitan Boston's social structure." In Social Structure and Human Problems in the Boston Metropolitan Area. Cambridge: Joint Center for Urban Studies of MIT and Harvard University.
1975 "Revolutions and collective violence." Pp. 483–555 in Fred I. Greenstein and Nelson Polsby (eds.), Handbook of Political Science. Reading, Mass.: Addison-Wesley.
1978 From Mobilization to Revolution. Reading, Mass.: Addison-Wesley.

Tilly, Charles, Louise Tilly, and Richard Tilly
1975 The Rebellious Century, 1830–1930. Cambridge: Harvard University Press.

Useem, Bert
1977 "Peasant involvement in the Cuban Revolution." Journal of Peasant Studies 5:99–111.
1979 "The Boston anti-busing movement and social movement theory." Ph.D. dissertation, Department of Sociology, Brandeis University.

Vanneman, Reeve D., and Thomas F. Pettigrew
1972 "Race and relative deprivation in the United States." Race 13:461–86.

Verba, Sidney, and Norman H. Nie
1972 Participation in America: Political Democracy and Social Equality. New York: Harper.

Von Eschen, Donald, Jerome Kirk, and Maurice Pinard
1971 "The organizational substructure of disorderly politics." Social Forces 49:529–44.

Wolf, Eric
1969 Peasant Wars of the Twentieth Century. New York: Harper and Row.

ISSUES IN THE COMPARATIVE MEASUREMENT OF POLITICAL DEMOCRACY*

KENNETH A. BOLLEN**
Brown University

American Sociological Review 1980, Vol. 45 (June:370–390)

The empirical study of the causes and consequences of political democracy has been the subject of considerable research. Yet cumulative development of this research is hampered by the controversial aspects and limitations of the existing indices of political democracy. These issues concern the validity of the indicators, the unknown reliability, and the limited sample and temporal coverage of these indices. After a discussion of these issues, a revised index of political democracy that overcomes some of these limitations is presented. The indicators of the revised index are analyzed by means of confirmatory factor analysis and the reliability of the measure is discussed. The index is generally better than existing measures in reliability, sample size, and temporal coverage; but the remaining limitations of the index are reviewed. The moderate to high correlations of this index with other democracy indices support its external validity. Yet, the differences in empirical results possible by using different indices are demonstrated. Finally, there are two appendices. The first provides the technical details of the confirmatory factor analysis. The second appendix lists the values of the political democracy index.

For over two decades, political democracy has been an important variable in quantitative, cross-national research (e.g., Lerner, 1958; Neubauer, 1967; Stack, 1978). Increasingly, many researchers view political democracy as a luxury that can be ill-afforded by Less Developed Countries (LDCs) facing the more pressing problems of socioeconomic development, income inequality, and rapid population growth. Yet, despite the shift in research to these other problems, political democracy has remained of interest because of its hypothesized impact upon (and relationship with) development, distribution, and population growth. For instance, a number of researchers suggest that political democracy is an obstacle to economic development. Others argue that the policies needed to reduce rapid population growth in the Third World are not possible in a democratic framework (e.g., Heilbroner, 1974). These and other hypotheses remain largely untested.

A researcher wishing to test these hypotheses faces a number of difficulties. One central problem is the controversy surrounding the measurement of political democracy (e.g., Cutright, 1968; Neubauer, 1968; May, 1973). These measurement problems can drastically affect empirical results. An illustration of this is the recent research on the impact of political democracy on income inequality. The findings of these studies are often contradictory. For instance, Cutright (1967) finds that political democracy reduces income inequality, while Jackman (1975) finds no effect once economic development is controlled. Hewitt (1977:460) concludes that it is "plausible to assume that the apparent relationship between democracy and equality is indeed a spurious one." On the other hand, Stack (1978) finds that democratic performance reduces income inequality even after controls for economic development and dependency. Jackman (1975:86–7), Hewitt (1977:456), and Stack (1979:168–9) argue that the primary reason for these contradictory findings is the difference among the measures of political democracy which are used.

Although many researchers have called attention to the importance of proper measurement instruments and the large degree of measurment error in cross-national variables, papers mainly devoted

---

* Direct all communications to: Kenneth A. Bollen; Societal Analysis Department; General Motors Research Laboratories; Warren, MI 48090.

** Now at General Motors Research Laboratories, Societal Analysis Department, Warren, Michigan 48090.

I wish to thank Barbara Entwisle and Robert M. Marsh for extremely helpful comments on earlier drafts of this paper. I also wish to thank Dietrich Rueschemeyer and Vince Covello who commented on the early stages of this research.

to developing valid and reliable cross-national indices are rare. The cost of this neglect is the variety of results that characterizes, for example, the democracy-inequality findings. Similar diversity will accompany other analyses of the causes and consequences of political democracy until some of these measurement issues are addressed. Thus, the cumulative development of research in this area dictates that greater attention be given to the measurement of political democracy (Lenski, 1976).

There are several purposes of this paper. First, the controversial aspects and limitations of the commonly used indices of democracy are discussed. These issues concern the validity of the indicators, the absence of any discussion of reliability, and the limited sample and temporal coverage of these indices. A second purpose is to present a revised index that overcomes some of these limitations. The indicators of the revised index are analyzed by means of confirmatory factor analysis, and the reliability of the measure is discussed. That index is generally better than existing indices in reliability, sample size, and temporal coverage. The correlation of this revised measure with the other democracy indices is calculated as a means of external validation. The differences in empirical results, made possible by using different indices, are demonstrated. Finally, to facilitate future research and to allow critical evaluation, an appendix lists the revised political democracy index for 123 countries in 1965, and 113 in 1960.

A Theoretical Definition of Political Democracy

A theoretical definition of political democracy is a prerequisite to evaluating the validity of past indices and in choosing indicators for a revised index. The theoretical definition is necessary to establish a standard by which the indicators may be evaluated. Much debate surrounds the definition of democracy and it is not possible to resolve the dispute, here. But it is possible and necessary to "outline" a definition which is used to guide the research in this paper.

As de Schweinitz (1964:13) observed:

> Democracy is one of those troublesome words which means all things to all people. Like motherhood and patriotism, it is thought to be a noble condition and is evoked by politicians, publicists, preachers, and demogogues to prove their unsullied intentions and just claim to popular support.

Today, one need only look at the various nations in the world which consider themselves democracies. It is clear that the various "People's Democracies" (e.g., East Germany) have a form of government which many persons living in market economy nations (e.g., the United States) do not consider democratic. Likewise, the "Bourgeois Democracies" existing in capitalist economies are not considered true democracies by the People's Democracies.

In the social sciences, a number of definitions of political democracy have come into use. Several of the more common definitions are discussed here. Lipset (1963:27) defines democracy as

> a political system which supplies regular constitutional opportunities for changing the governing officials, and a social mechanism which permits the largest possible part of the population to influence major decisions by choosing among contenders for political office.

Lipset's definition places a heavy emphasis on elections to choose representatives that hold political office.

Dahl (1956:67–90), like Lipset, emphasizes electoral processes in his definition of democracy. He also suggests a number of more specific criteria to evaluate whether or not the elections are democratic. For instance, Dahl suggests, among other things, that a democracy should have elections in which each vote is weighted equally, each individual possesses identical information about the alternatives presented for decision, and the orders of elected officials are executed.

Downs (1957:23–4) states that a democratic society must have periodic elections, decided by majority rule, in which each voter casts only one vote. In addition, he suggests that there must be two or more parties competing for control, and a single party (or coalitions of parties) chosen to run the government. Lenski

(1966:319) identifies democratic political systems not only by the extent of the electoral franchise but also by the existence of political liberties, which preserve the right of organized political opposition.

A second group of writers has focused, not on the existence of fair elections and political liberties but, instead, on the existence of political elites. Mosca (1939), Michels (1962), and Mills (1956), among others, have argued that in all nations there exists a group which holds a disproportionate degree of political power. Michels formulated his "iron law of oligarchy" in the belief that it is functionally necessary for power to be centered, eventually, in an elite of any organization or society. Similarly, Bachrach (1967:1) argues: "the exigencies of life in the industrial and nuclear age necessitate that key and crucial political decisions in a democracy, as in a totalitarian society be made by a handful of men."

It is true that the premier, president, or dictator all possess far more control over a nation's decisions than do the masses of the population. Theorists of political elites infer from this observation little variation across societies in the concentration of political power. Yet it does not necessarily follow that the political power of the elite, relative to that of the nonelite, is the same in each country.

In some countries the political system may allow for a stronger elite than in other countries. In Spain during Franco's rule, for instance, political power was far more concentrated than in Britain or Canada. Similarly, the elites of Saudi Arabia hold more political power than the elites of Australia. The important point to recognize is that the political power of the elite, relative to the nonelite, differs from nation to nation.

It is these *differences* in the political power held by the elite, relative to the nonelite, that helps identify how democratic a nation is. I define *political democracy* as *the extent to which the political power of the elite is minimized and that of the nonelite is maximized.* Identifying these differences in political power is extremely difficult. The major difficulty is that there is no widely accepted unit of measure for political power. Unlike in a study of the distribution of income, the number of "dollar-like" units of political power held by the various groups in a country cannot be evaluated. The approach to measurement, in this case, must be more indirect. The definitions of Lipset, Dahl, Downs, and Lenski discussed earlier provide some help in deriving these indirect measures. In particular they call our attention to elections and political liberties which may increase the political power of the nonelite.

There are a number of characteristics that determine whether elections bring political power to the nonelite. For example, the elections should be fair, allow some choice from among alternative candidates or policies, and allow a universal franchise. In addition, the results of the election must be binding on all parties. In nations where these and other conditions (e.g., the equal weighting of voting) are met, the nonelites have more political power over elites and their policies than in nations where no elections, or only rigged ones, occur. Popular sovereignty as represented in electoral processes is not enough. A nonelite should also possess political liberties.

Political liberties refer to the rights of all individuals and groups to protest or support—freely—government policies or decisions. As such, political liberties provide additional political power for the nonelite, allowing them to organize opposition to the elites and their policies. A population possessing free speech, a free press, and freedom of opposition has a greater potential for influencing the decisions of elites than does a country where these liberties do not exist. These political liberties also allow the formation of interest groups and participation in political parties that can compete with—and possibly replace—the existing elites. In countries where political meetings are not allowed, one or more parties are banned, or the press is censored, the nonelites' political power is reduced, and the political power of the elite is more secure.

It is sometimes thought that definitions of political democracy, as just discussed, are largely definitions of bourgeois democracy. This criticism assumes that these definitions lead to measures which uniformly rank capitalist societies as highly democratic, and socialist or communist countries as nondemocratic. However, the degree of political liberties and popular sovereignty (as represented in elections) varies both between and within capitalist, communist, and socialist nations. For instance, using these two criteria, Yugoslavia exhibits a higher level of political democracy than the Soviet Union or Albania, even though all three are communist countries. Similarly, the two criteria provide a ranking of differences in political democracy in capitalist countries. Canada, for instance, is more democratic than other capitalist countries such as Portugal or Honduras.

This definition of political democracy also shows that a communist or socialist country may be more democratic than a capitalist one. For example, in the mid-'60s Yugoslavia was more democratic, politically, than capitalist countries such as Taiwan and Spain. Furthermore, many of the Scandinavian countries with a socialist orientation are at higher levels of political democracy than are a number of capitalist nations. Thus, this definition, when evaluating the level of political democracy, does not consider the type of economic system of a country.

Previous Measures of Political Democracy

A variety of indicators appears in political democracy indices. Many of these indicators closely correspond to the theoretical definitions of political democracy. Others, however, are of questionable validity. Some of the indicators are measures of political characteristics that may occur in either democratic or nondemocratic political systems. Two such indicators are those measuring voter participation (or turnout) and political stability. These are not the only indicators with questionable validity, but these two are widely used in political democracy indices and deserve special attention.

The use of the percentage of the population voting in national elections as a measure of political democracy has a relatively long history. In his classic study, Lerner (1958) measures the level of political democracy by the percentage of the population voting in national elections. Voting-participation statistics are also in the democracy measures of Smith (1969), Jackman (1973; 1975), Coulter (1975), and Stack (1979).

There are several potentially valuable aspects of voter participation as an indicator of political democracy. First, political-participation statistics may provide a rough indicator of the extent of the national franchise, which is an important aspect of popular sovereignty.[1] Second, high political participation may lead to elected officials who are more representative of the class structure of the general population[2] (Wences, 1969; Stack, 1979). Finally, voter turnout measures the degree to which citizens play an active role in the political system (Stack, 1979).

Despite the potential usefulness of participation statistics, these statistics are confounded with many other factors, some of which are only marginally related to political democracy. Lerner (1958:68) discusses how, in some countries, voter participation is a *symbol* of political democracy often used in nondemocratic nations. Huntington (1968:89) argues: "Popular participation in politics does not necessarily mean popular control of government. Constitutional democracies and communist dictatorships are both participant polities." Similarly, Kornhauser (1959) states that voting participation is considered desirable and is encouraged in both democratic and authoritarian countries.[3] Furthermore, in a number of countries, all eligible voters are legally obligated to vote (e.g., Ecuador, *circa* 1960). In these nations, high participation does not indicate widespread political power in the nonelite. In fact, since the political elite is largely responsible for the laws of mandatory participation, the high voting turnout may indicate that the elite has considerable political power, relative to the nonelite.

---

[1] This is not true if voter turnout is expressed as a percentage of the electorate rather than of the total adult population.

[2] See Hewitt (1979) for a criticism of this assumption.

[3] Note that according to Taylor and Hudson (1972:Table 2.8), the ten countries with the greatest voter turnout are: Albania, North Korea, Soviet Union, Romania, Bulgaria, Ivory Coast, Guinea, Czechoslovakia, Gabon, and East Germany. Very few researchers would consider any of these countries as highly democratic.

An additional problem with voter participation is the ambiguous meaning of high- and low-turnout levels even when there is a universal franchise. Lipset (1963) suggests that a low level of participation may indicate that the people's will is expressed in government policy, so that there is no need for participation. That is, low participation may indicate at least partial satisfaction with the government. However, in some situations, low participation indicates not satisfaction but, rather, the apathy of the masses who feel alienated from their government. In these situations, the vote may be viewed as an ineffective tool for bringing about governmental change. In either case, these arguments suggest that voter turnout does not necessarily indicate the level of political democracy.

Furthermore, the doubtful validity of voter participation may be shown empirically. Evidence against voter turnout as a measure of political democracy is the low correlations it has with other indicators of democracy. For instance, the percentage of the over-20 population voting correlates −0.49 (N = 45) with Lowenstein's (1967) freedom of the press index and −0.28 (N = 52) with the fairness of elections (source of data: Taylor and Hudson, 1971). Voter turnout correlates 0.09 (n = 19) with Coleman's (1960) measure of the democracy in LDCs.[4]

Given the theoretical and empirical arguments against using voter turnout as a measure of political democracy, participation statistics should not be used in political democracy indices (see, also, May, 1973:14–5). This is not to deny the importance that political participation has as a concept, in its own right, but it does question the assumed equivalence of political democracy and participation.

A second component often built into political democracy indices is political stability. Frequently, countries are scored not only on the extent of political democracy but also on the stability of the political system. For instance, an early attempt at measuring political democracy classified European countries as democratic if they had had an "uninterrupted continuation of political democracy since WWI" and classified Latin American countries as democratic if they had had "a history of more or less free elections for most of the post-WWI period" (Lipset, 1959:74).[5] Other political democracy indices which incorporate measures of stability include Cutright (1963), Cutright and Wiley (1969), Smith (1969), Coulter (1975), and Hewitt (1977).

The most frequently used index is Cutright's (1963).[6] This measure (or variations of it) is used in many empirical studies of political democracy (e.g., McCrone and Cnudde, 1967; Smith, 1969; Coulter, 1975; Rubinson and Quinlan, 1977). Since this measure is widely used, it provides a good example of how political stability is incorporated into indices of political democracy. Thus, a brief description of its construction is instructive.

The time period covered by Cutright's (1963) index is 1940 to 1960. For each year, the legislative and executive branches of government are assigned a score. The highest score is assigned to each country having a parliament or legislative body that has representatives of two or more political parties, with the minority party holding at least 30% of those seats, and a chief executive who is in office through an open election. The score a nation receives for each year from 1940 to 1960 is summed to form the final index. As noted by Cutright (p. 256), this scoring technique penalizes a country for political instability and rewards it for stability.

---

[4] Even if all Communist countries are dropped, the correlation of voter turnout with press freedom is only 0.25 (N = 37) and with fairness of elections is 0.05 (N = 44), neither of which is significant at the 0.05 level. These results parallel those of Coulter (1975). Coulter constructs a democracy index of three components: competitiveness, liberties, and participation. Although he finds that liberties and competitiveness correlate 0.71, participation correlates 0.20 with competitiveness and 0.19 with liberties.

[5] As Jackman (1975:86) notes, Fox and Miller (1965) use Lipset's measure but refer to it as a measure of "political stability."

[6] Although commonly used as an index of democracy, Cutright (1963; 1968) claims that it actually measures political development. However, in later publications, Cutright refers to a very similar measure as a political representation index.

Combining measures of stability with measures of political democracy causes several difficulties. One practical problem is that of studying changes in political democracy. A stability/democracy measure is often aggregated over a ten- or twenty-year period. This degree of temporal aggregation may "average-out" many important changes in political democracy that occur. For instance, a country may alternate from a highly democratic to a nondemocratic system within a short time span. A measure aggregated over a number of years will show the average of these extremes and will, therefore, score it as moderately democratic—even though it never was moderate during this period.

An even more serious problem of using indices that combine political stability and democracy is that the results of analyses using such indices are ambiguous. This is because it is not known whether the discovered relationship is to stability, to democracy, or to some combination of the two. For instance, Cutright's (1967) analysis of the effect of political democracy on income inequality showed a significant positive effect. But, Jackman (1975:86) claims that it is not the political democracy component—but, rather, the stability component of Cutright's measure—that leads to this relationship. This ambiguity can also be expected in other cases where an index that confounds political democracy and stability is used. Thus, researchers should proceed by treating political stability as analytically and empirically separate from the concept of political democracy. Although these variables may be positively related, they are not one and the same. These arguments will be demonstrated empirically later in this article.

A Revised Index of Political Democracy

The previous sections of this article demonstrate the need for a revised index of political democracy that corresponds more closely to the theoretical definitions of the concept. In addition, indicators of voter turnout and political stability were shown to represent concepts separate and distinct from political democracy and not, therefore, properly used in democracy indices. This section of the article presents a revised political democracy index.

In the theoretical definition of political democracy, the two most important aspects were political liberties and popular sovereignty (as represented in electoral processes). Most characteristics attributed to political democracy may be classified under one or the other of these two general concepts. For instance, the fairness of elections, the proportion of the most important government positions that are elected rather than appointed, and the type of electoral system (proportionate vs. other) are indicators of popular sovereignty. The extents of free speech, free press, and the freedom to organize opposition parties or groups are indicators of political liberties. Thus, at a minimum, a measure of political democracy should include indicators of both political liberties and of popular sovereignty.

The revised index of political democracy (hereafter, POLDEM) has indicators of both of these concepts. Table 1 lists these indicators, their sources, and several technical notes on their use. A brief description of these measures follows. A more detailed description is in the original sources and in Bollen (1979).

The three indicators of political liberties are: (1) press freedom, (2) freedom of group opposition, and (3) government sanctions. Freedom of the press is ranked on a nine-point scale that measures the degree of control normally exercised by any official agency which has the power to interfere with the dissemination and discussion of the news (Nixon, 1960:17). Freedom of group opposition measures the degree to which organized opposition is allowed. It is scored on a four-point scale, ranging from a low of no parties or all but dominant party and its satellites excluded, to a high of no parties excluded.

The third indicator of political liberties is government sanctions. Government sanctions are actions taken by the government which curtail the political activities of one or more groups of the population. The sanctions include censorship of

Table 1. Six Components of the 1965 and 1960 Revised Index of Political Democracy

Variable	Symbol	Source	Notes
Press Freedom	Y_1	Nixon (1965; 1960)	—
Freedom of Group Opposition	Y_2	Banks (1971: segment 10, field o)	1
Government Sanctions	Y_3	Taylor and Hudson (1971)	1
Fairness of Elections	Y_4	Taylor and Hudson (1971)	2
Executive Selection	Y_5	Banks (1971: segment 2, field j)	1, 2, 3, 4
Legislative Selection	Y_6	Banks (1971: segment 2, field p; segment 10, field l)	1, 2, 4

Notes:

1. For Y_2, Y_3, Y_5, and Y_6, the average scores for 1964–1966 and 1959–1961 are used for the 1965 and 1960 measures, respectively. If the indicators are not available for the full three-year period, the average scores, over those years with data, are used.
2. If no elections were held from 1956–1964 (or 1961–1967) a country is scored zero for 1960 (or 1965) on variables Y_4-Y_6. Election information is from Taylor and Hudson (1971; 1972). No election information after 1967 is available from this source.
3. The "indirect election" category of Banks is combined with the "elective."
4. Trinidad and Tobago, Jamaica, and Uganda's 1960 rank, on Y_5 and Y_6, are derived from 1962 data, because earlier data are not available.

the mass media, instituting a curfew, banning or harassing political parties, and the arrest of opposition party members. This indicator is based on the number of such events.[7]

The three measures of popular sovereignty, as listed in Table 1, are: (1) fairness of elections, (2) executive selection, and (3) legislative selection. Fairness of elections scales the degree to which elections are relatively free from corruption and coercion. The scoring of this variable is based on whether or not alternative choices exist, and on whether or not the elections are administered by a nonpartisan administration. Also considered are whether or not the elections are rigged and if the results of the elections are binding on all parties. The four-point scale ranges from a low of no elections, to a high of relatively free and competitive elections.

The executive selection variable indicates whether the chief executive of a country is elected, or not. The legislative selection variable determines whether the legislative body is elected but, in addition, considers the effectiveness of the legislative body.[8] The lowest score is given to countries with no elections or a nonelective legislative body, while the highest score is given to legislative bodies that are both elected and an effective power in determining national policies.

Each of the six components was linearly transformed to range from a low of 0 to a high of 100.[9] For some countries, not all the indicators were available. In those

---

[7] One difficulty of using government sanction in its original form is that there is a built-in bias against countries with high political liberties to begin with; the greater the freedom, the more negative sanctions the government can impose. In contrast, a country low in political liberties has fewer potential restrictions to impose, because many of these negative sanctions are already in effect. Therefore, the negative government sanction indicator should take into account the degree of political liberties. The correction procedure used is to subtract the number of negative government sanctions from a political liberty score formed by a country's average freedom of the press and group opposition indicators. The correlations of this corrected measure with the five other indicators of the index are higher than when the uncorrected or logged negative sanction variable is used.

[8] The elective/nonelective legislature variable is scored 1 or 0. This is then multiplied by the effectiveness of the legislative body. The scores are multiplied so that, for example, elected "puppet" legislatures do not score as high as those that are both elected and effective, on the assumption that the former are not as important to political democracy as the latter.

[9] The range of the scale, from 0 to 100, is arbitrary. The 0 point does not represent the lowest degree of political democracy possible, nor does 100 represent complete equality. Further reductions or increases in

Table 2. Correlations of Six Components of Political Democracy, 1965 Measures Above the Diagonal (N= 123) and 1960 Measures Below Diagonal (N = 113)

		Y_1	Y_2	Y_3	Y_4	Y_5	Y_6
Press Freedom	(Y_1)	—	0.776	0.852	0.767	0.618	0.739
Freedom of Group Opposition	(Y_2)	0.770	—	0.881	0.704	0.557	0.829
Government Sanctions	(Y_3)	0.788	0.858	—	0.723	0.595	0.776
Fairness of Elections	(Y_4)	0.775	0.649	0.684	—	0.666	0.741
Executive Selection	(Y_5)	0.615	0.544	0.596	0.582	—	0.693
Legislature Selection	(Y_6)	0.765	0.795	0.758	0.695	0.681	—

All correlations significant at 0.001 level.

cases where more than three of the six components were missing, the case was dropped. The nations with one, two, or three components missing have missing values estimated, using least squares procedures.[10] Relatively few values had to be estimated. For example, less than 8% of the values necessary to compute the six components of the 1965 political democracy index needed to be estimated.[11]

Confirmatory Factor Analysis

The correlations between the 1965 and 1960 indicators are presented in Table 2. The correlations for the 1965 indicators are above the main diagonal, and those for the 1960 indicators are below the diagonal. All the correlations are relatively high, as is expected if the indicators are all measuring political democracy.

The hypothesis that the six indicators are measuring a single construct can be evaluated with confirmatory factor analysis. Confirmatory factor analysis allows the researcher to specify the relationship between unmeasured variables and their indicators, and to see if the hypothesized structure is consistent with the data (Jöreskog, 1969; 1974; 1978). In this case, it is hypothesized that the unmeasured variable—political democracy—is "driving" all six measures.

This hypothesis was tested using the covariance matrix of the six components of POLDEM for 1960. Details of the factor analysis are in Appendix 1. Figure 1 is a

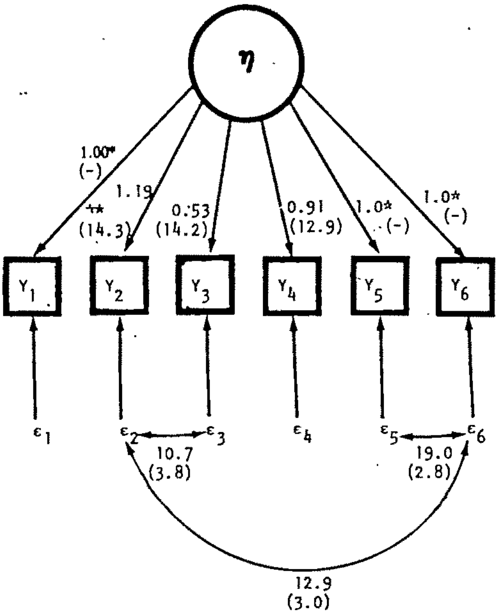


Figure 1. Confirmatory Factor Analysis of Six Components of Political Democracy, 1960 (N = 113)

* constrained parameter.

** t statistic, all estimated parameters significant at 0.01 level.

---

political democracy are possible for those countries at or near the end points. The countries which score near 100 still have political power concentrated in an elite, and generally the elite underrepresents females and minorities of the country. Limited though it may be, the nonelite in countries that score near the bottom of the index still have some political power.

[10] Four of the predictor variables are from Banks and Textor (1963): freedom of the press, constitutional status of regime, representative character of regime, current electoral system, and freedom of group opposition. A fifth variable used for estimation purposes was Lowenstein's (1967) freedom of the press score as given in Taylor and Hudson (1971). The regression equations used to estimate the individual indicators were usually good predictors. Most had multiple correlation coefficients of 0.85 or over. Conceptually, the variables used to estimate the missing indicators were quite similar to those used in the index. All these factors lead to added confidence in the accuracy of the estimation procedure.

[11] Each country required seven values to compute the index: two for the legislative selection component, and one for each of the remaining five components. Since for 1965 there are 123 countries, the total number of values needed is 123×7 or 861. Less than 8% of these values were estimated. Over half the countries that had any estimated values needed only *one* of the six components estimated.

path diagram of the model with the best fit ($\chi^2_8 = 5.78$, $p = 0.6716$). In conformity with Jöreskog's (e.g., see Jöreskog, 1974) path notation, a circle represents the true unmeasured variable (η = political democracy). The boxes enclose the fallible indicators (Y_i's). The errors of measurement are represented by the ϵ_i's..

The hypothesis that "η has a significant effect on all six indicators" is consistent with the data. However, the initial assumption of independent errors of measurement is not supported (see Appendix 1). As Figure 1 shows, there is a significant positive relationship between the errors of measurement for three of the variables.

This finding is interesting for several reasons. First, virtually all indices used in cross-national research implicitly assume that the errors of measurement of each indicator in an index are independent of the errors of the other indicators. If POLDEM is representative of other cross-national indices, such an assumption may not be warranted. Second, positively correlated measurement errors suggest that a "halo" effect may exist for indicators measured by the same data source. For instance, if there is a positive error in measuring a country on one indicator of a concept, another indicator from the same source may also have a positive error for that country. Finally, the correlated errors of measurement are important because many of the formulas for computing the reliability of an index assume independent errors of measurement. Correlated measurement errors will bias estimates of reliability that do not take it into account (Maxwell, 1968; Werts et al., 1974:31).

For the purpose of this analysis, a reliability formula that allows correlated measurement errors is derived. Defining the reliability as the "true variance" divided by the total variance, the reliability formula is:[12]

$$\text{reliability} = \frac{(\Sigma\lambda_i)^2 \text{ Var }(\eta)}{(\Sigma\lambda_i)^2 \text{ Var }(\eta) + \Sigma \text{ Var }(\epsilon_i) + 2\sum_{i<j}\Sigma \text{ Cov }(\epsilon_i\epsilon_j)} \qquad (1)$$

where i,j = 1,2,. . ., total number of indicators.

The last term of the denominator takes into account the covariance of the errors of measurement. If all errors of measurement are independent, then this term drops out.

The formula is a conservative estimate of the reliability because it uses as a measure of the true variance only that variance associated with η. Indicator-specific variance or variance associated with the correlated errors of measurement is not included in the true variance estimate. This generally will reduce the estimated true variance and reduce the estimated reliability of the index. Thus, if the index is estimated to have high reliability under these restrictive conditions of defining the true variance, then other estimates of reliability that use less restrictive definitions can be expected to be higher.

Formula (1) is used to estimate the reliability of the equal weighted sum[13] of the six indicators that form POLDEM. Substituting the sample estimates of the quantities in (1) from the confirmatory factor analyses, the 1960 and 1965 indices of political democracy have estimated reliabilities of 0.89 and 0.88, respectively.[14] By the usual standards of index construction, this is a high degree of reliability.

Alternatively, a weighted, rather than a simple, sum of the indicators could be used to form POLDEM. There are a number of different ways to derive a weighting scheme, the most popular being those using factor analytic techniques to estimate factor scores. To investigate possible differences between the simple-sum and factor-weighted indices, a regression-based estimate of the factor score was formed. The estimated factor score correlates 0.979 with the simple-sum index. This extremely high correlation between the estimated factor score and the simple-sum suggests that, in terms of practical significance, there is little difference between these two scoring techniques. These results parallel those of Alwin (1973:209–10) who concludes:

---

[12] In a personal correspondence, Jöreskog states that this formula is correct—provided that the congeneric measurement model $Y_i = \lambda_i\eta + \epsilon_i$ holds, with η uncorrelated with all the ϵ_i's. Other reliability estimates using a different definition of true score variance are also possible but are beyond the scope of this article.

[13] The index is formed by POLDEM = $(Y_1 + Y_2 + Y_3 + Y_4 + Y_5 + Y_6)/6$. The simple sum is divided by 6, so that the theoretical range of the index is from 0 to 100.

[14] The estimated error variances for $Y_1 - Y_6$ in 1960 are: 15.02, 55.42, 8.68, 33.87, 107.21, and 37.52, respectively. Results of the 1965 factor analysis are available from the author.

> Until substantial evidence is generated to the contrary perhaps the wisest conclusions with regards to factor score estimation at this point is . . . that they are of no great practical significance owing to their inability to produce meaningfully different results.

There seems, then, to be little reason not to use the simple sum to form POLDEM.

Comparisons with Other Political Democracy Indices

The last section indicated that the Political Democracy Index (POLDEM) has high reliability. This is one important characteristic when choosing any measure for empirical research. But it is clearly not the only one. Practical considerations of choosing a measure for cross-national research include the availability of the measure for as wide or representative a sample and for as many time points as possible.

Table 3 provides a summary comparison of ten measures of democracy. The list is not exhaustive, but it includes some of the most important measures. By far the most commonly used indices are Cutright's (1963) and Jackman's (1973). In fact, Cutright's measurement procedure is part of Smith's (1969), Coulter's (1975), and Cutright and Wiley's (1969) indices. The other indices are not as frequently used by sociologists but should be familiar to researchers in this field of study.

The second column of Table 3 lists the number of countries for which these indices are available. The sample size ranges from a low of 20 for the Fitzgibbon-Johnson Index (Johnson, 1976; 1977) to a high of 123 countries for POLDEM. Smith's index with 110 countries is the

Table 3. Summary Comparison of Measures of Political Democracy

Political Democracy Measure	Sample Size	Reliability	Time Points (or Periods) of Measure	Number of Index Components	Comments*
Adelman and Morris (1971)	73 74	— —	1963–1968 1957–1962	3 3	LDCs only
Author's Index (POLDEM)	123 113	0.88 0.89	1965 1960	6 6	See text for details
Coleman (1960)	66	—	1960 (?)	1	LDCs only, 3-rank scale
Coulter (1975)	85	—	1950–1970	3	Cutright (1963) as part of index
Cutright (1963)	77	—	1940–1960	2	African nations excluded
Cutright and Wiley (1969)	40	—	1927–1936 1937–1946 1947–1956 1957–1967	3 3 3 3	Similar to Cutright (1963) with addition of franchise measure
Jackman (1973)	60	—	1960	4	Communist countries excluded
Johnson (1977) Fitzgibbon-Johnson *Revised* Index	20	—	1945, 1950, 1955, 1960, 1965, 1970, 1975	5	Latin American countries only, excellent over-time coverage
Neubauer (1967)	23	—	1940–1960 (?)	4	Select sample of democratic nations, mostly MDCs
Smith (1969)	110	—	1946–1965	2	Cutright (1963) measure for one component, 20 assorted indicators for second component

* A number of these indices use indicators of stability and other measures of questionable validity. The comments in this table do not address this issue. See the text for discussion.

only index that approaches POLDEM in sample coverage. Jackman's (1973) and Cutright's (1963) indices have only 60 and 77 countries, respectively.

A number of these measures are limited to certain types of countries. For instance, Adelman and Morris (1971) and Coleman (1960) consider only LDCs. The Fitzgibbon-Johnson Revised Index is only for Latin American countries. Neubauer (1967) develops his measure for only relatively democratic nations (mostly MDCs). If the researcher wishes to restrict his/her attention to countries of a particular type (e.g., LDCs), then one or more of these indices may be suitable. But if the generality of hypotheses or the comparison of countries in different regions or at different levels of development is sought, then these indices will not do.

The third column of Table 3 lists the reliability of the indices. Unfortunately, none of the indices except POLDEM reports an estimate of reliability, so it is not possible to compare, directly, these measures. However, the indirect evidence that exists suggests that POLDEM compares very favorably with these indices.[15]

The temporal coverage of the indices of political democracy is also an important consideration. Table 3 shows that the Fitzgibbon-Johnson Revised Index covers the most time points. It is available for seven points of time over a 30-year period (1945–1975). The primary drawback is that the index is limited to Latin American countries. Adelman and Morris's (1971) measure is available for two five-year periods of time (1957–1962 and 1963–1968) but it is available for LDCs only.

Cutright and Wiley's (1969:27) measure extends the furthest back in time. The measure is available for four ten-year periods from 1927–1967. However, the sample is restricted to 40 nations that were self-governing and that continued to be self-governing during the full 40-year period. This criteria leads to the exclusion of many African and Asian nations from their sample.

The only index in Table 3 that is available for more than one time period, and is not restricted to only LDCs or Latin American countries (or some other group), is POLDEM. POLDEM is available for a diverse sample of countries in 1965 and 1960.[16] Although this is a short interval, an examination of the index values in Appendix 2 shows that some change did occur in a number of countries.

In summary, a comparison of POLDEM with nine other measures shows that it is the only index that reports an estimate of reliability. Given the magnitude of the coefficient and the number of components in the index, it is at least as reliable as the other measures. In addition, POLDEM is available for a greater number of (and a more heterogeneous sample of) countries than are the others. Two of the measures are available for more time points (or periods) than POLDEM, but these indices are limited in sample coverage.

Although these preceding considerations are important in empirical analyses, the overriding criterion must be the validity of the index. Earlier in the discussion, I questioned the validity of several indicators (e.g., political stability) used in other measures of political democracy. The six components of POLDEM were chosen to measure popular sovereignty (as reflected in electoral processes) and political liberties. The indicators appeared to have "face" validity. Yet, face validity is only a first step in the validation process.

The moderate-to-high intercorrelations between the components provide support for the internal validity of the index. But an internal check on validity is not a complete substitute for external checks. One method used to evaluate the external validity of this measure is correlating the scale with other indices that are intended to measure political democracy. The limitations of some aspects of these other measures were pointed out in an earlier part of this article. However, the index devised here would be suspect if it did not have positive and significant correlations with most of these previous measures. Table 4 presents the correlations of POLDEM with a number of previous indices appearing in the literature.[17]

---

[15] Two factors that affect the reliability of an index are the number of components and the average correlation of the components in the index. The next to the last column of Table 3 shows that POLDEM has more components than the other measures. The intercorrelation of components is not reported for many of these indices. An exception is Coulter (1975). The average correlation of his three components is less than 0.4, considerably less than the 0.7 average correlation of the components in POLDEM.

[16] Five of the six indicators used in this democracy index are also available for 1950 and 1955, so that the democracy measure could be constructed for an even-longer time period. However, the sample size available for these earlier years is much less than for the more recent time points.

Table 4. Pearson Correlations of POLDEM with Other Political Democracy Measures

Political Democracy Index	Correlation With Author's Political Democracy Index*	Number of Cases
Adelman and Morris (1971)		
1963–1968	0.68**	70
1957–1962	0.69	65
Coleman (1960)	0.69	52
Coulter (1975)	0.74	82
Cutright (1963)	0.76	73
Cutright and Wiley (1969)	0.85	40
Jackman (1973)	0.79	55
Johnson (1976) 1965	0.90**	20
1960	0.89	20
Lipset (1963)		
European	0.63	28
So. American	0.58	20
Neubauer (1967)	0.46	23
Smith (1969)	0.79	104

* All correlations are with the 1960 political democracy index (POLDEM) except those marked ** which are with the 1965 measure. All correlations are significant at the 0.01 level or better.

The magnitude of these correlations lends further support to the validity of POLDEM.[18] The lowest correlation (0.46) with Neubauer's (1967) democracy[19] is still a moderate relationship. The relationship between the Political Democracy Index (POLDEM) and the democracy measures devised for only LDCs is of particular interest since the greatest measuring inaccuracies are likely for developing countries. Table 4 shows that POLDEM correlates at about 0.7 with Adelman and Morris's 1963–1968 and 1957–1962 measures for developing countries. Considering the political change occurring during this period, and the fact that their measure is aggregated over a five-year period, this is a relatively strong relationship. POLDEM also correlates about 0.7 with Coleman's three-rank scale for LDCs and about 0.6 with Lipset's simple dichotomy measure for South American countries.

The highest correlations in Table 4 are between POLDEM and the Fitzgibbon-Johnson Revised Indices for 1965 and 1960. The correlation for both time points is about 0.9. The Fitzgibbon-Johnson index is only for Latin American countries. This specialization in Latin America, combined with its long history (since 1945), suggests that it is one of the best available measures for a region of developing countries. The high correlation of POLDEM with this measure further supports the use of the POLDEM for the analysis of LDCs.[20] POLDEM also correlates highly (0.74 or more) with the indices of Coulter, Cutright, Cutright and Wiley, Jackman, and Smith.

---

[17] A correlation matrix for POLDEM, Jackman's (1973), and Cutright's (1963) measures was also computed. For the 47 countries with data on all three measures, POLDEM correlated 0.81 with Cutright's index and 0.79 with Jackman's, while Cutright's measure correlated 0.61 with Jackman's.

[18] Considering the previously discussed limitations of certain indicators of the other indices and the fact that they are not all for the same time period, the magnitude of these correlations is impressive. Part of the reason for this is that at least some parts of the other indices are similar (and, in one case, the same) as are one or two of the components used in the revised index. For example, Cutright's (1963) index uses whether or not the chief executive is elected as part of his political development index. This is similar to one of the components used in the Political Democracy Index. Similarly, both the Political Democracy Index and Jackman's measure use the electoral irregularity variable.

[19] Part of the reason for the moderate correlation of Neubauer's measure with POLDEM is a result of the unusual indicators selected for his index (e.g., "information equality," as measured by the relative number and circulation of newspapers in capital cities; and "competition," as measured by the percentage of the time period in which the dominant party held office (1967:1005). This index does not seem to be for a single time *point* only but, rather, for a longer time *period*, so stability factors may be built into it. For an extensive critique of Neubauer's index, see May (1973). Additional criticism may be found in Cutright (1968). Also, the countries selected by Neubauer were all relatively democratic, restricting the variability of POLDEM. This may have led to a lower correlation than would be found if Neubauer had also included relatively nondemocratic countries in this sample. For a discussion of how the magnitude of the correlation coefficient depends on the range of variability in both variables, see Blalock (1972:381–3).

[20] Wendy Bhattacharyas (forthcoming) has also devised an index of political democracy for devel-

Given the magnitude of the relationships, one is tempted to conclude that it does not matter which measure is used because they are so closely related.[21] This argument suggests that the validity issues raised by other researchers, and here, are not important empirically. Such a view is suggested by Rubinson and Quinlan (1977:614). They state that, for all practical purposes, Jackman's and Cutright's measures are empirically indistinguishable: "... Jackman and Cutright indexes are so empirically interrelated that the difference between them is not sufficient to account for the different empirical results in their studies." (p. 614). This view conflicts with Jackman (1975:86–7), who claims that the incorporation of political stability into Cutright's (1963; 1967) measure has led to a spurious relationship between political democracy and social equality.

Since there is no agreement on whether the various democracy measures are empirically interchangeable, it is worthwhile to pursue this issue here. Several points are worth mentioning. First, the moderately high correlation between these measures can be overemphasized. For example, the correlation of 0.76 between POLDEM and Cutright's (1963) measure still means that over 40% of the variance of each measure is unexplained by the other. Although a part of this 40% may be explained by random measurement error, there is still a considerable fraction that could be accounted for by more systematic influences (e.g., Cutright's measure incorporating political instability in its construction).

A second factor to consider is that an association between variables does not necessarily mean that they are measuring the same concept. Two variables may also have a high correlation because part (or all) of one variable is causally related to the other. Or both variables may be spuriously related to some third set of factors. Since many of the politically democratic countries are also stable, the incorporation of stability into Cutright's (1963) measure of democracy does not necessarily mean that his measure will correlate low with other measures of democracy (e.g., Jackman's [1973] measure). But these arguments are still abstract and do not tell whether, *empirically*, the incorporation of stability distinguishes one index from another.

One empirical test is as follows.[22] If Jackman's (1973) index (JACDEM) and Cutright's (1963) index (CUTDEM) are both measuring the same concept of political democracy, then they should be empirically indistinguishable. If these indices are empirically indistinguishable, then most of the systematic variance in each index should be accounted for by the other index. In this case, the regression of one index on the other index and on some other variables should result only in a significant coefficient for the democracy index and not for any other variables in the regression.

Jackman (1975) and others have argued that Cutright's (1963) index (CUTDEM) is misleading because of its heavy reliance on political stability in its construction. If this argument is correct, then the regression of CUTDEM on JACDEM and a measure of political stability (POLSTAB) should result in a significant coefficient for

---

oping countries. The measure is for two time periods, 1959–1962 and 1963–1966. The first correlated 0.88 with the 1960 POLDEM and the second correlated 0.89 with the 1965 POLDEM.

[21] Of course, the sample and time coverage of some of the indices may lead to choosing one over another.

[22] The possibility of testing these validity arguments within a LISREL model was considered, but was decided against for several reasons. First, LISREL is a Full Information Maximum Likelihood (FIML) technique which is particularly sensitive to specification errors. Since this technique uses the information contained in all the equations in the model, a misspecification in one part has its effects spread throughout the model. Given the rather limited specification in the arguments presented here, a FIML technique may not be justified. A second, practical, consideration is that the components used in the other indices are not readily available. Third, even if the indicators for each index were available, there is a missing data problem which would severely reduce the sample size if all indicators were considered simultaneously. Such a small sample may not provide accurate estimates of relationships. Finally, the technique that is used has the advantage of being relatively simple and clearly demonstrates the effects of combining stability with political democracy.

POLSTAB even when JACDEM is controlled. On the other hand, if Rubinson and Quinlan (1977) are correct, once JACDEM is controlled, POLSTAB should not have a significant effect on CUTDEM.

The number of years, up to 1970, that the present constitutional form of a government has been in effect is used as a measure of political stability (Taylor and Hudson, 1971). The natural log of this variable is used to reduce skewness. This variable is referred to as POLSTAB. The regression of CUTDEM on JACDEM and POLSTAB results in:

$$\begin{aligned} \text{CUTDEM} = {} & 32.3 \\ & (3.64) \\ & + 0.211\ \text{JACDEM} \\ & \quad (0.057) \\ & + 2.23\ \text{POLSTAB} \\ & \quad (0.739) \end{aligned} \tag{2}$$

$$(\text{Standard Errors})\ R^2 = 0.48 \quad N = 48$$

Both JACDEM *and* POLSTAB are significant at greater than the 0.0001 level.[23] This means that—even controlling for the level of democracy as measured by Jackman—political stability has a significant association (not necessarily causation) with Cutright's index.

This difference in indices is further illustrated by regressing JACDEM on CUTDEM and POLSTAB. If JACDEM does not confound stability in its construction, then POLSTAB should not have a significant relationship once CUTDEM is controlled. The following regression tests this hypothesis:

$$\begin{aligned} \text{JACDEM} = {} & 2.85 \\ & (13.8) \\ & + 1.10\ \text{CUTDEM} \\ & \quad (0.298) \\ & + 1.97\ \text{POLSTAB} \\ & \quad (1.83) \end{aligned} \tag{3}$$

$$(\text{Standard Errors})\ R^2 = 0.39 \quad N = 48$$

In Equation (3) POLSTAB does not have a significant positive association as was found in Equation (2). This provides strong evidence that political stability is confounded with political democracy in Cutright's index.

Further evidence that stability has an empirically discernible effect on Cutright's measure is found when the Political Democracy Index (POLDEM for 1960) is substituted for JACDEM:

$$\begin{aligned} \text{CUTDEM} = {} & 32.9 \\ & (1.90) \\ & + 0.188\ \text{POLDEM} \\ & \quad (0.024) \\ & + 1.90\ \text{POLSTAB} \\ & \quad (0.622) \end{aligned} \tag{4}$$

$$R^2 = 0.63 \quad N = 73$$

$$\begin{aligned} \text{POLDEM} = {} & -65.0 \\ & (13.7) \\ & + 2.45\ \text{CUTDEM} \\ & \quad (0.317) \\ & + 1.77\ \text{POLSTAB} \\ & \quad (2.38) \end{aligned} \tag{5}$$

$$R^2 = 0.58 \quad N = 73$$

Once again controlling for democracy as measured by POLDEM, POLSTAB still has a significant positive association with CUTDEM. But POLSTAB does not have a significant association with POLDEM once CUTDEM is controlled.

The incorporation of stability in Cutright's measure can have consequences for theory testing. If a hypothesized cause of political democracy is related to political stability, but not to democracy, the use of a measure that confounds stability with democracy may result in a spurious significant relationship. To illustrate, consider the hypothesized relationship between the time of development and political democracy. De Schweinitz (1964), Moore (1966), and a number of other social scientists have hypothesized that the historical conditions conducive to democracy have steadily declined, over time. Thus, the early developers are more likely to be democratic than are the late developers. Those countries that have been developing the longest are generally the most stable. Therefore, a measure of "democracy" that also measures stability increases the likelihood of a positive relationship because of the relationship between stability and the number of years a country has been developing (Bollen, 1979).

To demonstrate this problem, three re-

---

[23] Significance is determined with a two-tail t-test.

Table 5. Regression of Author's, Jackman's, and Cutright's Measures of Political Democracy on Timing of Development and Level of Development

Independent Variable	Author's (POLDEM)	Jackman (1973) (JACDEM)	Cutright (1963) (CUTDEM)
	Dependent Variable		
Timing of Development	0.026 (0.054)[a]	−0.054 (0.048)	0.053* (0.021)
Level of Development (In energy cons. per capita, 1965)	10.20* (1.93)	9.74* (2.03)	2.97* (0.870)
R^2	0.44	0.37	0.54
N	99	57	60

[a] (standard error).

* Significant at 0.01 level.

gressions are run. The independent variables in each are the same: "l" in energy consumption per capita, 1965, as a measure of economic development; and Black's timing of development (Taylor and Hudson, 1971). The dependent variables are Jackman (JACDEM), Cutright (CUTDEM), and POLDEM. If the use of stability in Cutright's (1963) measure has no empirical consequences, the results of the regression should be similar to that using the two other measures. However, if the use of stability does lead to a spurious effect, then it is possible for the timing of development to have a significant relationship with Cutright's measure, but not with the other two. The results of these regressions are reported in Table 5.

Table 5 indicates that both the author's and Jackman's (1973) measures have *no* significant relationship with the timing of development, once the level of development is controlled. But Cutright's (1963) measure leads to a positive and significant relationship with development timing. This supports the hypothesis that the incorporation of stability in Cutright's measure can lead to spurious relationships with other variables that are related to stability, but not to democracy.[24]

More generally, these results conflict with Rubinson and Quinlan's (1977) claim that Cutright's (1963) and Jackman's (1973) measures are so closely related that the use of either will lead to empirically similar results. Rather, my analysis supports Jackman's claim that the incorporation of political stability in measures of political democracy may lead to spurious findings. This does not mean that differences in results always occur, but it does suggest that the selection of the indicators used in democracy measures has empirical consequences.

Discussion

Few concepts in the social sciences can be measured simply and directly. The concept of political democracy is no exception. As an abstract and complex concept, there are a large number of potential indicators that indirectly represent it. Yet each indicator is a fallible measure of political democracy.

In the ideal case, a random sample of all possible indicators of political democracy could be chosen, selecting a sufficient number to reach the desired reliability. Unfortunately, this is never the case. A number of desirable indicators of political democracy were not available; for example, the composition of the ruling elite and how representative they are of the general population, the extent of political discrimination, the mobility in-and-out of the political elite, the extent to which policy decisions are in accordance with the "will" of the people, and the real franchise (not the "legal" franchise) of the population.

In contrast to the need for *additional valid* indicators of political democracy, much of this article has argued against the indiscriminate use of other political indicators. Many of the conflicting results of empirical studies are likely to be a function of the indicators of political democracy used. For instance, Cutright's

---

[24] For a fuller discussion of development timing and political democracy, see Bollen (1979).

(1963; 1967) reliance on political stability in his index makes it difficult to determine whether political democracy or stability has an effect on income inequality. Other researchers have combined indicators of voter participation, socialist party strength, or party fractionalization—as well as stability—into indices of political democracy. I have argued that these concepts are important in their own right and should be kept out of indices of political democracy. Treating these concepts theoretically and operationally separate allows the researcher to determine if, for instance, a democratic political structure, high political participation, or some combination of high participation and democracy reduces inequality. Similarly, the impact of political stability on income inequality, independent of political democracy, can be determined *if* these concepts are measured separately and are *not* combined into a single index.

The index of political democracy presented here is designed to measure popular sovereignty (as represented in electoral processes) and political liberties. It does not consider political stability, voter turnout, socialist party strength, or labor union strength in its construction. Appendix 2 lists the index values for 1965 and 1960.[25]

It must be remembered that these are observed scores and not true scores. Small differences between countries on the index do not necessarily mean that the actual level of political democracy differs. For example, in 1965 Sudan scores 37.9 and Togo scores 45.5. This does not mean that the *true* political democracy score of Togo is greater than Sudan. Due to measurement error, each true score gives rise to a number of different observed scores. So it is possible that Sudan and Togo have the same true score even though their observed scores differ. Of course, the greater the distance between observed scores, the more likely two countries' true scores differ. But for smaller differences the researcher should not overinterpret these or any other values.

The validity of the political democracy index was explored in several ways. First, the face validity of each component was discussed. Only those measures that corresponded to the theoretical definition of political democracy were selected. Second, the relatively high intercorrelation of the indicators provided some evidence of the internal validity of the indicators. Furthermore, a confirmatory factor analysis supported the hypothesis that the assumed latent political democracy variable has a significant effect on all six components. Finally, the moderate-to-high correlation of the index with other proposed measures of political democracy provided support for its external validity. Further discussion and tests of the validity of this index are desirable. Perhaps the ultimate validity test is the utility of the index in cross-national research.

APPENDIX 1

Confirmatory Factor Analysis of the Six Indicators of Political Democracy

The six components of POLDEM are assumed to be caused by a single latent variable (i.e., political democracy) and random errors of measurement. This structure is represented in Equation (A1):

$$\begin{bmatrix} Y_1 \\ Y_2 \\ Y_3 \\ Y_4 \\ Y_5 \\ Y_6 \end{bmatrix} = \begin{bmatrix} \lambda_1 \\ \lambda_2 \\ \lambda_3 \\ \lambda_4 \\ \lambda_5 \\ \lambda_6 \end{bmatrix} \eta + \begin{bmatrix} \epsilon_1 \\ \epsilon_2 \\ \epsilon_3 \\ \epsilon_4 \\ \epsilon_5 \\ \epsilon_6 \end{bmatrix} \quad (A1)$$

In Equation (A1) Y_1 through Y_6 represent the six indicators of political democracy as defined in Table 1 of the text. The Y_i's are assumed to have a multivariate normal distribution.[26] The unmeasured political democracy variable is represented by η.

Measurement errors in the variables are represented by ϵ_1 to ϵ_6. These errors of measurement are assumed, independent of η. All variables are measured as deviations from their means and it is assumed that the E (E_i) is zero.

Figure A1 is a path diagram of this structure.

---

[25] The index values are listed according to the Russett et al.'s (1968) Standard Country Code, to facilitate the merger of the data with any others of a number of cross-national data sets (e.g., Taylor and Hudson, 1971) that also use this country code.

[26] The assumption of a multivariate normal distribution is probably not met by this data. Part of the reason for this is the relatively few values that some of the indicators take. The robustness of the model to the violation of this assumption is not well understood. Stapleton (1977:62) suggests that LISREL estimates still have merit even when this assumption is violated. However, further research is needed:

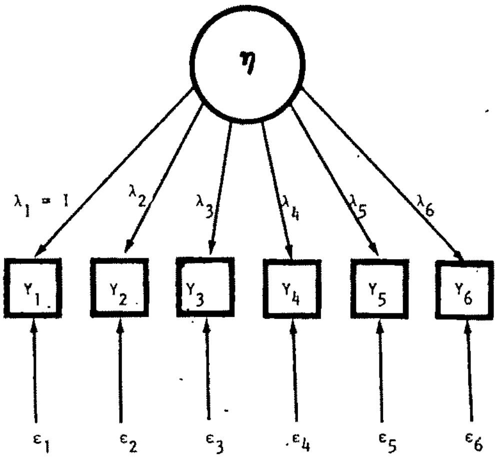


Figure A1. Initial Specification of Political Democracy (η), Determining Six Components ($Y_1 - Y_6$)

The λ_i's summarize the expected change in the Y_i's for a one-unit change in political democracy (η). A one-unit change in η, for instance, is expected to change Y_2 by λ_2. The scale for measuring η is arbitrary, but should be fixed to estimate the model. To resolve this indeterminacy of scale, λ_1 is set equal to 1. This sets the scale of η to Y_1 and allows estimation of the model (Sörbom, 1975:140). For this initial specification, the errors of measurement are assumed to be independent ($E(\epsilon_i\epsilon_j) = 0$ for $i \neq j$).

The model represented in Figure A1 is estimated using the covariance matrix[27] of the six indicators for 1960. A χ^2 value is calculated to evaluate the fit of the model. The probability level (P) of the χ^2 is "the probability of getting a χ^2 value larger than the value actually obtained, given that the hypothesized pattern is true. Thus, small values of P correspond to poor fit and large values to good fit" (Jöreskog, 1969:195).

The χ^2 (9 degrees of freedom) of the structure in Figure A1 is 38.92 with P less than 0.0001. Although all the estimated λ_i's are highly significant, the overall fit is not good. The poor fit of this initial estimate is probably a result of improper restrictions in the hypothesized structure. By relaxing some of these restrictions, improvement in fit may result. There are several empirical techniques for selecting which restrictions to relax (see Sörbom, 1975; Long, 1976), but as Saris et al. (1978:157) state: "Corrections of the model on theoretical grounds would generally be preferred. . . ."

Reconsidering the model diagrammed in Figure A1, the most questionable assumption is probably that of the independence of the errors of measurements. This means that any error of measuring legislative selection (ϵ_6), for instance, is not related to the measurement error of executive selection (ϵ_5), or the measurement errors of any of the other variables. This may not be realistic. Since freedom of group opposition (Y_2), executive selection (Y_5), and legislative selection (Y_6) are all constructed by the same source (Banks, 1971), the errors of measurement may be correlated. The use of freedom of group opposition (Y_2) and freedom of the press (Y_1) in forming the government sanction variable (Y_3) suggests that the measurement error of the last variable (ϵ_3) should be freed to estimate its possible correlation with ϵ_1 and ϵ_2. This revised structure is represented in Figure A2.

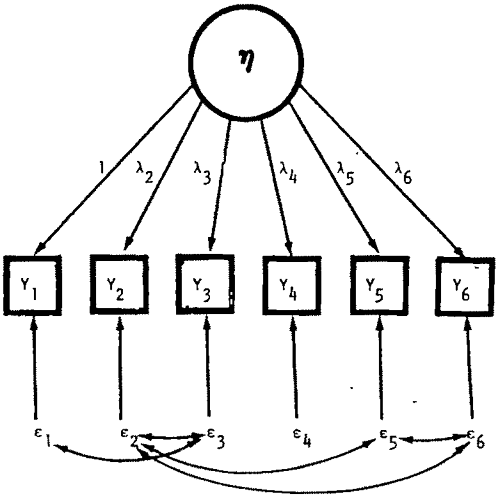


Figure A2. Political Democracy (η) Determining Six Components ($Y_1 - Y_6$), Allowing Certain Correlated Measurement Errors

Estimation of this model results in a χ^2 (4 d.f.) of 4.62 with a probability of 0.3282. The total fit is good but an important question is whether its fit is better than of the model of Figure A1. The difference in χ^2 values and degrees of freedom of Figure A1 and Figure A2 forms a new χ^2 value that addresses this question. If the difference in χ^2 values is not significant, then the fit of the more restricted model with uncorrelated measurement errors (Figure A1) is not significantly different than the more general model of Figure A2. On the other hand, a significant χ^2 suggests that the restricted model of Figure A1 does not have as good a total fit as does Figure A2. In this case a χ^2 difference (5 d.f.) of 34.3 results. This is highly significant ($p < 0.001$), so the hypothesis of no significant difference in fit is rejected, indicating that Figure A2's fit is better than that of Figure A1.

Although the *total* fit of Figure A2 is good, the estimated covariances, $\hat{\sigma}_{\epsilon_1\epsilon_3}$ and $\hat{\sigma}_{\epsilon_2\epsilon_5}$ are not statistically significant at the 0.05 level. Therefore, the model is reestimated setting these covariances to zero. Figure A3 presents the resulting model.

---

[27] The lower half of the symmetric covariance matrix of Y_1 to Y_6 for 1960 is: 100.5, 104.2, 182.2, 45.4, 66.7, 33.1, 80.2, 90.4, 40.6, 106.5, 86.7, 103.3, 48.2, 84.4, 197.7, 86.7, 121.3, 49.3, 81.1, 108.2 and 127.7.

The χ^2 (6 d.f.) is 5.57 with a probability of 0.4731. In addition, all the estimated parameters are significant at the 0.05 level.

Figure A3 represents a more restricted model than Figure A2. The difference in χ^2 values of these two models helps determine whether the restricted model of Figure A3 has a significantly worse fit than the more general model of Figure A2. The difference of χ^2's (2 d.f.) is only 0.95, which is not even significant at the 0.95 level. Therefore, the model of Figure A3 is chosen over Figure A2.

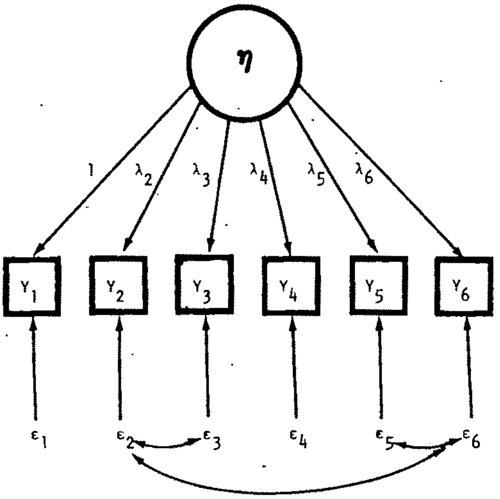


Figure A3. Political Democracy (η) Determining Six Components ($Y_1 - Y_6$) Reestimated, Setting Nonsignificant Covariances of Measurement Errors to Zero

One final restriction is tested. The estimates of λ_5 and λ_6 appear equal to λ_1 which is constrained to one. To test this additional restriction, the model is reestimated with the constraint that $\lambda_1 = \lambda_5 = \lambda_6 = 1$. Figure 1 in the text reports these final estimates. The χ^2 (8 d.f.) is 5.78 with a probability of 0.6716. The difference in χ^2 values (2 d.f.) of Figure 1 and Figure A3 is only 0.21 ($p > 0.99$). Thus, these two additional constraints lead to a model with two additional degrees of freedom, and a fit not significantly different from that of Figure A3.

Table A1 presents a summary of the χ^2 fit of the models discussed in the Appendix. Table A2 summarizes the comparisons of fit.

Table A1. Summary of Models in Confirmatory Factor Analysis of 1960 Indicators of Political Democracy

Model	χ^2	d.f.	Probability Level
Figure A1	38.92	9	0.000
Figure A2	4.62	4	0.328
Figure A3	5.57	6	0.473
Figure 1	5.78	8	0.672

Table A2. Comparison of Models in Confirmatory Factor Analysis of 1960 Indicators of Political Democracy

Model (Restricted vs. General)	Difference of χ^2	d.f.	Probability Level
Figure A1 vs. A2	34.3	5	Less than 0.001
Figure A3 vs. A2	0.95	2	Greater than 0.950
Figure 1 vs. A3	0.21	2	Greater than 0.990

APPENDIX 2

Political Democracy Index for 1965 and 1960, Listed by Russett et al.'s (1968) Standard Country Code

Standard Country Code	Country	Political Democracy 1965	1960
002	United States	92.4	94.6
020	Canada	99.5	99.9
040	Cuba	5.2	13.9
041	Haiti	20.7	35.0
042	Dominican Republic	38.8	20.6
051	Jamaica	90.1	91.0
052	Trinidad and Tobago	84.7	94.0
053	Barbados	99.6	—
070	Mexico	74.5	80.1
090	Guatemala	39.5	69.8
091	Honduras	50.0	70.1
092	El Salvador	72.1	53.5
093	Nicaragua	55.4	48.7
094	Costa Rica	90.1	91.3
095	Panama	76.9	75.4
100	Colombia	71.4	69.7
101	Venezuela	73.4	72.5
110	Guyana	53.1	—
130	Ecuador	44.6	84.3
135	Peru	87.0	83.4
140	Brazil	60.9	90.5
145	Bolivia	36.2	59.8
150	Paraguay	44.7	39.1
155	Chile	97.0	99.7
160	Argentina	52.6	62.7
165	Uruguay	99.6	99.8
200	United Kingdom	99.1	99.3
205	Ireland	97.2	94.8
210	Netherlands	99.7	99.9
211	Belgium	99.9	99.7
212	Luxembourg	97.7	100.0
220	France	90.8	89.7
225	Switzerland	99.7	99.6
230	Spain	10.4	10.7
235	Portugal	39.0	41.6
255	West Germany	88.6	88.0
265	East Germany	18.1	19.6
290	Poland	22.1	23.8

APPENDIX 2 (continued)

Political Democracy Index for 1965 and 1960, Listed by Russett et al.'s (1968) Standard Country Code

Standard Country Code	Country	Political Democracy 1965	Political Democracy 1960
305	Austria	97.1	97.2
310	Hungary	11.6	28.0
315	Czechoslovakia	20.5	22.3
325	Italy	96.8	97.0
339	Albania	18.2	20.4
345	Yugoslavia	50.8	42.6
350	Greece	82.8	88.0
352	Cyprus	87.0	85.7
355	Bulgaria	37.5	23.0
360	Romania	20.9	20.3
365	Soviet Union	18.2	20.4
375	Finland	97.3	97.3
380	Sweden	99.9	99.9
385	Norway	99.9	99.9
390	Denmark	99.9	99.9
395	Iceland	100.0	100.0
432	Mali	37.2	53.0
433	Senegal	53.7	49.4
434	Benin (Dahomey)	24.7	55.4
435	Mauritania	59.1	66.8
436	Niger	16.0	56.8
437	Ivory Coast	45.6	43.1
438	Guinea	37.3	—
439	Upper Volta	7.2	13.5
450	Liberia	45.6	47.2
451	Sierra Leone	74.6	93.9
452	Ghana	23.7	57.4
461	Togo	45.5	—
471	Cameroon	56.0	73.5
475	Nigeria	49.5	86.5
481	Gabon	44.7	63.4
482	Central African Republic	34.2	64.8
483	Chad	47.6	57.8
484	Congo (Brazzaville)	45.8	71.1
490	Zaire	38.5	50.0
500	Uganda	68.9	76.7
501	Kenya	58.2	71.1
510	Tanzania	63.3	—
516	Burundi	58.0	30.8
517	Rwanda	29.9	30.8
520	Somalia	77.0	85.7
530	Ethiopia	12.5	31.0
540	Angola	12.0	—
541	Mozambique	11.8	—
551	Zambia	67.4	—
553	Malawi	57.9	—
560	South Africa	58.9	64.7
580	Madagascar (Malagasy Republic).	83.0	91.7
600	Morocco	32.2	40.6
615	Algeria	27.2	—
616	Tunisia	63.6	83.7
620	Libya	34.4	31.0
625	Sudan	37.9	25.2
630	Iran	45.0	34.9
640	Turkey	76.4	59.1
645	Iraq	11.4	15.5
651	United Arab Republic	38.7	37.0
652	Syria	19.9	39.3
660	Lebanon	74.0	84.0
663	Jordan	30.8	31.9
666	Israel	96.8	94.6
670	Saudia Arabia	9.7	12.0
678	Yemen	6.5	11.9
700	Afghanistan	23.2	14.4
710	China	16.4	22.6
712	Mongolia	16.2	22.1
713	Taiwan	22.8	—
731	North Korea	21.0	21.0
732	South Korea	53.0	51.7
740	Japan	99.8	99.3
750	India	91.2	93.6
770	Pakistan	62.5	40.0
775	Burma	—	63.4
780	Sri Lanka	85.9	94.0
790	Nepal	29.2	39.7
800	Thailand	17.3	33.1
811	Cambodia	36.3	38.6
812	Laos	42.8	56.6
816	North Vietnam	33.1	31.2
817	South Vietnam	12.4	54.6
820	Malaysia	80.3	83.5
830	Singapore	76.9	81.2
840	Philippines	92.6	93.0
850	Indonesia	9.8	20.3
900	Australia	99.9	100.0
920	New Zealand	100.0	100.0

APPENDIX 2 (continued)

Political Democracy Index for 1965 and 1960, Listed by Russett et al.'s (1968) Standard Country Code

REFERENCES

Adelman, Irma, and Cynthia T. Morris

1971 A Conceptualization and Analysis of Political Participation in Underdeveloped Countries. Final Report, Part II. Washington: AID.

1973 Economic Growth and Social Equity in Developing Countries. Stanford: Stanford University Press.

Alwin, Duane F.

1973 "The use of factor analysis in the construction of linear composites in social research." Sociological Methods and Research 2:191–214.

Bachrach, Peter

1967 The Theory of Democratic Elitism. Boston: Little, Brown.

Banks, Arthur S.

1971 Cross-Polity Time Series Data. Cambridge: MIT Press.

Banks, Arthur S., and R. B. Textor

1963 A Cross-Polity Survey. Cambridge: MIT Press.

Bhattacharya, Wendy

Forthcoming Economically Underdeveloped Democracies—Deviants in the Theory of Socio-economic and Political Development. Ph.D. dissertation. Department of Sociology, Brown University.

Blalock, Hubert M.
1972 Social Statistics. New York: McGraw-Hill.
Bollen, Kenneth A.
1979 "Political democracy and the timing of development." American Sociological Review 44:572–87.
Coleman, J. S.
1960 "Conclusion: the political systems of the developing area." Pp. 532–81 in G. A. Almond and J. S. Coleman (eds.), The Politics of Developing Areas. Princeton: Princeton University Press.
Coulter, P.
1975 Social Mobilization and Liberal Democracy. Lexington: Lexington Books.
Cutright, Phillips
1963 "National political development." American Sociological Review 28:253–64.
1967 "Inequality: a cross-national analysis." American Sociological Review 32:562–78.
1968 "Communications." American Political Science Review 63:578–81.
Cutright, Phillips, and James A. Wiley
1969 "Modernization and political representation: 1927–1966." Studies in Comparative International Development 5:23–44.
Dahl, Robert
1956 A Preface to Democratic Theory. Chicago: University of Chicago Press.
de Schweinitz, Karl
1964 Industrialization and Democracy. Glencoe: Free Press.
Downs, Anthony
1957 An Economic Theory of Democracy. New York: Harper and Brothers.
Fox, Thomas G., and S. M. Miller
1965 "Economic, political and social determinants of mobility: an international cross-sectional analysis." Acta Sociologica 9:76–93.
Greenberg, Bradley
1961 "Additional data on variables related to press freedom." Journalism Quarterly 38:76–8.
Heilbroner, Robert L.
1974 An Inquiry into the Human Prospect. New York: W. W. Norton.
Hewitt, C.
1977 "The effect of political democracy and social democracy on equality in industrial societies: a cross-national comparison. American Sociological Review 42:450–64.
1979 "Reply to Stack." American Sociological Review 44:171–2.
Huntington, S. P.
1968 Political Order in Changing Societies. New Haven: Yale University Press.
Jackman, Robert W.
1973 "On the relation of economic development to democratic performance." American Journal of Political Science 17:611–21.
1975 Politics and Social Equality: A Comparative Analysis. New York: Wiley.
Johnson, Kenneth F.
1976 "Measuring the scholarly image of Latin American democracy, 1945–1970." Pp. 347–65 in James W. Wilkie (ed.), Statistical Abstract of Latin America, Volume 17. Los Angeles: UCLA Latin American Center.
1977 "Research perspectives on the revised Fitzgibbon-Johnson index of the image of political democracy in Latin America, 1945–75." Pp. 87–91 in James W. Wilkie and Kenneth Ruddle (eds.), Quantitative Latin American Studies, Supplement 6. Los Angeles: UCLA Latin American Center.
Jöreskog, Karl G.
1969 "A general approach to confirmatory maximum likelihood factor analysis." Psychometrika 34:183–202.
1974 "Analyzing psychological data by structural analysis of covariance matrices." Pp. 1–56 in D. H. Krantz and W. H. Freeman (eds.), Contemporary Developments in Mathematical Psychology Vol. II. Freeman and Company
1978 "Structural analysis of covariance and correlation matrices." Psychometrika 43:443–77.
Kornhauser, W.
1959 The Politics of Mass Society. New York: McGraw-Hill
Lenski, Gerhard
1966 Power and Privilege. New York: McGraw-Hill.
1976 "The Need for Reader-Access to the Measures of Variables Used in Quantitative Cross-National Studies." American Sociological Review 41:751.
Lerner, Daniel
1958 The Passing of Traditional Society. Glencoe: Free Press.
Lipset, Seymour
1959 "Some social requisites of democracy." American Political Science Review 53:69–105.
1963 Political Man. Garden City: Anchor Books.
Long, J. Scott
1976 "Estimation and hypothesis testing in linear models containing measurement errors." Sociological Methods and Research 5:157–206.
Lowenstein, Ralph L.
1967 "World Press Freedom, 1966." Freedom of Information Center Report No. 201. School of Journalism, University of Missouri at Columbia.
Maxwell, A. E.
1968 "The effect of correlated errors on estimates of reliability coefficients." Educational and Psychological Measurement 28:803–11.
May, J. D.
1973 "Of the conditions and measures of democracy." Morristown: General Learning.
McCrone, D. J., and C. F. Cnudde
1967 "Toward a communications theory of democratic political development." American Political Science Review 61:72–9.
Michels, Robert
1962 Political Parties. New York: Free Press.
Mills, C. W.
1956 The Power Elite. New York: Oxford University Press.

Moore, Barrington, Jr.
1966 Social Origins of Dictatorship and Democracy. Boston: Beacon.

Mosca, G.
1939 The Ruling Class. New York: McGraw-Hill.

Neubauer, Deane E.
1967 "Some conditions of democracy." American Political Science Review 61:1002–9.
1968 "Communications." American Political Science Review 62:581.

Nixon, Raymond B.
1960 "Factors related to freedom in national press systems." Journalism Quarterly 37:13–28.
1965 "Freedom in the world's press: a fresh appraisal with new data." Journalism Quarterly 42:3–15, 118–9.

Rubinson, R., and D. Quinlan
1977 "Democracy and social inequality: a reanalysis." American Sociological Review 42:611–23.

Russett, Bruce M., J. David Singer, and Melvin Small
1968 "National political units in the twentieth century: a standardized list." American Political Science Review 62:932–51.

Saris, W. E., W. M. de Pijper, and P. Zegwaart
1978 "Detection of specification errors in linear structural equation models." Pp. 151–71 in Karl F. Schuessler (ed.), Sociological Methodology 1979. San Francisco: Jossey-Bass.

Smith, A. K.
1969 "Socioeconomic development and political democracy." Midwest Journal of Political Science 30:95–125.

Sörbom, Dag
1975 "Detection of correlated errors in longitudinal data." British Journal of Mathematical and Statistical Psychology 28:138–51.

Stack, Steven
1978 "Internal political organization and the world economy of income inequality." American Sociological Review 43:271–2.
1979 "The effects of political participation and socialist party strength on the degree of income inequality." American Sociological Review 44:168–71.

Stapleton, David C.
1977 "Analyzing political participation data with a MIMIC Model." Pp. 52–75 in Karl Schuessler (ed.), Sociological Methodology 1978. San Francisco: Jossey-Bass.

Taylor, Charles L., and Michael C. Hudson
1971 World Handbook of Political and Social Indicators II. Inter-University Consortium for Political and Social Research. Ann Arbor: University of Michigan.
1972 World Handbook of Political and Social Indicators. 2nd ed. New Haven: Yale University Press.

Wences, Rosalio
1969 "Electoral participation and the occupational composition of cabinets and parliaments." American Journal of Sociology 75:181–92.

Werts, C. E., R. L. Linn, and K. G. Jöreskog
1974 "Intraclass reliability estimates: testing structural assumptions." Educational and Psychological Measurement 34:25–33.

Wilenski, Harold L.
1975 The Welfare State and Equality. Berkeley: University of California.

ORGANIZATIONAL PROPERTIES FROM AGGREGATE DATA: SEPARATING INDIVIDUAL AND STRUCTURAL EFFECTS*

JAMES R. LINCOLN
Indiana University, Bloomington

GERALD ZEITZ
SUNY, Stony Brook

American Sociological Review 1980, Vol. 45 (June:391–408)

In this paper, we argue that aggregate measures (means computed on distributions of individual scores) may be valid indicators of organizational properties which also enable an investigator to determine whether statistical relations among organizational measures arise from organization-level, as opposed to individual-level, causal processes. We review certain statistical aggregation issues as these pertain to organizational analysis, and we propose Hauser's path analytic model of analysis of covariance as a device for separating individual and structural effects. These methods are then applied to data gathered in a survey of 20 social service organizations. We specify and estimate a causal model wherein administrative intensity, lateral communications, and decentralization of decision making are endogenous variables. We find that a number of "total" effects on these properties mask quite different—and in some cases contradictory—processes at individual and organization levels. Among other inferences, we suggest that the organizational- and individual-level influences we observe on decentralization raise questions regarding certain widely accepted interpretations of this property. We recommend that analysts working with aggregate measures adopt similar procedures in order to fully exploit their data for the insights that may be gained into multilevel organizational processes.

Introduction

This paper combines a study of decision-making decentralization in social service organizations with a discussion of issues and opportunities posed by aggregating data on organizational members to produce measures of organizational structure. We think that aggregate measures can be valid indicators of organizational properties. And, unlike "global" measures, they permit dissection of observed relations between organizational traits into components attributable to organizational- and individual-level causal processes. Some background to our discussion may be found in the attempts to treat "structural" effects in organizational analysis by Blau (1957; 1960), Coleman (1959), and Tannenbaum and Bachman (1964). Yet their observations have had little sustained impact on organizational research; primarily, we think, because the methodology for separating individual and structural effects was at the time poorly understood (Hauser, 1970). The state of the art has advanced since then, however, and methods of partitioning aggregate variable relations into their organizational and individual components are today readily available. We use such methods in the present investigation to show that tests of structural theories could benefit from the insights these methods permit into the causal processes at multiple levels which generate patterns in organizational data.

These issues are of some general relevance, for a significant body of research on organizational structure has been built on findings obtained with aggregate data (Aiken and Hage, 1966; R. B. Duncan, 1972; Glisson, 1978; Hage and Aiken, 1967; Hall, 1963; Lawrence and Lorsch, 1967; Pennings, 1975). Approaches to the measurement of organizational properties were for some time bifurcated into what Pennings (1973) has called "subjective" and "objective" methods. The subjective method meant the collection of responses from individual members of organizations which, upon aggregation, yielded measures of structural properties for the organ-

---

* Direct all communications to: James R. Lincoln; Department of Sociology; Indiana University; Bloomington, IN 47401.

A previous version of this paper was presented at the 74th annual meeting of the American Sociological Association in Boston, August, 1979. We are indebted to Duane F. Alwin, Larry J. Griffin, David Knoke, and Maurice Garnier for helpful comments on a previous draft. They are in no way responsible for deficiencies that remain, however.

ization as a whole. Objective strategists performed content analyses of formal documents and key informant interviews to produce "global" measures—that is, those not reducible to individual traits (Lazarsfeld and Menzel, 1961).

While Pennings's (1973) concern lay with the convergent validity of these modes of organizational measurement, that is not our interest here. However, in the debate surrounding these methods, certain views were aired which are pertinent to the problems we address. Several critics of the subjective methodology expressed doubt that the averaged perceptions or traits of organizational participants could be meaningfully identified with structural properties of the organization (Hinings and Lee, 1971; Pennings, 1973; Tausky, 1967). We agree that some aggregate measures may indeed be of doubtful validity. But the same is true of global measures, and the validity issue does not turn on this distinction. It would be hard to improve upon the proportion of administrators in an organization as an indicator of administrative intensity. Yet this is an aggregate measure in our sense: the mean of the membership's distribution on a binary variable measuring occupancy or nonoccupancy of administrative positions.

Several writers have also alluded to problems of interpretation engendered by aggregation (Aiken and Hage, 1966; Pennings, 1973; Tannenbaum and Bachman, 1964). By computing correlations among averages of individual traits, can one be sure that the observed statistical relationships derive from organizational-, as opposed to individual-level, processes? For example, if the proportions of professionals and members who report participating in decisions are correlated over organizations, perhaps the only process at work is that of individual professionals influencing decisions more frequently than nonprofessionals. This being the case, one might hesitate to claim support for a more complex "structural" theory relating professionalization as an organizational attribute to decentralized decision making (Blau, 1968; Hall, 1968).

We think this is a legitimate concern. It frequently is relevant to the aims of research to know whether an empirical relation between organizational properties stems from an organizational or an individual process. But the fallacy here lies in assuming that such interpretive ambiguity occurs only when organizational properties are estimated using averaged members' traits or perceptions. In fact, these same questions concerning the locus of causation may legitimately be raised when organizational properties are measured with global indicators not derived from, nor disaggregable to, individual characteristics. In the case of aggregate measures, the analogous indicator at the individual level is readily apparent since its distribution is the raw material from which the organizational measure is derived. But the fact that a global measure does not have an immediately evident or accessible microcounterpart does not make it immune to questions concerning the level at which one's theoretical explanations should be pitched.

We see three dimensions to the level of analysis problem. These pertain to: (a) operational measures; (b) theoretical variables; and (c) relations between theoretical variables. Lazarsfeld and Menzel (1961) restricted their discussion to the measurement level and observed that "analytical" (aggregate) and global measures may be made of the same underlying property. It is clear, however, that not all individual attributes may be averaged to produce a theoretically meaningful organizational property; nor may all organizational properties have meaningful analogues for individuals. Job satisfaction, for example, is an individual sentiment only, and we think no purpose is served by aggregating satisfaction scores to produce an organizational mean (Aiken and Hage, 1966; Talacchi, 1960). Size and division of labor, on the other hand, are organizational properties which have no parallels as individual traits. The division of labor may, of course, express itself at the individual level in the range of skills or responsibilities involved in tasks. But the average breadth of work roles in the organization and its division of labor—the variety of work roles—are hardly equivalent concepts (but cf. Hall, 1963).[1] This example does serve, however, to illustrate the third dimension: the level at which a causal relation occurs. Division of labor might be thought to centralize decision making through an individual-level process. Differentiation of functions could narrow the scope of individuals' tasks, implying greater routinization and less autonomy and opportunity to exercise discretion. Aggregated to the organization level, these effects produce a skewed (that is, centralized) decision-making distribution.

Thus, a reasonable theory may specify that the intervening process producing an observed relation between organizational properties is wholly an individual one (Blau, 1957). At the opposite extreme is the case where such an individual-level process is conceived solely as a source of measurement error, confounding one's ability to estimate true relations between organizational traits. If multiple informants are asked to report on the same objective state of the organization (for example, on how many levels there are in the authority hierarchy), we can assume that any individual-level effects of their background characteristics (age, rank, sex, education) represent measurement bias. Note that it is *not* correct to assume, as Seidler (1974) does, that organization-level covariations between informant background traits and the reported property are necessarily symptomatic of bias. In a perceptive comment on Seidler's paper, McGranahan (1976) notes that the composition of the organization on informant background variables may reflect meaningful organizational properties belonging in one's theoretical model: education could be an indicator of professionalization, for instance. But to the extent that the process relating education to reported levels in the hierarchy is an individual one (more highly educated persons reporting more or fewer levels), such effects would be viewed rightly as errors to be held constant in evaluating relations among organizational variables.

It is important to stress, however, that the measurement error interpretation can incontrovertibly be applied only to cases where within-organization variation in informant reports is illogical, substantively. The earlier-cited example meets this criterion. Assuming a single hierarchy, informant disagreement as to its number of levels indicates error. But take a less obvious example. Informants might be asked whether they perceive the environment to be stable or changing (R. B. Duncan, 1972). In this case, intraorganizational variation in reports might be real variability in the task environments experienced by participants in different organizational positions (Roberts et al., 1978). As Comstock and Scott (1977) point out, organizational theory predicts effects on structure of task and environmental variability at individual, subgroup, and organization levels. It is, thus, not always easy to tell whether the organizational property in question is subject to real variability at the microlevel. Therefore, the optimal approach to these issues, we believe, is to separate individual and organizational effects whenever the data permit. Since aggregate measures of organizational properties readily permit such decomposition while global measures do not, this dictum favors an expanded role for aggregate measurement in organizational research.

Some Aggregation Issues

Our methodology for decomposing a relation between aggregate organizational

---

[1] This example underscores the ambiguity of the term "aggregate" in our discussion. We follow dominant usage and intend by it means, proportions, and other averages computed on distributions of individual (or other subunit) scores. Such central tendency measures are subject to a smilar interpretation as that applied to individual observations as the "level" of some variable. The level of decision-making participation, for instance, could refer to whether the individual participates or the proportion of individuals participating. As Lazarsfeld and Menzel (1961) noted, however, measures of dispersion or inequality computed on such distributions have the status of "global" indicators, for they permit no counterpart interpretation at the individual level. Comstock and Scott (1977), for example, measure differentiation in organizations with the index of qualitative variation, Σp_i^2, where p_i is the proportion of persons in the ith organizational position and the summation is over all positions.

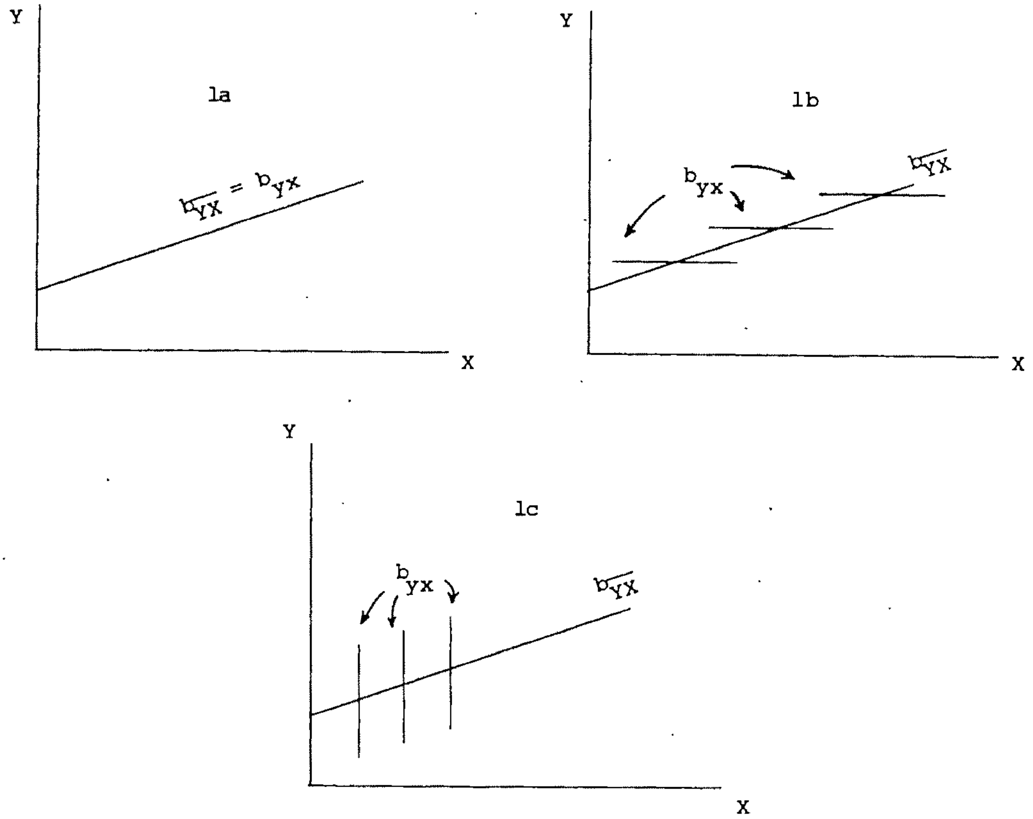


Figure 1. Examples of Possible Within- and Between-Organizations X, Y Relations

measures into its organizational and individual components rests on the established premise that aggregate variable relations can be shown to depend in certain ways on the relations between the disaggregated forms of the same variables (Hannan, 1971; Hauser, 1971:chap. 2). Let us illustrate. Figures 1a and 1b show that the same regression of organizational means, $b_{\bar{Y}\,\bar{X}}$, may arise in the presence of very different patterns involving variables X and Y within organizations. As we shall demonstrate, a proper interpretation of $b_{\bar{Y}\,\bar{X}}$ rests not only on its value and sign but also on a comparison between it and b_{yx}, the average within-organization slope.[2] An assumption required throughout is that the regression of Y on X within different organizations may be described by a common slope.

In Figure 1a, $b_{\bar{Y}\,\bar{X}} = b_{yx}$. If organizations selected or socialized members so that average levels of X varied between organizations, and a positive change in an individual's X produced a positive change in that person's Y, we would expect to observe this outcome. Thus, organizations might vary in the proportions of professionals they employ. Since professionals are powerful actors in organizational settings, average levels of influence should rise with professionalization. Note that the causal process relating professionalization to decentralization operates at the individual level. The only organizational "effect" is that which sorts members on X (professional status) by organization.

---

[2] $b_{\bar{Y}\bar{X}}$ and b_{yx} are the unstandardized coefficients from the regressions of $\bar{Y}_j$ on $\bar{X}_j$ and y_{ij} on x_{ij}, respectively. $\bar{Y}_j$ and $\bar{X}_j$ are the means of the jth organization on Y and X, while y_{ij} and x_{ij} are the deviations of individual members' scores from the means of their organizations. b_{yx} is an *average* within-groups (i.e., organizations) slope and may not describe the regression of y on x for any given j. We assume, however, that variation over j in the observed organization-specific slopes is within sampling error of b_{yx}. Tests for statistical interaction between organization and our independent variables upheld that assumption here.

Figure 1b illustrates a different state of affairs. Within an organization, to be a professional has no influence on one's decision-making involvement: the within slope, b_{yx}, is zero. Any regression of Y on X occurs through the organizational means. The implication here is that highly professionalized organizations distribute greater influence to professionals and nonprofessionals alike. Thus, professionalization is found to affect decentralization through a process which operates wholly at the organization level.

Suppose our interest lay not in the effect of professionalization but in that of size. Size, as we pointed out, is a global property which has no counterpart at the individual level. Thus, the regression of decision making on size could be plotted, as in Figure 1c. Since size is a constant for all individuals in the same organization, no within-organization regression may be contemplated, and the total regression is that relating means on Y to values of X. Similarly, Figure 1b would also describe the regression of a global measure—the Aston centralization scale, for example—where the data points are individuals (Pugh et al., 1968). Even though centralization might be thought to have an individual-level analogue (participation in decisions), the global measurement of this property means it is a constant for members of a common organization which may be associated only with average (mean) levels of professionalization.

Our concern is the converse of that addressed by a host of writers whose interest lies with aggregation bias in the use of group-level data to estimate variable relationships for individuals (Hannan and Burstein, 1974; Hammond, 1973; Burstein, 1978). As Firebaugh (1978) points out, the presence of such bias, which is measured by the difference in between- and within-group slopes, may testify to the existence of a causal process operating at the group level which is qualitatively different from that found at the individual level. If the goal is to generalize from aggregate observations to individuals, such group effects are a nuisance and confound cross-level inferences. But if the theory is couched at the group or organization level, evidence that the individual-level effects deviate from those observed for aggregates may be highly relevant to a test of it.

The group effects we allude to were first labeled "structural" effects by Blau (1960) but are more commonly called "contextual" effects (Hauser, 1970). Previous attempts to conceptualize and research contextual effects have in common a focus on how the group or organizational setting influences the attributes or behaviors of its individual members. Both the early applications of contextual analysis to organizational problems (Blau, 1960; Tannenbaum and Bachman, 1964), as well as the more sophisticated recent treatments of school effects on student attitudes and performance (Alexander and Eckland, 1975; Alwin and Otto, 1977; Hauser, 1971), take the individual to be the unit of analysis. This orientation is notably different from our concern with interpreting relationships among organizational properties which are measured with aggregate indicators. Consequently, while the methodology we employ is taken from the literature on contextual effects, our adaptation of it—to problems wherein the theoretical unit is the organization—calls for certain special manipulations which have no precedents in organizational research. We discuss the model underlying those manipulations, in the following section.

Contextual Analysis and the ANCOVA Model

A contextual effect traditionally has been presumed to exist if some individual level attribute, Y, displays a net association with the group mean on a predictor, $\bar{X}$, controlling for X, the individual's score on the predictor. Thus, one would wish to estimate the regression equation:

$$Y = a + b_{YX \cdot \bar{X}} + b_{Y\bar{X} \cdot X} + e \qquad (A)$$

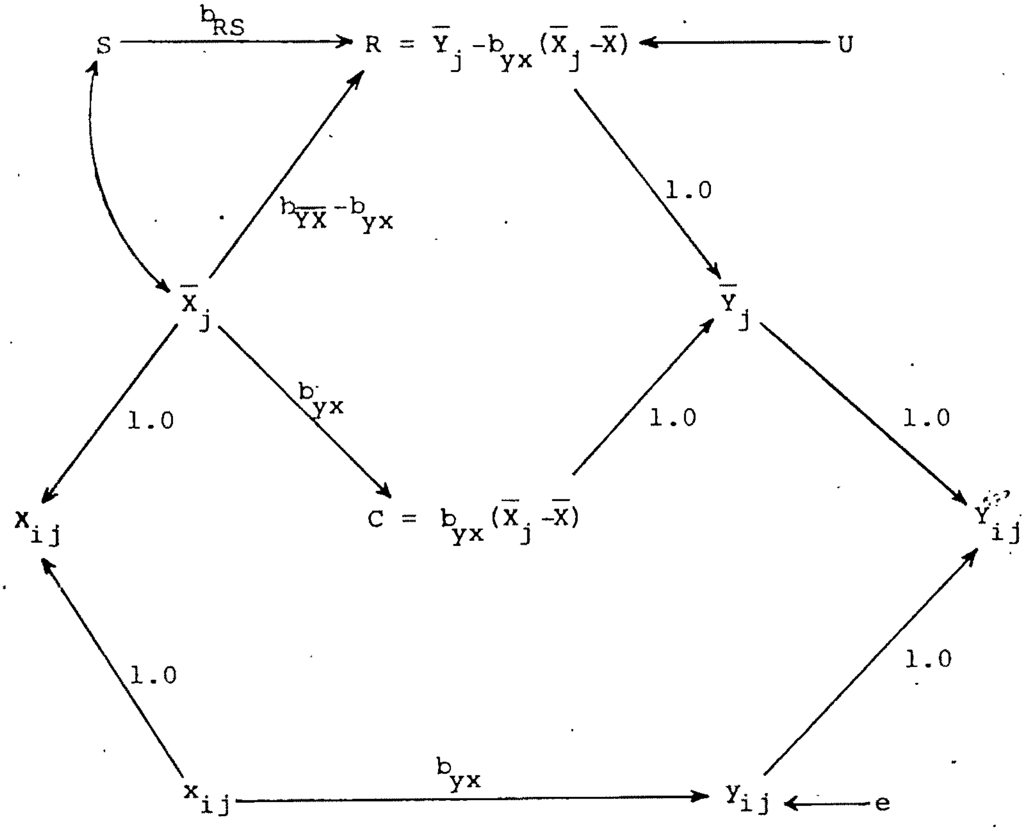
As Alwin (1976; see also Werts and Linn, 1971)has shown $b_{YX \cdot \bar{X}} = b_{yx}$, the average within-group slope, and $b_{\bar{Y}\bar{X} \cdot X} = b_{\bar{Y}\bar{X}} - b_{yx}$, the difference in the between and within-group slopes.[3] The contextual effect is measured, then, by the degree to which the group-level slope deviates from the individual-level slope within groups. This difference, as the contrast between our Figures 1a and 1b makes plain, also measures the extent to which theorizing in terms of group or organization-level causal processes is warranted by the data.

---

[3] We do not reproduce Alwin's derivations but they depend merely upon multiplying through (A) by $\bar{X}_j$ and X_{ij} and solving for $b_{YX \cdot \bar{X}}$ and $b_{Y\bar{X} \cdot X}$.

Because it takes the individual as unit of analysis, however, the standard regression method of evaluating contextual effects permits a less clear interpretation of how an aggregate relation derives from group and individual-level processes than does an alternative (if formally equivalent) approach. Furthermore, the modelling strategy to which we now turn has the added advantage of permitting one to grasp directly the implications of introducing global variables with no intraorganizational variation, as well as additional aggregate variables.

Duncan et al. (1972:195) and Alwin (1976) discuss the model which concerns us, but their treatments are basically elaborations on a theme best developed by Hauser (1971:chap. 2). Hauser's model is a path analytic conceptualization of analysis of covariance. Given two interval-scaled variables, X_{ij} and Y_{ij}, where i indexes individuals and j indexes groups, the total regression of Y_{ij} on X_{ij} may be decomposed into the component causal processes displayed in Figure 2. Although Hauser's treatment is in terms of path coefficients, our purposes are better served by concentrating on unstandardized coefficients. Y_{ij} and X_{ij} are shown to be completely determined by the sums of their within (x_{ij}, y_{ij}) and between ($\bar{Y}_j$, $\bar{Y}_j$) components. x_{ij} is by definition independent of $\bar{X}_j$ as y_{ij} is independent of $\bar{Y}_j$. Part of the effect of X_{ij} on Y_{ij} is through the path relating x_{ij} to y_{ij}, the within-group regression. The other part is through the effect of $\bar{X}_j$ on $\bar{Y}_j$. But note that this effect may itself be decomposed into two paths, and it is this decomposition which concerns us chiefly here. One such path, measured by b_{yx}, the within-



b_{RS}
S
$R = \bar{Y}_j - b_{yx}(\bar{X}_j - \bar{X})$
U
$b_{\bar{Y}\bar{X}} - b_{yx}$
1.0
$\bar{X}_j$
$\bar{Y}_j$
b_{yx}
1.0
1.0
1.0
X_{ij}
$C = b_{yx}(\bar{X}_j - \bar{X})$
Y_{ij}
1.0
1.0
x_{ij}
b_{yx}
y_{ij}
e

Figure 2. The Analysis of Covariance Model

groups slope, is mediated by a term denoted "C." C is the predicted value of $\overline{Y}_j$ from its constrained regression on $\overline{X}_j$; the constraint being that $b_{\overline{Y}\overline{X}} = b_{yx}$. The second path is mediated by "R," where $R = \overline{Y}_j - C$. Thus, R is the residual in $\overline{Y}_j$ from its constrained regression on $\overline{X}_j$. R is a normal byproduct of analysis of covariance: the group mean of Y adjusted for composition on X (Schuessler, 1969).

As Alwin (1976) notes, the effect of $\overline{X}_j$ on C is measured by b_{yx}, the within-groups slope. If $b_{yx} = b_{\overline{Y}\overline{X}}$, R would be the residual from the ordinary least-squares regression of $\overline{Y}_j$ on $\overline{X}_j$ and thus would be uncorrelated with $\overline{X}_j$. Any observed effect from $\overline{X}_j$ to R, therefore, must arise from a discrepancy between $b_{\overline{Y}\overline{X}}$ and b_{yx}, and, as Alwin (1976) shows again, to regress R on $\overline{X}_j$ is to produce a regression coefficient equal to $b_{\overline{Y}\overline{X}} - b_{yx}$. The model thus demonstrates that part of the effect of $\overline{X}_j$ on $\overline{Y}_j$ stems from the same individual-level process that connects x_{ij} and y_{ij} in the lower half of the model. Should there remain a residual effect of $\overline{X}_j$ on $\overline{Y}_j$, purged of this individual-level component, it implies the existence of a different causal process at the group level from that operating at the individual level.[4]

Although $b_{\overline{Y}\overline{X}} - b_{yx}$ and b_{yx} are the same estimates of group and individual effects, respectively, which would be obtained via Equation A, the ANCOVA model allows other insights into the processes at work which the conventional regression method obscures. One such insight is that the contextual effect, $b_{\overline{Y}\overline{X}} - b_{yx}$, describes a component of the relation between $\overline{Y}_j$ and $\overline{X}_j$; the aggregate or group-level relation. To refer to a contextual effect on individuals, which is the theme, as we pointed out, of most literature on this topic, is to consider an *indirect effect*, one which operates on individuals only through their membership in a common group (Hauser, 1971: Chap. 2).[5] Secondly, the model reveals the point at which a global variable such as size (S) would intervene. Such a variable could have no effect on the within-group component of the process relating X_{ij} to Y_{ij} as our Figure 1c showed. Its inclusion or exclusion from the model is therefore irrelevant to the estimate of b_{yx}. Moreover, since $\overline{Y}_j$ and C are wholly determined, any direct effect of size would be confined to R.[6]

An Empirical Application: Decentralization in Social Service Organizations

In the sections to follow, we apply the analyses described earlier to data gathered by a survey of social service agencies in a large midwestern metropolis.

---

[4] Cronbach (1976:3.18) has criticized the substantive interpretation of $b_{\overline{Y}\overline{X}} - b_{yx}$ as a contextual effect. In his view, only if the "selection factor[s]"—the variable(s) on which individuals are sorted into groups—are fully captured by the measured independent variables in the model would such an interpretation be warranted. If not, the model is misspecified, and $b_{\overline{Y}\overline{X}} - b_{yx}$ is a biased estimate of the contextual effect. While we take Cronbach's concerns seriously, we are not persuaded that the specification problem is any more serious here than with respect to models pitched at a single level of analysis. We have no a priori reason to assume that there exist unmeasured selection variables determining the distribution of persons among these organizations which are linearly related to the variables in our model.

[5] Because $b_{Y\overline{Y}} = b_{\overline{Y}R} \doteq 1.0$, the contextual effect is estimated with the same value $(b_{\overline{Y}\overline{X}} - b_{yx})$ regardless of whether Y, $\overline{Y}$, or R is taken as dependent variable. However, since $s_{\overline{Y}}/s_Y$ and $s_R/s_{\overline{Y}}$ normally will be less than 1.0, the standardized effect of $\overline{X}_j$ on Y_{ij} will be smaller than its similarly measured influences on $\overline{Y}$ and R (Hannan et al., 1976:139). It is, therefore, not surprising that school effects studies typically fail to find large path coefficients measuring the influences of contextual variables on student behavior and attitudes. To the degree that that Y variance lies within schools, rather than between them, standardized estimates of such effects will be reduced. If the research goal is to determine the influence of group (e.g., school) context *relative* to that of individual-level determinants, it may be reasonable to thus take the individual as unit of analysis (Alexander and Griffin, 1976). But if the goal (as in our case) is to ascertain whether and how some properties of collectivities affect others, then an individual-level analysis is inappropriate.

[6] In an otherwise trenchant critique of Bidwell and Kasarda's (1975) attempt to use aggregate student performance data to evaluate the organizational effectiveness of school districts, Hannan et al. (1976) state erroneously that the unstandardized coefficient (b_{IS}), which takes, say, student ability (I) as dependent upon some global property of the district (S), such as size, is subject to aggregation bias: a difference in the district-level and student-level slopes. This is incorrect. To regress an individual-level trait on a global trait yields the identical coefficient as the regression of the group mean of that variable on the same global property of the group.

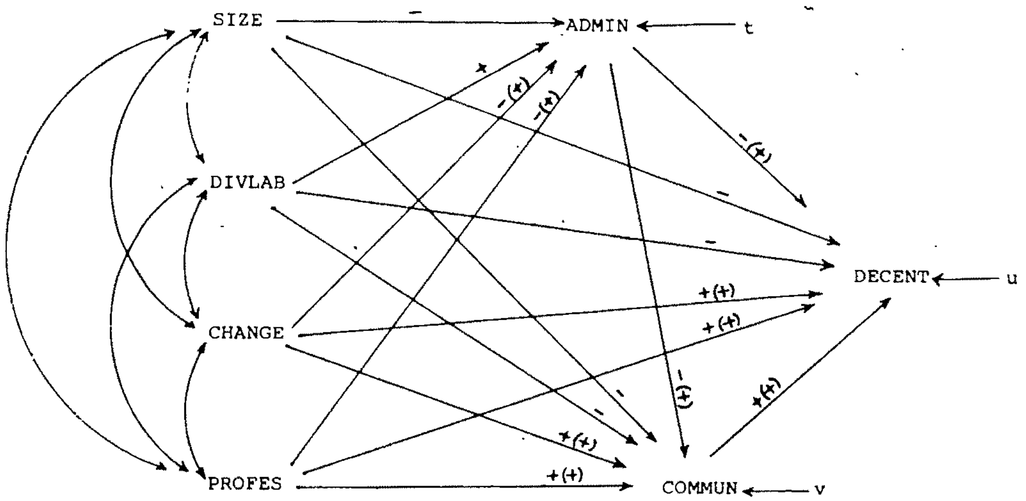


Figure 3. Causal Diagram of Processes Leading to Decision-Making Decentralization (Note: Signs indicate predicted direction of effect at the organization level and individual level (in parentheses).

A sample of 548 members of 20 public and private agencies responded to questionnaires or standardized interview schedules. The organizations ranged in size from 11 to 248. They were chosen randomly from a field of 85 human service agencies with staffs of ten or more, as listed in the Directory of Services published by the local welfare planning agency. The response rate varied from a low of 51% in one public welfare agency to 100%. The overall response rate was 71%.[7]

We will propose a simple model designed to explain decision-making decentralization in organizations. Figure 3 diagrams that model. The specific measures of variables we examine are discussed in an appendix. All such variables are means on distributions of scores derived from individual members with the exceptions of size (1n. total employees) and division of labor (no. occupational titles). Since any relation, in which both dependent and independent variables are means, may be partitioned into its organizational- and individual-level components, Figure 3 includes predicted signs of both micro- and macroeffects of all variables but size and division of labor. Our reasoning behind these predictions follows.

The size, division of labor, level of professionalization, and change in the task environment are specified as exogenous variables. Administrative intensity is taken as the first endogenous variable to be determined by these factors. We hypothesize, following Blau (1970), that administrative intensity is inversely dependent upon size (because of administrative economies of scale) and directly dependent upon division of labor (because of increased coordination problems). Following Burns and Stalker (1961), Hage (1965), and others, we predict that administrative intensity should fall with rising professionalization and task environment change. The argument here is that professionalism and instability or uncertainty are both forces rendering close supervision and high administrative overhead ill-suited to effective performance. We acknowledge, however, that some investigators have found empirical patterns which deviate from this prediction. Blau (1968), for example, found a higher proportion of administrators in organizations where the average level of employee expertise was high, although his initial hypothesis was the same as our own. We should note that herein lies a case where one would predict very different organization-level and individual-level processes at work. The theory we advanced, positing an inverse relation, is pitched at the organization level. At the individual level, we think it highly probable that professionals (in terms of expertise or orientation to professional reference groups) might also be administrators (in the sense of wielding supervisory power). This process could produce a positive correlation between average professional status and average administrative status of organizations, even though the strictly organizational-level process was absent or worked in the opposite direction.

---

[7] Random samples stratified by rank were drawn of professional and administrative personnel within agencies. Nonprofessional (e.g., clerical and custodial employees) were excluded. The sampling fraction increased with rank and decreased with organizational size in order to ensure adequate representation of higher ranking employees and smaller agencies.

Another complication is evident in regard to the changing task environment/ administrative intensity relation. Most theory asserts that an unstable task environment is a cause, not an effect, of the shape of an organization's administrative structure (Woodward, 1965). But at the individual level, the preferred specification would appear to be that incumbency in an administrative position determines the stability of one's task environment, rather than the other way around. For the present, we are forced to ignore this complexity. Since our theoretical model is specified at the organization level, the causal ordering we first stipulated seems the appropriate one to retain.

The next stage of the model takes the level of horizontal communications as dependent upon all foregoing variables in this causal sequence. While we do not doubt that communication flows are strongly shaped by the size and configuration of the administrative component in an organization, there has been some debate over the direction of this effect (Blau, 1968; Brewer, 1971; Caplow, 1964:76). We predict it is negative and, thus, stay within that tradition which sees administratively intense organizations channeling and restricting communications (Lincoln and Miller, 1979). On similar theoretical grounds, we expect the level of communications to fall with size and division of labor but to rise with professionalization and change in the task environment. Individual administrators may well communicate frequently across departments and sections, so that the individual-level effect runs contrary to the organizational effect. On the other hand, the individual-level effects of environmental change and professionalization—we would expect—would have the same signs as their organizational counterparts: individual professionals and personnel confronted with unstable work conditions are more likely to communicate with their fellows than is true of employees not faced with these circumstances (Blau, 1955; Burns and Stalker, 1961).

Finally, decision-making decentralization is the last endogenous variable determined under this model. Numerous theoretical and empirical precedents suggest the hypothesis that organizations with high rates of communications develop decentralized decision-making structures (Bacharach and Aiken, 1976; Burns and Stalker, 1961; Hage, et al., 1971). Likewise, we predict that individuals who are centrally placed in communication networks acquire influence in decision-making processes (Blau, 1955; Guetzkow, 1965). As for the relation between administrative intensity and decentralization, however, the hypothesis is less clear. Blau (1968) has argued that the proportion of managers is a prima facie index of decentralization: more managers means more persons in decision-making roles—hence, decentralization. Similarly, studies by Pugh et al. (1968), Blau and Schoenherr (1971), Child (1972), and others identify decentralization with the spread of formal decision-making roles among the membership. On the other hand, this reasoning seems incompatible with both popular and scholarly themes which view administrative intensity and centralized decision making as twin manifestations of formal bureaucracy (Downs, 1967; Weber, 1947).

Our view is that administrative intensity gives rise to decentralization only if one assumes an individual-level causal process. The more administrators in the organization, the larger the proportion of members (who are administrators) making decisions. But at the organization level, we might find an opposing effect: the greater the proportion of administrators, the less the influence on decision making of the membership as a whole. For administrators, of course, the effect of his or her individual position offsets such a loss of power, whereas the effect of the subordinate's status is to enhance it.

This reasoning seems consistent with certain strands of organizational theory. Formal bureaucracy decentralizes in the sense that it divides decision-making responsibilities into specialized roles and delegates these to official positions (Child, 1973; Weber, 1947). But this same process of formal differentiation and delegation means that rank-and-file members who might have had some input to the decision, before it became an official function, now have less. Similarly, the administrator's own power is circumscribed as the number of officials holding responsibilities for specialized decisions grows (Crozier, 1964). The case of traditional Japanese management is instructive here to illustrate that delegation of formal authority and decentralization of decision making need not be the same. Yoshino (1968) describes the *ringi* system as a decision-making practice in which little formal authority is delegated but participation is widespread. Such participation takes the form not of individuals carrying out their separate assigned decision-making roles, but of a cooperative, consensus-seeking group process involving many members in each decision to arise.

Following this same line of thought, we also deviate from the findings of Blau and Schoenherr (1971) and Pugh et al. (1968) and predict that division of labor—while having a positive effect on administrative intensity—has a negative effect on decentralization: the greater the differentiation of functional roles in the organization, the less involvement by the general membership in decisions. Similarly, increasing organizational scale diminishes the breadth of collective involvement in organizational decisions. By contrast, we follow Burns and Stalker (1961) in viewing a changing task environment and professionalization as forces making centralized decision making ill-adapted to organizational goals. Organizations subject to changing environments and possessing professional staffs are more likely to make decisions through fluid, unstructured consultations. Any individual-level effects of these two properties we believe to be in the same direction. Certainly professional status and expertise are resources which organizational members convert easily to power in organizations. And Crozier's (1964) famous discussion of the maintenance workers in the French tobacco firm offers classic testimony for the proposition that individuals responsible for unstable and changing domains acquire informal organizational power.

Analysis and Findings

Table 1 presents zero-order correlations and univariate statistics for the measures we examine. As we will not have further opportunity to consider the associations among exogenous variables in our model, we should make certain observations now. Overall, this pattern of relations is consistent with the "mechanistic-organic" or "bureaucratic-professional" dimensions often seen to underlie structural variability in organizations (Blau and Scott, 1962; Burns and Stalker, 1961; Hage, 1965). Size and division of labor, as virtually every previous study has shown (e.g., Blau and Schoenherr, 1971), are highly correlated, and both reveal mild inverse correlations with professionaliza-

Table 1. Zero-Order Correlations and Summary Statistics

	2	3	4	5	6	7	$\bar{X}$	SD	E^2
1 SIZE	.668	.363	−.105	−.278	−.225	−.507	4.614	1.030	1.000
2 DIVLAB		.076	−.204	−.163	.070	−.456	11.863	4.927	1.000
3 CHANGE			.241	.141	.089	.239	2.959	.097	.044
4 PROFES				−.204	−.035	.550	5.963	1.384	.146
5 ADMIN					.622	.429	.411	.164	.111
6 COMMUN						.543	10.468	3.839	.181
7 DECENT							2.059	.302	.113

tion. Professionalization, in turn, is positively correlated with task environment change (Comstock and Scott, 1977). The exception to this pattern is the likewise-positive correlation between size and CHANGE. Received theory generally views large organizations as operating in stable task environments (Starbuck, 1965).

The other information worth noting in Table 1 is the column of correlation ratios (E^2). These indicate proportions of total variation in respondent reports which lie between organizations. That they have certain implications for our ability to generate stable estimates of $b_{\bar{Y}\bar{X}}$ and b_{yx} should be evident from the extreme cases where all variation is either between (as with size and division of labor) or within organizations.[8] They range from less than 5% of the total individual variation in perceived change to 18% of that in reported communications. We do not attach great importance to the fact that these values appear low. First, our interest is confined to the between-organizations variation. Patterns involving these variables within organizations are of interest only insofar as their form also describes a component of the between-organizations pattern. Secondly, we do not attribute intraorganizational variation to perceptual error. Should we do so, such low E^2's might be cause for concern with the quality of our measurement. Thus, R. B. Duncan (1972) defends the perceptual measures of environmental uncertainty he employs by noting the large fractions of organizational variance they display. Our aggregate measures have valid analogues for individuals, however, and we interpret the latter's variation substantively.

In the ANCOVA model (see Figure 2), the total relation between $\bar{X}_j$ (or a set of $\bar{X}_j$'s) and $\bar{Y}_j$ operates through two constituent processes. First there is C, the "compositional" or individual-level component: the organizational mean of Y predicted from the regression of $\bar{Y}_j$ on $\bar{X}_j$ under the assumption that the average within-group slope sufficed to capture the relation of Y to its causes at both individual and organization levels. The second component, R, is a residual (Hauser, 1971:chap. 2): the difference between the $\bar{Y}_j$ and its C component. Unlike C, which is an exact linear function of the $\bar{X}_j$'s, R combines the effects of the $\bar{X}_j$ (as organization-level or "structural" effects) with variation originating from unspecified sources (a disturbance term). It is instructive, then, to evaluate the relative importance of these components by partitioning the total variance in each endogenous variable into the fractions derived from these sources. Table 2 presents this decomposition.

While we have not carried a torch for organization-level explanations in this investigation, we nonetheless note with interest that in two out of three comparisons they seem more powerful than explanations favoring individual-level processes in accounting for our dependent variables. Table 2 shows the organizational- and individual-level components of the variance in decentralization to be roughly equal. The organizational contributions to administrative intensity and communications, however, exceed their individual-level counterparts by substantial margins. (We should bear in mind, however, that the presence of global measures as independent variables means they necessarily contribute nothing to the individual-level variance component.) The joint component is variation due to an inseparable composite of individual and organizational effects (see Duncan et al., 1972:195). The residual component—that explained by neither of these sources—ranges from 19% (decentralization) to 75% (administrative intensity).

If Table 2 underscores the importance

Table 2. Decomposition of Variance

	Component			
Dependent Variable	Organizational	Individual	Joint	Residual
ADMIN	.217	.092	−.054	.745
COMMUN	.393	.053	.043	.511
DECENT	.245	.278	.290	.187

Note: Table entries are proportions of total variance attributable to each source.

---

[8] The issue here is the familiar one that limiting the variance in a regressor increases its standard error. Where values of E^2 are low, between-organization effects will be estimated less reliably.

Table 3. Estimates of Total and Component Effect Parameters

		Predetermined Variables							
Dependent Variable		COMMUN	ADMIN	CHANGE	PROFES	SIZE	DIVLAB	R^2	$\bar{R}_2$
ADMIN	P			.402	−.338	−.514	.081		
Total	b			.6813*	−.0401*	−.0819*	.0027	.255	.057
Effect	SE(b)			(.4332)	(.0281)	(.0528)	(.0103)		
Individual	P			.018	.342	.000	.000		
Component	b			.0297	.0406*	.0000	.0000		
Organization	P			.385	−.680	−.514	.081		
Component	b			.6515*	−.0807*	−.0819*	.0027		
COMMUN	P		.608	.070	.121	−.315	.423		
Total	b		14.2197*	2.7839	.3361	−1.3099*	.3296*	.489	.307
Effect	SE(b)		(5.1727)	(9.3667)	(.5994)	(1.1392)	(.2078)		
Individual	P		.121	.039	.205	.000	.000		
Component	b		2.8242	1.5350	.5700	.0000	.0000		
Organization	P		.488	.031	−.084	−.315	.423		
Component	b		11.3956*	1.2488	−.2339	−1.3099*	.3296*		
DECENT	P	.418	.142	.167	.484	−.238	−.218		
Total	b	.0329*	.2606	.5213*	.1056*	−.0697*	−.0114*	.813	.727
Effect	SE(b)	(.0132)	(.3169)	(.4639)	(.0299)	(.0588)	(.0111)		
Individual	P	.185	.364	.091	.252	.000	.000		
Component	b	.0146*	.6697*	.2830	.0550*	.0000	.0000		
Organization	P	.234	−.222	.076	.232	−.238	−.218		
Component	b	.0184*	−.4090*	.2384	.0506*	−.0697*	−.0114*		

Note: P=standardized coefficient, b=unstandardized coefficient, SE(b) standard error of b. Standard errors of individual and organization component coefficients are equal to SE(b)'s total coefficients. Component effects are *indirect effects* on each dependent variable as mediated through C and R terms (see Figure 2).

* Coefficient exceeds its standard error.

of organization-level processes in our data, it leaves ambiguous their nature and how they differ from contributing individual-level patterns. Again consider Figure 2. It shows that the total effect of $\bar{X}_j$ on $\bar{Y}_j$ is the sum of two indirect effects, one representing the organization-level causal process, the other representing the individual-level process. Applying the path analysis convention for partitioning total effects, we see that $P_{\bar{Y}\bar{X}} = P_{C\bar{X}}P_{\bar{Y}C} + P_{R\bar{X}}P_{\bar{Y}R}$. Table 3 applies such a partitioning procedure to our data. First presented for each dependent variable are the total effects of antecedent organizational properties. Subsequent rows present individual-level and organization-level components of the total effect. Note that both unstandardized and standardized component coefficients sum (within rounding error) to their corresponding total effects. Also note that the standard errors of the component effects equal the standard errors of the total effects.[9] With only twenty organizations, we adopt a weak criterion of statistical significance: whether a coefficient exceeds its standard error. As Rao and Miller (1971:37) have observed, variables whose coefficients fall short of this criterion impair, rather than enhance, prediction, for they produce decrements to the adjusted R^2. Finally, Table 3 demonstrates our earlier point that any effects of global variables must operate through an organization-level process: the individual-level effects of size and division of labor are null, while the corresponding organizational effects equal the total effects.

---

[9] We determined the equality of the three component effects' standard errors through an inductive procedure. If $1-R^2_{\bar{X}}$ is the proportion of variation in $\bar{X}_j$ which is unexplained by other independent variables, then $(1-R_{\bar{X}}^2)(P_{R\bar{X}}P_{\bar{Y}R})^2$ is the unique contribution made by $\bar{X}_j$ to the variation in $\bar{Y}_j$ through an *organization-level* process. $(1-R^2_{\bar{X}})(P_{c\bar{X}}P_{\bar{Y}C})^2$ is the analogous contribution through an *individual-level* process. When either quantity is divided by $1-R^2_{\bar{Y}}/N-k$ (where $N-k$ is degrees of freedom), we have an F-ratio which, in conjunction with the metric coefficients for the organization- and individual-level effects, can be solved for these coefficients' standard errors: $SE(b) = b/\sqrt{F}$. As an example, consider the effect of COMMUN on DECENT. For the organization-level component, we have: $(.511)(.234)^2 = .0279$. For the individual-level component, we have: $(.511)(.185)^2 = .0175$. The denominator of the F-ratio is: $1-R^2_{\bar{Y}}/N-k = .187/13 = .0144$. Then $F_o = .0279/.0144 = 1.938$ and $F_I = .0175/.0144 = 1.215$. Taking square roots and dividing into the metric coefficients gives us: $.0184/\sqrt{1.938} = .0146/\sqrt{1.215} = .0132$ which proves to be the standard error of the "total" coefficient as well.

We begin our review of this evidence by considering total effects on administrative intensity. Readily apparent is its negative dependence on size, which the majority of investigators researching this relation have also found. In our data, however, division of labor fails to reveal the positive connection to administrative overhead which others have reported (e.g., Blau, 1970). Both task environment change and professionalization have significant influences on administrative intensity, but these diverge in direction. Professionalization, as hypothesized, reduces an organization's propensity to generate administrative positions. Contrary to images of the "organic" organization (Burns and Stalker, 1961; Hage, 1965), however, we find that a changing task environment raises the proportion of administrators. This result is reminiscent, perhaps, of Crozier's (1964) suggestion that bureaucracies respond to uncertainty by building in new administrative roles and other formal structures to eliminate it.

Now let us see what new insights may be introduced by decomposing the aggregate effects of change and professionalization into their constituent individual-level and organization-level components. We conclude that the positive influence of change on administration derives entirely from an organization-level process. The organization-level effect of this variable is nearly identical to its total effect, while the individual-level contribution is miniscule. This outcome warrants confidence in the causal ordering we specified between these variables and in the supposition that this perceptual measure can validly tap an organizational property.

Professionalization's influence on administration conforms closely to our predictions. The negative total effect proves to mask strikingly different individual and organizational patterns. The negative effect recurs at the organization level. But at the individual level we find a pronounced positive impact. These findings have considerable intuitive appeal. Individuals with high scores on the professionalism scale are, unsurprisingly, more likely to wield supervisory power. Holding constant this pattern, however, professionalization depresses the proportion of supervisory personnel. The latter tendency is strongly supportive of the venerable proposition that "bureaucracy" and "professionalism" are alternative, incompatible modes of organization (Blau and Scott, 1962).

Now consider the total and component effects on communications, which we take to be a function of these same exogenous determinants plus administrative intensity. The estimated total effects do not conform well to our hypothesis. We predicted the negative influence of size on communications: the ease with which information flows among organizational subunits declines as the number of members grows (Graicunas, 1937). But we observe a strong positive effect of division of labor—an outcome discordant with Burns and Stalker's (1961) view that lateral communications intensify when official roles are less differentiated and formally defined. Furthermore, neither a changing task environment nor professionalization has more than a slight (and nonsignificant) influence on communications, although both are regularly seen as forces causing flexible structures and horizontal exchanges (Burns and Stalker, 1961; Hage et al., 1971). The final surprise is that administrative intensity displays a strong positive relationship with communications flow. We hypothesized the opposite, assuming that a large administrative component meant that communications were more likely to be confined to vertical channels.

Our impressions do change somewhat when the component processes generating these patterns are taken into account, but the general picture at the organization level remains substantially the same. The negative effect of size and the positive effect of division of labor are necessarily replicated, but so are the null estimates of the influences of change and professionalization. On the other hand, the individual-level effect of administrative intensity—while positive—is trivial, and we observe a positive influence of professionalism which almost satisfies our statistical decision rule. A reasonable interpretation of these results might be: while individual professionals are more likely to engage in lateral communications than administrators, professionalization as an organizational process does not facilitate communications flow.[10] Administrative intensity, however, is a rather powerful determinant of such flows.

The final variable explained by the present model is decentralization. The pattern here is essentially the anticipated one. Both size and division of labor are inversely related to average decision-making participation, although these effects are small and marginally significant. The positive effect of task environment change is equally small. The coefficient estimated for administrative intensity is judged within sampling error of zero by our criterion. Yet both professionalization and communications are rather strongly linked to decentralization in the hypothesized direction: High communication rates and a professionalized work force engender decentralized decision making.

Once again, we learn considerably more about the processes underlying these patterns when we partition them into organizational and individual components. Professionalization, communications, and change have positive effects at both individual and organizational levels. It is, thus, not merely the case that professional organizations are decentralized because individual professionals within them are more likely to be involved in decisions. That process operates, but it is paralleled by one at the organization level: as the proportion of professionals rises, all employees experience an increase in influence. A similar interpretation is warranted concerning the micro- and macroeffects of communications and change; the total effects of both are divided almost evenly between compatible organizational- and individual-level processes.

Finally, the organizational- and individual-level influences which administrative intensity is shown to have on decentralization are perhaps the most noteworthy of these findings. We would indeed be surprised if persons in positions of formal supervisory authority did not participate in decisions more than colleagues not so officially advantaged. The individual-level results do reveal that administrative status has the single greatest impact on decision-making involvement. Yet the organizational effect is *negative*: organizations with large administrative components appear to centralize decision making in relatively fewer hands.

This is an intriguing finding which we think clarifies certain contradictory themes in the comparative research literature on organizations. To review our earlier argument, we might, in the face of these results, be disinclined to accept the "revisionist" argument which arose from the findings of Blau and Schoenherr (1971), the Aston Group (Pugh et al., 1968), and Child (1972) concerning bureaucracy and centralization (see Aldrich, 1979:11; Blau, 1970; Child, 1973). While early comparative analysts of organizations (Hage and Aiken, 1967; Hall, 1963; Pugh et al., 1968) had inferred from Weber (1947) that highly structured and formal organizations centralize decision-making at high levels, evidence soon accumulated that centralization was characteristic of simpler, less bureaucratic, organizations. Researchers encountering this finding, however, measured centralization essentially as the opposite of administrative intensity: highly centralized organizations were those in which a relatively small proportion of the membership performed formal decision-making roles. Assuming an individual-level process, administratively intense organizations are indeed decentralized: as more participants in organizations acquire supervisory authority, those persons add to the number making inputs to decisions, and influence spreads. But our data suggest that growth in the administrative component has an opposite "structural" effect: if A's becoming a supervisor means that B's power is diminished, a process of centralization is at work as well. By focusing on the distribution of formal authority, while neglecting that of actual participation in decisions, organizational research, we suspect, has obscured key issues in organizational theory.

---

[10] Again we should stress that, while we discuss our results here and elsewhere in terms of "organizational-level" and "individual-level" processes, we consider individual-level, within-organization effects only insofar as they describe a component of the between-organizations covariation (see Figure 2).

Conclusions

Aggregate indicators of organizational properties have been subject to much criticism and held to be poor substitutes for "global" measures. Yet we have shown that they permit insights into organizational phenomena which global indicators do not make available. It has long been recognized that an observed association between organizational traits can arise through a variety of processes at multiple levels. In general, however, organizational theory has emphasized organization-level effects and regarded them as something distinct from the aggregation of relations observable for individuals. This is particularly true of sociological approaches to organizational study in the late 1960s and 1970s. The Durkheimian dictum that sociologists should find their subject matter in properties of collectivities which are not reducible to those of individuals has nowhere been taken more seriously than by organizational sociologists who have insisted that organizations should be units of analysis—not merely contexts for studying individual or interpersonal behavior—and that organizational theory should address itself to developing propositions which hold at that macrolevel and not necessarily below it (see, for example, Perrow, 1972).[11]

Although not methodological Durkheimians in the sense of holding individual explanations to be irrelevant to our scientific mission, we do believe it is often at least informative and occasionally it is crucial (as when individual-level effects are viewed as measurement error) to determine whether observed associations among organizational properties have organizational versus individual origins. The methods we have discussed and applied seem an effective means of making that determination. Moreover, we do not believe that merely because we measured organizational variables as means of individual-level distributions we have sacrificed validity. Such measurement, as we pointed out, may not always be valid or appropriate, but its widespread use in organizational research suggests that it often seems so to investigators faced with concrete measurement tasks. Our recommendation is simply that researchers take advantage of these properties of aggregate measurement to extract meaning from their data which they have neglected in the past.

Also, we do not intend our own data analyses to be merely illustrative of our techniques. Our findings speak to several current and important issues in organizational theory and research. Without reviewing all that has gone before, certain key findings should be underscored. As Comstock and Scott (1977) discuss, the level of analysis at which an unstable task environment is believed to produce its effects on organizational structure varies among treatments of this topic in the literature. Hickson et al. (1969), for example, suggested that the weak influence of technology on most structural measures, which they found, might be due to an improper level of aggregation. They concluded that structural changes in response to technical variations were most pronounced for organizational components closest to the workflow. Similarly, Comstock and Scott (1977) found stronger effects of task variability at levels of aggregation lower than that of the organization as a whole. Our results diverge from these precedents, however; for the strongest effect of a changing task environment we find—that giving rise to increased administrative intensity—is wholly an organization-level one.

---

[11] We have dealt here only with organizational and individual levels of aggregation but, as some of our examples suggest, other levels (e.g., departments, administrative strata) are often of interest to organizational researchers as well. Clearly, however, an attempt to apply in the same analysis the methods we discuss to more than two levels of aggregation would run into technical complexities beyond the scope of our paper.

The portrait we painted of organizations with high communication flows is at odds with our own expectations and with the impressions left by past research. An extensive division of labor and a large administrative component—properties reminiscent of traditional bureaucracy—were strongly related through organizational processes to heightened communication flows. Professionalization and a changing task environment—factors which, on theoretical grounds, we expected would increase communications—did so only through an individual-level channel in the former case and in the latter case not at all.

Finally, our findings regarding decentralization support an earlier conception of bureaucracy as compared with that which has grown out of recent studies measuring decentralization as delegation of formal authority. That is, we find organizations with high average levels of decision-making participation to be small, less differentiated, professionalized, prone to lateral communications, and more likely to be found in changing task environments. Furthermore, net of the tendency for individual administrators to make decisions, participatory organizations prove to have relatively small administrative components. Although the limitations of our data base preclude strong inferences, we would nonetheless hope that these findings, combined with our discussion of aggregation issues, occasions some reexamination of the decentralization question in organizational research.

APPENDIX

MEASURES OF VARIABLES

1. SIZE: total employment size transformed to its natural logarithm.

2. DIVLAB: total number of occupational titles.

3. CHANGE: perceived change and uncertainty in tasks and work environment. Mean of the response codes to the following six items. Cronbach's Alpha = .492.

   a. How much change has there been in your job or position during the last year? (no change = 1, complete change = 5).

   b. How often are there changes in the social, economic, or political conditions outside your organization which affect your work? (never = 1, always = 5).

   c. People can often point to prevailing ideas in the community or in their profession about the best methods or techniques to be used in their job. How often are there changes in such ideas regarding your work? (never = 1, always = 5).

   d. How often are you certain of the requirements or methods to be used in your work? (never = 1, always = 5).

   e. How often do you encounter new types of problems in your work? (never = 1, always = 5).

   f. How often do you apply new techniques or methods in doing your job? (never = 1, always = 5).

4. PROFES: professionalization. The sum of responses to the following three items:

   a. How many professional or occupational associations do you belong to?

   b. During the past two years, how many meetings of professional or occupational associations have you attended?

   c. During the past two years, how many professional or occupational journals have you read on a somewhat regular basis?

5. ADMIN: administrative intensity. A dummy variable indicating whether or not the respondent was responsible for supervising the work of others.

6. COMMUN: interdepartmental communications. The respondent's estimate of the number of individual contacts or conferences he or she had with persons outside his or her department in an average week.

7. DECENT: decentralization of decision making. The mean of the response codes (never = 1, always = 5) to the following six items. Alpha = .85.

How frequently during the past year did you participate in the following decisions in your organization?

   a. on personnel matters (hiring, firing, and promotion).

   b. on approval of budgets.

   c. on approval of new policies and goals.

   d. on adoptions of new programs.

   e. on handling public relations outside your organization.

   f. on establishing joint or cooperative programs with other agencies.

Note: With the exception of size and division of labor, organizational scores on these variables were computed as unweighted means of respondents' scores.

REFERENCES

Aiken, Michael, and Jerald Hage
1966 "Organizational alienation: a comparative analysis." American Sociological Review 31:497–507.

Aldrich, Howard
1979 Organizations and Environments. Englewood Cliffs: Prentice-Hall.

Alexander, Karl L., and Bruce K. Eckland
1975 "Contextual effects in the high school attainment process." American Sociological Review 40:402–16.

Alexander, Karl L., and Larry J. Griffin
1976 "School district effects on academic achievement: a reconsideration." American Sociological Review 41:144–51.

Alwin, Duane F.
1976 "Assessing school effects: some identities." Sociology of Education 49:249–303.

Alwin, Duane F., and Luther B. Otto
1977 "High school context effects on aspirations." Sociology of Education 50:259–73.

Bacharach, Samuel B., and Michael Aiken
1976 "Structural and process constraints on influence in organizations: a level-specific analysis." Administrative Science Quarterly 21:623–42.

Bidwell, Charles E., and John D. Kasarda
1975 "School district organization and student achievement." American Sociological Review 40:55–70.

Blau, Peter M.
1955 The Dynamics of Bureaucracy. Chicago: University of Chicago Press.
1957 "Formal organization: dimensions of analysis." American Journal of Sociology 63:58–69.
1960 "Structural effects." American Sociological Review 25:178–93.
1968 "The hierarchy of authority in organizations." American Journal of Sociology 73:453–67.
1970 "A formal theory of differentiation in organizations." American Sociological Review 35:201–18.

Blau, Peter M., and Richard A. Schoenherr
1971 The Structure of Organizations. New York: Basic.

Blau, Peter M., and W. Richard Scott
1962 Formal Organizations: A Comparative Approach. San Francisco: Chandler.

Brewer, John
1971 "Flow of communications, expert qualifications, and organizational authority structures." American Sociological Review 36:475–84.

Burns, Tom, and G. M. Stalker
1961 The Management of Innovation. London: Tavistock.

Burstein, Leigh
1978 "Assessing differences between grouped and individual-level regression coefficients: alternative approaches." Sociological Methods and Research 7:5–28.

Caplow, Theodore
1964 Principles of Organization. New York: Harcourt.

Child, John
1972 "Organizational structure and strategies of control: a replication of the Aston studies." Administrative Science Quarterly 17:163–77.
1973 "Strategies of control and organizational behavior." Administrative Science Quarterly 18:1–17.

Coleman, James
1959 "Relational analysis: the study of social organizations with survey methods." Human Organization 17:28–36.

Comstock, Donald E., and W. Richard Scott
1977 "Technology and the structure of subunits: distinguishing individual and workgroup effects." Administrative Science Quarterly 22:177–202.

Cronbach, Lee J.
1976 "Research on classrooms and schools: formulation of questions, design, and analysis." Stanford, California: Occasional Papers of the Stanford Evaluation Consortium.

Crozier, Michel
1964 The Bureaucratic Phenomenon. Chicago: University of Chicago Press.

Downs, Anthony
1967 Inside Bureaucracy. Boston: Little, Brown.

Duncan, Otis D., David L. Featherman, and Beverly Duncan
1972 Socioeconomic Background and Achievement. New York: Seminar.

Duncan, Robert B.
1972 "Characteristics of organizational environments and perceived environmental uncertainty." Administrative Science Quarterly 17:313–27.

Firebaugh, Glenn
1978 "A rule for inferring individual-level relationships from aggregate data." American Sociological Review 43:557–72.

Glisson, Charles A.
1978 "Dependence of technological routinization on structural variables in human service organizations." Administrative Science Quarterly 23:383–95.

Graicunas, V. A.
1937 "Relationship in organization." Pp. 183–7 in L. Gulick and L. Urwick (eds.), Papers on the Science of Administration. New York: Institute of Public Administration.

Guetzkow, Harold
1965 "Communication in organizations." Pp. 543–72 in J. G. March (ed.), Handbook of Organizations. Chicago: Rand McNally.

Hage, Jerald
1965 "An axiomatic theory of organizations." Administrative Science Quarterly 10:289–320.

Hage, Jerald, and Michael Aiken
1967 "Relationship of centralization to other structural properties." Administrative Science Quarterly 12:72–92.

Hage, Jerald, Michael Aiken, and Cora Bagley Marrett
1971 "Organizational structure and communications." American Sociological Review 36:860–71

Hall, Richard H.
1963 "The concept of bureaucracy: an empirical assessment." American Sociological Review 69:32–40.
1968 "Professionalization and bureaucratization." American Sociological Review 33:92–103.

Hammond, John L.
1973 "Two sources of error in ecological correlations." American Sociological Review 38:764–77.

Hannan, Michael T.
1971 Aggregation and Disaggregation in Sociology. Lexington, Mass.: Heath.

Hannan, Michael T., and Leigh Burstein
1974 "Estimation from grouped observations." American Sociological Review 39:374–93.

Hannan, Michael T., John Freeman, and John W. Meyer
1976 "Specification of models for organizational effectiveness." American Sociological Review 41:136–43.

Hauser, Robert M.
1970 "Context and consex: a cautionary tale." American Journal of Sociology 75:645–64.
1971 Socioeconomic Background and Educational Performance. Washington, D. C.: Arnold and Caroline Rose Monograph Series of the American Sociological Association.

Hickson, David J., Derek S. Pugh, and Diana Pheysey
1969 "Operations technology and organization structure: an empirical reappraisal." Administrative Science Quarterly 14:378–97.

Hinings, C. R., and G. Lee
1971 "Dimensions of organization structure and their context: a replication." Sociology 5:267–72.

Lawrence, Paul R., and Jay W. Lorsch
1967 Organization and Environment. Boston: Harvard Business School.

Lazarsfeld, Paul F., and Herbert Menzel
1961 "On the relation between individual and collective properties." Pp. 422–40 in Amitai Etzioni (ed.), Complex Organizations: A Sociological Reader, Glencoe, Ill.: Free Press.

Lincoln, James R., and Jon Miller
1979 "Work and friendship ties in organizations: a comparative analysis of relational networks." Administrative Science Quarterly 24:181–99.

McGranahan, David A.
1976 "Correcting for informant bias." American Sociological Review 41:176–9.

Pennings, Johannes M.
1973 "Measures of organizational structure: a methodological note." American Journal of Sociology 79:686–704.
1975 "The relevance of the structural-contingency model for organizational effectiveness." Administrative Science Quarterly 20:393–410.

Perrow, Charles
1972 Complex Organizations: A Critical Essay. New York: Scott, Foresman.

Pugh, Derek, David Hickson, Robert Hinings, and Chris Turner
1968 "Dimensions of organizational structure." Administrative Science Quarterly 13:65–104.

Rao, Potluri, and Roger L. Miller
1971 Applied Econometrics. Belmont: Wadsworth.

Roberts, Karlene H., Charles L. Hulin, and Denise M. Rousseau
1978 Developing an Interdisciplinary Science of Organizations. San Francisco: Jossey-Bass.

Schuessler, Karl
1969 "Covariance analysis in sociological research." Pp. 219–44 in Edgar F. Borgatta (ed.), Sociological Methodology, 1969. San Francisco: Jossey-Bass.

Seidler, John
1974 "On using informants: a technique for collecting quantitative data and controlling measurement error in organizational analysis." American Sociological Review 39:816–31.

Starbuck, William H.
1965 "Organizational growth and development." Pp. 451–553 in J. G. March (ed.), Handbook of Organizations. Chicago: Rand McNally.

Talacchi, Sergio
1960 "Organizational size, individual attitudes, and behavior." Administrative Science Quarterly 5:398–420.

Tannenbaum, Arnold S., and Jerald G. Bachman
1964 "Structural versus individual effects." American Journal of Sociology 69:585–95.

Tausky, Curt
1967 "On organizational alienation." American Sociological Review 32:118.

Weber, Max
1947 The Theory of Social and Economic Organization. Translated by A. M. Henderson and T. Parsons. New York: Oxford.

Werts, Charles E., and Robert L. Linn
1971 "Considerations when making inferences within the analysis of covariance model." Educational and Psychological Measurement 31:407–16.

Woodward, Joan
1965 Industrial Organization: Theory and Practice. London: Oxford.

Yoshino, M. Y.
1968 Japan's Managerial System: Tradition and Innovation. Cambridge, Mass.: MIT Press.

MEAD AND BLUMER: THE CONVERGENT METHODOLOGICAL PERSPECTIVES OF SOCIAL BEHAVIORISM AND SYMBOLIC INTERACTIONISM*

HERBERT BLUMER
University of California, Berkeley

American Sociological Review 1980, Vol. 45 (June:409–419)

The article, "Mead vs. Blumer. . ." (*American Sociological Review*, June, 1979), carries serious misrepresentations. The authors, Clark McPhail and Cynthia Rexroat, have given an erroneous picture of (a) Blumer's view of social reality, (b) Blumer's view of naturalistic study, (c) Mead's view of scientific method, and (d) Mead's view of social behavior. Their misrepresentations arise from an effort to reduce Mead's thought to a much narrower scheme of how human social behavior should be studied.

Introduction

Scholarship in any important controversial area, such as that of the theoretical position of symbolic interactionism, should not have to carry the additional burden of misrepresentation. It is for this reason that I wish to correct the inaccurate depictions that are advanced in the article, "Mead vs. Blumer: The Divergent Methodological Perspectives of Social Behaviorism and Symbolic Interactionism" (McPhail and Rexroat, 1979:449–67). The authors, Clark McPhail and Cynthia Rexroat, proclaim that there is a basic difference, indeed a profound contradiction, between the views of George Herbert Mead on "social behaviorism" and my views on "symbolic interactionism." Their effort to establish this thesis is flawed by serious misrepresentations. I wish to call attention to these misrepresentations.

THE MCPHAIL-REXROAT ARGUMENT

The McPhail-Rexroat argument has three parts. (1) They contend that there is a basic difference between the methodological scheme of Mead's "social behaviorism" and my scheme of "symbolic interactionism." (2) They assert that this alleged difference in methodology springs, in turn, from a fundamental ontological difference between my views of "reality" and those of Mead. (3) Finally, they present several instances of research (McPhail and Rexroat, 1979:460–5) which, they declare, exemplify the proper nature of social investigation according to Mead's methodological views. I wish to consider each of these three parts of their argument. I will take up first, however, their second allegation, that of an ontological difference.

Alleged Ontological Difference

McPhail and Rexroat declare that: (a) whereas Mead adopts a pragmatic stance, my pragmatic stance "bears no resemblance to Mead's position" (McPhail and Rexroat, 1979:459); (b) whereas Mead sees scientific study as leading to a recasting of the world, I see such study as merely disclosing in an Aristotelian manner the fixed nature of the given reality; (c) whereas Mead recognizes that reality in its "objective" form requires a shared perspective, or what McPhail and Rexroat label "convergent responses," I fail to recognize this requirement; and (d) whereas Mead saw the scientific act as a social act shared by investigators, I see it as solely an individual act. I wish to consider each of these four contentions.

(a) McPhail and Rexroat declare that, contrary to Mead, whose philosophical position is that of pragmatism, I "waffle" or "vacillate" between the positions of realism and idealism. They see this alleged waffling in that I recognize a "reality" which is "out there," a reality which has a "nature" that is to be "discovered"; this, according to McPhail-Rexroat, puts me in the camp of realism. However, they assert that my view to the effect that "the world depends on how it is perceived" beds me in the camp of idealism. In the eyes of the authors this alleged vacillation not only places me in opposition to Mead's pragmatic stance but also introduces a "paradox" or contradiction into my ontological position.

---

* Direct all correspondence to: Herbert Blumer; Department of Sociology; University of California; Berkeley, CA 93720.

The inaccuracies in this characterization are shown by noting the following basic tenets of Mead's pragmatism:

1. There is a world of reality "out there" that stands over against human beings and that is capable of resisting actions toward it;
2. This world of reality becomes known to human beings *only* in the form in which it is perceived by human beings;
3. Thus, this reality changes as human beings develop new perceptions of it; and
4. The resistance of the world to perceptions of it is the test of the validity of the perceptions.

These tenets signify that, *in opposition to the idealist position,* there is a real world "out there" which may not be perceived at all by human beings or which may be perceived incorrectly. The tenets signify, *in opposition to the realist position,* that the real world does not have a basic intrinsic, fixed makeup but may change as human beings reconstruct their perceptions of it. The linking of "perception" with something "out there," that thereby comes to be perceived, is not "waffling"; nor is it "paradoxical." Instead of being contrary to Mead's position, it is Mead's position.

(b) In the case of their second ontological charge, McPhail and Rexroat use several of my literary expressions to attribute to me an utterly inapplicable Aristotelian view of reality. In my writings on the scientific study of ongoing group life, I have spoken of the need to "lift the veils" from what is hidden, or to "discover the nature" of what is thereby revealed, or to "dig out [data] by careful empirical scrutiny." McPhail and Rexroat gratuitously conclude that in using these expressions I am thereby designating the existence of an implanted, fixed, and eternal "reality," in opposition to the changeable reality in Mead's pragmatic stance. This conclusion is without foundation. Let me quote the relevant portion of my original discussion:

> . . . the study of [ongoing group life] requires us to expand and deepen our perception of it . . . . The metaphor that I like is that of lifting the veils that obscure or hide what is going on . . . . The veils are lifted by getting close to the area and by digging deep into it through careful study. . . . How does one get close to the empirical social world and dig deeply into it? This is not a simple matter of just approaching a given area and looking at it. It is a tough job requiring a high order of careful and honest probing, creative yet disciplined imagination, resourcefulness and flexibility in study, pondering over what one is finding and a constant readiness to *test and recast one's views and images of the area.* . . . It is particularly important in exploratory research for the scholar to be constantly alert to *the need of testing and revising his images, beliefs, and* conceptions of the area of life he is studying. (Blumer, 1969:39–41. My new emphasis.)

The quoted statements show that, contrary to the picture that McPhail and Rexroat are striving to present, the expressions I was using do not imply a fixed, unchangeable reality but, instead, one that is obviously always open to being recast as investigators approach its study with new perspectives.

(c) McPhail and Rexroat's (see p. 458, first [top] paragraph) third contention in support of their ontological allegation is that, contrary to Mead's view of reality, my view does not allow or provide for the sharing of perspectives—something that is necessary for the "objective reality of perspectives." This contention reflects serious misunderstanding; it amounts to a charge that, contrary to Mead's view of reality as socially shared, I hold and propose a view of reality that is solipsistic. Anyone who reads intelligently and carefully my discussion of objects and of how they are formed through the process of shared social interaction could not possibly attribute to me a solipsistic view of reality (See Blumer, 1969:10–12).

(d) The last "ontological" argument given by McPhail and Rexroat is not ontological but is actually a *methodological* argument. The argument reads:

> Blumer fails to recognize Mead's requirements that exceptional facts and their circumstances be so described by the initial investigator that others will experience the facts and that the investigator's working hypothesis be set forth and corroborated by others. (McPhail and Rexroat, 1979:458)

This methodological charge is false, as I will show, shortly, in the discussion of alleged methodological differences between Mead and me.

I trust that the foregoing discussion will make students and critics of symbolic interactionism aware of the serious misrepresentations that are contained in the McPhail-Rexroat allegations of ontological differences between Mead and me. No one of their contentions is true.

Alleged Methodological Difference

In seeking to specify their charge of an opposition between Mead's *methodological* perspective and mine, McPhail and Rexroat assert that Mead and I differ with regard to: (1) how we view the antecedents and the initiation of scientific inquiry, (2) how we treat the development and examination of hypotheses, and (3) how we treat the consequences of scientific inquiry. I wish to discuss each of these three lines of argument on which they base their case.

(1) Alleged differences in the viewing of antecedents and the initiation of scientific inquiry. The first part of this portion of McPhail and Rexroat's argument is that Mead and I differ in our views of the "antecedents of scientific inquiry." Yet, as the reader of their article will note, they actually assert no difference between Mead and me on this point except that my characterization and analysis is "erratic." Aside from the legitimate question of whether being "erratic" establishes a basic difference, it is odd to see how McPhail and Rexroat try to show that I am "erratic." To establish this charge they (p. 451) say, "On the one hand he [Blumer] is less sanguine than Mead with the adequacy of existing behavioral scientific theories, laws and beliefs" and, on the other hand Blumer "warns investigators to beware 'imprisonment' by their antecedent theoretical schemes." I am unable to understand how some sort of a combination of my being less "sanguine" than Mead and of my "warning" to investigators establishes me as being "erratic." While having no relation to this puzzling argument, I would like to add that no one knows whether I am more or less "sanguine" than Mead on the matter in question; also, Mead, as well as multitudes of other philosophers of science, would agree with me in recognizing that scientific investigators may become "imprisoned" by their antecedent views. Obviously, McPhail and Rexroat have done nothing to establish a difference between the way in which Mead and I view the "antecedents of scientific inquiry."

The allegation of a difference between Mead and me with regard to the "initiation of scientific inquiry" is stated by McPhail and Rexroat (1979:451–2) to be: "But Mead, unlike Blumer, requires and discusses procedures for the controlled perception of observed facts and for establishing problems as social objects" (p. 452).[1] Stated more precisely, this seems to mean that the method of study which I propose does not allow or require the investigator to identify his problem, his data, and his ideas in such a way that other investigators can check what he says. This charge by McPhail and Rexroat is completely wrong. Let us examine this matter in some detail, since it is pressed vigorously by McPhail and Rexroat. They are referring to my treatment of "exploration" and "inspection" as the two parts of the process which I proposed for the "naturalistic" study of human conduct and human group life. By "naturalistic" study I mean the study of conduct and group life as these occur naturally in the everyday existence of people—in the interaction of people as they associate in their daily lives, as they engage in the variety of activities needed to meet the situations that confront them in their day-to-day existence. This natural makeup of human conduct and group life covers what is done by individuals, organizations, institutions, communities, and collectivities as they carry on their lives. It comprises what Mead spoke of so frequently in his lectures as the "stuff" of human society—the complex of ongoing social acts that, according to Mead, constitutes the social life of human societies. This natural social world is the empirical domain of social science, the domain out of which problems arise, and the domain which is the test of the validity of their analysis.

---

[1] Mead did not deal in his writings or his lectures with the methodological problems involved in applying his scheme to the study of human conduct and human group life. He is frightfully shy in telling us how to study the social or joint act, which he set up as the basic unit of human group life. He did not tell us how to proceed to study the "generalized other" as it operates in the case of given individuals or groups in given situations. He did not tell us how to study the self-interaction which, let us say, a prospective bank embezzler is carrying on with himself prior to committing his embezzlement. He did not tell us how, as social scientists, we should proceed to take the roles of those we are studying, or what we should do to be sure that we are taking their roles. He did not tell us how to study the ways in which the human actor constructs his act. With brilliant insight, Mead identified the basic character of human social interaction, of self-interaction, of the joint or shared makeup of human group life, and of the emerging nature of individual and social acts. But he did *not* inform us how to proceed to study these basic matters. This picture is in utter contradiction to what McPhail and Rexroat (1979:452) declare: "But Mead, unlike Blumer, requires and *discusses* procedures for the controlled perception of observed facts and for establishing scientific problems as social objects" (my emphasis). *Where* does Mead discuss these procedures for the study of human social behavior?

Now I have called attention to the fact that (following Mead) this empirical domain of human group life is caught up in a continuous process of formation as people have to adjust to one another and to the situations with which they are faced. This means two things of great significance in the case of social science: (1) Much of what develops in human group life, especially in complex societies, is obscure or hidden from immediate notice; and (2) social scientists are *not* likely to have an initial firsthand, intimate familiarity with the empirical areas of group life that they propose to study. Because of these two conditions, I advocated and stressed the need of exploratory study (Blumer, 1969:40–2). Such exploratory study permits a research scholar "to form a close and comprehensive acquaintance with a sphere of life that is unfamiliar and hence unknown to him" and it is "the means of developing and sharpening his inquiry so that his problems, his direction of inquiry, data, analytical relations and interpretations arise out of and remain grounded in the empirical life under study." It is clear that "exploration," as I see it, is to be used in place of studies in which the research scholar has no close familiarity with, or knowledge of, his empirical area of concern. The purpose of exploration is to make sure that his perception of his field and the problem that he sets up are empirically grounded instead of being fashioned out of inadequate or faulty knowledge.

"Inspection" is the label I have used for the subsequent part of scientific inquiry, namely the exacting study of the problem that arises out of exploration. Since the stage of inspection is particularly the target of McPhail and Rexroat's (1979) attempt at criticism, it is desirable to see how I described it. Thus I introduce here a quote from my article:

> By "inspection" I mean an intensive focused examination of the empirical content of whatever analytical elements are used for purposes of analysis, and this same kind of examination of the empirical nature of the relations between such elements. Let me explain this abstract statement. By analytical elements I have in mind whatever general or categorical items are employed as the key items in the analysis, such as integration, social mobility, assimilation, charismatic leadership, bureaucratic relation, authority system, suppression of dissent, morale, relative deprivation, attitudes, and institutional commitment. As the examples suggest, such analytical elements may refer to processes, organization, relations, networks of relations, states of being, elements of personal organization, and happenings. These analytical elements may be cast in different degrees of generality, ranging from something very broad such as integration to something more restricted such as mobility aspiration in the case of urban Negro adolescents. The procedure of inspection is to subject such analytical elements to meticulous examination by careful, flexible scrutiny of the empirical instances covered by the analytical element. The empirical instances are those that appear in the area under study; their careful, flexible scrutiny is done in the

> context of the area in which they take place. Thus in the case of an analytical element such as assimilation, referring let us say to the assimilation of girls into organized prostitution, the empirical instances would consist, of course, of the separate careers of girls undergoing such assimilation. The careful scrutiny of such instances with an eye to disengaging the generic nature of such assimilation represents what I have in mind by "inspection." (Blumer, 1969:43–4)

With this brief reminder of what is meant by "exploration" and "inspection" as making up the *naturalistic* study of human group life, let me return to McPhail and Rexroat's (1979) argument. They are asserting that exploration and inspection, as I have defined them, do not allow the research scholar to describe his problem, his leading ideas, his mode of inquiry, and his data in such a way that other investigators can check on what he presents. Such a contention has no foundation. There is nothing in exploration and inspection to prevent the investigator from describing the question he has raised, the problem he has set, the ideas that guide his inquiry, his sources of data, the nature of the data, and the interpretations that he makes—all in such a way that other investigators can review what he has done and can examine the empirical world to see whether the account stands up. Indeed, this has been done in multitudes of naturalistic studies ranging from monographic studies of fauna and flora, in biology, through hundreds of anthropological monographs of so-called primitive societies, and scores of well-known sociological studies, such as Thomas and Znaniecki's (1927) *Polish Peasant*, Park and Miller's (1921) *Old World Traits Transplanted*, Thrasher's (1927) *The Gang*, and Wirth's (1928) *The Ghetto*. Such studies were naturalistic studies, using primarily the methods of exploration and inspection. They were presented in such form as to definitely allow other research scholars to reexamine what is claimed by the original investigator, to see whether the account is empirically correct. For McPhail and Rexroat to assert, in effect, that "exploration" and "inspection" can yield only solipsistic accounts that are incapable of test or corroboration shows a misunderstanding of the nature of naturalistic study and ignores a vast body of literature.

(2) Alleged differences in the treatment of hypotheses. The second methodological claim advanced by McPhail and Rexroat (1979:452–5) is that Mead and I differ in how we treat "hypotheses" in scientific inquiry. They seek to build their case on the following arguments:

1. "Blumer avoids the term *hypotheses.*" (p. 453)
2. For Blumer, the "meaningful connection between components *in the empirical world*" is "not assigned or hypothesized. It is discovered." (p. 453)
3. "Whereas Mead's treatment of hypotheses is theoretically grounded, though not formally deductive, Blumer's treatment is virtually atheoretical, inductive empiricism." (p. 453)
4. "Blumer rejects 'the four customary means' of empirically validating relationships between analytical components." "In short Blumer rejects all that Mead requires." (p. 454)
5. Mead "did not construe observation as a matter of recording 'the natural ongoing character' of things. . . . Rather, observation is guided by the problems and interests of the investigators." (p. 454)
6. "The investigator must specify the conditions to be met and the actions to be taken such that others, through their conduct, can experience, must observe, can confirm or reject the reported observations of the investigator." (p. 454)

I will briefly discuss each of these arguments, using the same numbers for identification.

1. I did not avoid the term "hypothesis." I merely used other words, such as "ideas," "leading ideas," "premises," "postulates," and so forth, just as Mead did in a very liberal fashion. (See, for example, [Mead, 1936:268–74] his discussion in *Movements of Thought in the Nineteenth Century.*)

2. As my earlier ontological discussion in the present article should have made clear, the second argument here by McPhail and Rexroat has no ground. To juxtapose "discovery" and "hypotheses" is unwarranted. It is thoroughly possible to make a "discovery" by following out a hypothesis, and it is thoroughly possible to treat a "discovery" as hypothetical.

3. The third argument is similarly false. I have made it clear that, in the case of both "exploration" and "inspection," the investigator necessarily has to guide himself by sets of ideas in seeking to carve out his problem, to get his data, and to analyze relations.

4. The fourth argument of McPhail and Rexroat is the result of a faulty reading by them of my discussion (Blumer, 1969:27–33) of four conventional methods of study: adhering to a scientific protocol; engaging in replication of research studies; relying on the testing of hypotheses; and employing so-called operational procedures. As a careful reading of what I wrote will show, I criticized these four methods *when they are used as substitutes* for the meticulous examination of the empirical area under study. McPhail and Rexroat have ignored this restriction and have extended my criticism to apply to all studies! This regrettable misinterpretation can be illustrated specifically in the case of the "testing of a hypothesis." I was at pains to point out in my discussion (pp. 29–30) that I was criticizing a widely held procedure—the procedure of (a) starting with a preestablished model whose premises have not been empirically validated and of (b) seeking to validate *the model* by testing a hypothesis deduced from the model. In my discussion I was not denying the need of testing hypotheses; I was denying the value of testing hypotheses as the means of establishing the validity of premises when the premises, themselves, had not been empirically tested. McPhail and Rexroat have failed to catch the point of the discussion.

5. The fifth McPhail-Rexroat argument is to the effect that the research scholar engaging in exploration and inspection has no problems or interests to guide his observations. As I have already explained, this is a false understanding of what takes place in naturalistic study.

6. The sixth argument by McPhail and Rexroat is another repetition of their claim that exploration and inspection do not permit the research scholar to describe his study so that it is open to test and corroboration by others. As I have pointed out above, this claim flies in the face of a mass of naturalistic studies in the fields of biology, anthropology, and sociology which have relied on exploration and inspection.

I have to conclude that there is nothing in the McPhail-Rexroat arguments to show their claimed difference in the ways in which Mead and I view the use of hypotheses in scientific inquiry.

(3) Alleged differences in the consequences of scientific inquiry. McPhail and Rexroat (1979:455) present their third alleged methodological difference between Mead and me in the statement that "Blumer's treatment of the theoretical consequences of inquiry is as equivocal as his discussion of the theoretical antecedents." I have already shown the meaningless character of the claim that I was "erratic" in the case of the "antecedents of scientific inquiry" (see discussion [*1*], earlier). Similarly, their claim here that I am "equivocal" with regard to the consequences of scientific inquiry has no meaningful foundation. The equivocation which they claim is that, on the one hand, for me, "every object of empirical consideration has a distinctive character" and that, on the other hand, "Blumer calls for the scientist to move beyond the empirical findings from a problematic investigation to some transcending theory or outside conceptual scheme." Put in simple form, these two sentences mean nothing more than that, on the one hand, I recognize that every empirical object has an individual uniqueness but that, on the other hand, it also possesses features that are common to a class of such individual objects. Thus, a scholar can describe and analyze an empirical object in terms of its distinctive makeup and career, and he can also describe and analyze it in terms of a theory that applies to a class of objects of which it is a member. I am unable to understand how McPhail and Rexroat can see "equivocation" in the fact that an empirical object may be perceived as distinct from every other empirical object and also may be perceived as sharing something in common with other empirical objects.

Some Further Observations on Alleged Methodological Differences.

I discern underneath the McPhail-Rexroat (1979:449–67) discussion what they really have in mind in alleging ontological and methodological differences between Mead and me. Their fundamental intention is to justify and promote a special mode of scientific inquiry that relies on controlled experiments or on observations closely akin to those made in controlled experiments. But they also regard themselves as followers of George Herbert Mead. They are, thus, forced to interpret Mead in such a way as to support their methodological orientation. They seek to do this in two ways. First, they try to interpret Mead's thought on "scientific method" in such a way as to uphold their methodological preference. Second, they endeavor to depict Mead's "social behaviorism" in such a manner as to fit their experimental or near-experimental commitment. I wish to comment on these two lines of their effort.

It is important to note that Mead did *not* see "scientific method" as a *special* kind of knowledge-producing procedure, or as a type of inquiry associated only with experimental science. To the contrary, he viewed scientific method as merely the extension of reflective intelligence. Referring to scientific method (1938:90), he said: "There is no difficulty in showing that, whether we call it the inductive or the experimental method, it is implicated in all intelligent conduct and that it has been involved in knowing the world at all times." Also,

> [t]he scientific character of the method is evidently found in the careful exactness with which the problem is defined, the data gathered, the hypothesis formulated, and the experiment carried out; but *these are merely the elaboration of the simple processes of everyday inference* by which we meet our constantly recurring difficulties. (Mead, 1938:83. My emphasis.)

With this modest view of the nature of scientific method, Mead could readily see scientific method in historical studies (p. 92) or speak of the "scientific treatment of religious institutions, beliefs, and experiences," or say that "more than one metaphysical problem [has been brought] into the range of scientific investigation" (p. 92), or declare that "The field of biblical dates is, of course, *one in which we find one of the most striking applications of scientific method* during the nineteenth century" (Mead, 1936:274–5. My emphasis).

Obviously for Mead, the mark of scientific method was the exercise of exacting care in (a) the setting of the problem, (b) the gathering of data, (c) the formulation of hypotheses or leading ideas, and (d) the testing of the ideas. Since these four steps can be executed in the case of naturalistic study, there is no difference at all between Mead and me. As I have said, there is nothing intrinsic in exploration and inspection to preclude the research scholar from specifying his problem exactly, from identifying his data carefully, from refining his leading ideas into clear form (formulating hypotheses), and from testing his ideas against the obdurate response of the empirical world (carrying out the experiment). McPhail and Rexroat have done nothing to show that it is impossible to carry out these four steps *with care* in the case of naturalistic study. Nor have they shown at all that it is impossible for the conscientious scholar who carries out these four steps to record the steps so that others may check on his experience. Indeed, naturalistic study in the form of exploration and inspection serves *the precise purpose* of enabling the research scholar to make a clearer identification of a problem, a more definite specification of what are the relevant data, a clearer formulation of leading ideas, and a more accurate identification of the obdurate empirical items that are to test the leading ideas. In direct opposition to what McPhail and Rexroat say, exploration and inspection lead the scholar to the empirical world in which the problem lies, instead of away from it.

To make sure that the understanding of these matters is fully clear, let me make some additional comments on the inaccurate renditions of Mead which McPhail and Rexroat (1979:449–67) make or imply repeatedly in their discussion.

> (1) The "exception" which Mead recognizes as the source of a scientific problem is in no sense confined to a challenge of an existing scientific law. It may be *any observation* which raises a new question, or sets a problem, or doesn't fit in with current thought or, as Mead says in one place, "the

observing of that which runs counter to accepted opinion" (Mead, 1936:284). In Mead's view, contrary to what McPhail and Rexroat imply, the "exception" which initiates scientific inquiry may arise from an exceedingly diverse background of antecedents. It may arise with the greatest of ease in the observation of the natural world.

(2) Mead stresses the need that the statement of the problem be made "in terms of the conditions of its possible solution." McPhail and Rexroat make a great deal of this statement, implying that it requires an exacting account of prospective research procedure, as in the design of a laboratory experiment. For Mead, the "conditions of its possible solution" had no such limitation; it referred to what was set by the problem and varied with the problem. Thus, Mead could recognize that careful historical studies that are used to establish biblical dates could be regarded as a striking application of scientific method. The term, "conditions of its possible solution," meant for Mead no more than an identification of the conditions that would allow for the careful execution of *whatever procedure* was needed to solve the problem; it did *not* mean adherence to a particular procedure.

(3) It is important to keep in mind that, although Mead used the term "experimental method" with great frequency, he recognized very clearly that it was not synonymous with "scientific method." He saw that "observation" outside of an experimental framework could be an integral and important part of scientific procedure. "There is just as much accurate technique in observation as there is in experimentation" (Mead, 1936:284). In no way did Mead exclude scientific observation from what I have spoken of as naturalistic study.

(4) It is important, also, to note that Mead saw that the part of the empirical world that is under scientific investigation has an *obdurate or resistant nature*. This obdurate nature is "there," in the "facts" which set the problem—and in the facts that have to be met by the proposed solution. They are the "hard data" of scientific study that have to be reckoned with. This obdurate or resistant nature of the empirical world that is under scientific study was not seen by Mead as eternal or immutable; it is recognized in true pragmatic spirit as subject to reconstruction. This is exactly my position.

To sum up, McPhail and Rexroat (1979:449–67) have grievously misrepresented Mead's ideas on scientific method in asserting that it cannot be employed in exploration and inspection. There are no fundamental differences between the ways in which Mead and I see methodology in scientific inquiry.

OBSERVATIONS ON MEAD'S "SOCIAL BEHAVIORISM"

As I have mentioned here, there is little doubt that McPhail and Rexroat are determined to depict Mead's thought in a way to make it fit their idea of how human group life is to be studied. Their idea has three compelling dimensions: It narrowly limits the scope of behavior that one is allowed to observe; it requires the investigator to be able to control or manipulate the behavior that is to be observed; and it requires the investigator to control and record his observations (McPhail and Rexroat, 1979:463). Mead's thought on the *nature of social behavior* is being squeezed by McPhail and Rexroat to fit this procrustean methodological bed—and, in the process, is being squeezed badly out of shape. We can see this deformation of Mead's thought by McPhail and Rexroat in what they propose in the latter section (pp. 460–6) of their article, "Investigating Mead's Ideas on His Own Terms." In that section, they present the two lines of study which, they say, represent the way in which Mead sees, and would study, "social behavior." These two lines of study are (1) the substitution of reflex movements in human behavior for the social meaning or character of that behavior, and (2) a reduction of social behavior to quantifiable units of overt responses. I wish to discuss each of these two lines of distortion of Mead's thought.

Substitution of Reflex Movements for Social Behavior

It is obvious from the discussion (see pp. 460–2 of their article) that McPhail and Rexroat regard the study of reflex movements to be the *major* way in which Mead would have us study human social behavior. They present three such studies of reflex movements (O'Toole and Dubin, 1968; Smith, 1971; and Cottrell, 1971) as exemplifying the investigation of Mead's social behavioral ideas "in terms of his own sociological perspective." I can say with unqualified confidence that Mead would have regarded as ludicrous the effort to center the scientific study of human social behavior in the reflex movements involved in such behavior. Viewed in terms of Mead's thought, such a shift to reflex movements suffers a serious disability: It replaces what is social in behavior by what is physiological. I wish to show this, using the cited O'Toole-Dubin study as a convenient illustration.

The O'Toole-Dubin study consisted of observing mothers who were spoon-feeding their infants and of counting the frequency with which each mother opened her mouth as she presented the spoon to the mouth of the infant. This study is hailed by McPhail and Rexroat as an exemplary study of Mead's ideas of what is involved in "taking the attitude of another." Yet, it doesn't even come close to catching what Mead saw as involved in taking the attitude of another. The spoon-feeding of the infant by a mother is what Mead spoke of as a "social act," one in which two organisms (the mother and infant) are adjusting their individual acts to one another in carrying out a joint act. In this instance, the mother would have the need of taking the "attitude of the infant," namely, to catch the perceived and the anticipated action of the infant so that the mother could adjust to the infant's action. The effort by the mother to catch the perceived and the anticipated action of the infant is a complicated matter—one that is dependent in large part on the kind of *social object* that the mother makes of the infant in the feeding act. Thus, the mother may see the infant in the feeding act in a variety of ways: as a good baby or an obstinate baby, as an infant who needs to be coaxed, as one who is making inordinant demands, as one who is delaying the mother's schedule to prepare dinner, as one who is to be nursed along with great care and attention, as one who is unwelcome but still has to be fed, and so on, in many different ways. These different ways in which the mothers may *see* their infants in the feeding act are definitely a "taking of the attitude" of the infant; they represent the ways in which the mothers see and treat the infants as acting toward them (the mothers). It is precisely these crucial objects—which mothers make of their infants in the feeding act—which are not revealed at all by the reflex movements. One would find out pitifully little about how a mother is taking the role of her infant by counting the frequency with which the mother opens her mouth as she extends a spoon to the infant's mouth. Such counting of her mouth openings will not tell us whether, as suggested, she sees the infant, at the time, as a burdensome demander, as a beloved object, as one who needs a great deal of guidance, as one who is possessed by a demon that needs to be exorcized, or as one who merely needs attention on schedule. The study of the mouth-opening reflex does not tell, at all, the kind of object that the mother is making of the infant. Yet, in Mead's thought, it is precisely this latter kind of knowledge that is essential to understanding the mother's *social* behavior toward the infant.

Mead saw the social behavior of human beings as constituted by their approaches and responses to one another as they engaged in their respective parts of their joint acts. These approaches and responses turn on how the human beings see and define each other in their interaction—the kind of objects they make of one another in the situations in which they are placed. Any scheme that proposes to study the social behavior of human beings, as seen by Mead, has to respect this social process. The study of the reflex movements of participants in the social process fails ignominiously to catch this social process. The study of reflex movements that are involved in social behavior may well fit McPhail and Rexroat's methodological procrustean bed, but such study grievously ignores what Mead saw *as central* to human social behavior.

Reduction of Social Behavior to Quantifiable Units of Overt Response

A similar verdict of distortion has to be made in the case of *the other form of study* which, McPhail and Rexroat declare, exemplifies Mead's ideas of social behavior. They (McPhail and Rexroat, 1979:462–5) speak of this second and remaining form of study as "exploratory-descriptive" research and illustrate its character by referring to a study by Melbin. McPhail and Rexroat propose their scheme of "exploratory-descriptive" research as a replacement for my scheme of "exploration" and "inspection"—a replacement which they announce as being faithful to Mead's idea of social behavior and, thus, as being a correction of my allegedly faulty rendition of Mead. Their scheme of exploratory-descriptive research is designed by them for naturalistic study—the study of the social life of human beings in their everyday natural settings. But McPhail and Rexroat find that their scheme of exploratory-descriptive research cannot be made to quite fit their *basic* methodological procrustean bed. So they make a concession by casting aside the requirement for the control and manipulation of the behavior that is under observation, and (p. 462) merely specify the other two requirements—limiting the scope of behavior to be observed, and having the investigator control and record his observations of his own behavior. However, within the framework of these two requirements, McPhail and Rexroat set up a *new* group of more exacting *requirements* that have to be met in order for social behavior in the natural world *to qualify* for study under their exploratory-descriptive scheme. From what they say—and particularly from the Melbin illustration—it seems clear that, to be studied in its naturalistic setting, social behavior must meet the following criteria:

> (1) The social behavior must be overt and thus open to direct observation;
> (2) the social behavior must be simple in makeup and brief in duration;
> (3) the social behavior must be capable of easy and unequivocal detection by investigators who are trained to make the same kind of observation; and
> (4) the social behavior must be capable of being counted in the form of clear-cut units.

This set of four requirements constitutes the *new procrustean bed* which human social behavior in natural life *has to fit* in order to qualify for study under the McPhail-Rexroat scheme of "exploratory-descriptive" research.

The question which has to be faced is whether human social behavior which is forced to fit this new procrustean bed represents social behavior as it was seen by Mead. My answer is resoundingly in the negative. To make this clear, let us consider each of the four requirements. The *first* requirement (that the behavior must be overt) rules out immediately the vast amount of covert behavior (the communication of the person with himself) which Mead elevated to such high importance in human group life. The *second* requirement (that the behavior be simple in makeup and have a brief span of duration) similarly rules out a vast portion of the complex and lengthy social acts that Mead recognized as taking place in natural group life. The *third* requirement (that the behavior be capable of easy and unequivocal detection by trained observers) puts to the side the sizeable amount of obscure and hidden behavior in natural life which can be identified only by a great deal of artful probing. Finally, the *fourth* requirement (that the behavior be capable of reduction to countable quantitative units) casts aside an enormous range and amount of social behavior that takes place in human group life. These simple observations show that human social behavior, as Mead recognized it to be in everyday life, has an abundance of important forms and developments which elude and, indeed, defy compression into the McPhail-Rexroat mold of their exploratory-descriptive research. Mead's ideas of social behavior definitely recognize the existence, in everyday life, of those social forms and processes which the McPhail-Rexroat scheme would exclude. Further, Mead never said, or hinted, that these forms and processes which McPhail and Rexroat would exclude cannot be studied scientifically.

I urge interested readers to examine carefully the two kinds of research which McPhail and Rexroat assert are the ways which Mead would have us use to study human behavior. These two kinds of research (the study of reflex movements and the highly compressed "exploratory-descriptive" research) fail overwhelmingly to catch what Mead had in mind in his conception of human social behavior. Mead did not have such a debased view of human social behavior as to equate it with reflex movements. Nor did he have such a constricted view of human social behavior as to confine it to small portions of simple, overt, quantifiable actions. Instead, Mead saw social behavior in its breadth, complexity, and natural character as it takes place in the multitude of *social acts* that make up the life of a human society. This was the empirical social world for Mead. Further, Mead's conception of scientific method fully permitted the application of scientific method to the naturalistic study of this empirical social world. In this naturalistic study, observations could be made, questions could be asked, problems could be narrowed and sharpened, data could be specified, leading ideas or hypotheses could be formulated, and special data could be selected to test the validity of the leading ideas. To contend or pretend that Mead's views did not allow for such direct scientific study of human social behavior in its natural empirical setting is to grievously distort Mead's views of both the nature of human social behavior and of the nature of scientific method.

To sum up, a careful examination of the McPhail-Rexroat (1979:449–67) article shows that they have seriously misrepresented (a) my ontological view of reality, (b) my view of naturalistic study, (c) Mead's view of scientific method, and (d) Mead's view of social behavior.

REFERENCES

Blumer, Herbert
1969 Symbolic Interactionism: Perspective and Method. Englewood Cliffs: Prentice-Hall.
Cottrell, Leonard S., Jr.
1971 "Covert behavior in interpersonal interaction." Proceedings of the American Philosophical Society 115:462–9.
McPhail, Clark, and Cynthia Rexroat
1979 "Mead vs. Blumer: the divergent methodological perspectives of social behaviorism and symbolic interactionism." American Sociological Review 44:449–67.
Mead, George Herbert
1934 Mind, Self, and Society: From the Standpoint of a Social Behaviorist. Charles W. Morris (ed.), with an Introduction. Chicago: University of Chicago Press.
1936 Movements of Thought in the Nineteenth Century. Merritt H. Moore (ed.). Chicago: University of Chicago Press.
1938 The Philosophy of the Act. Charles W. Morris (ed.). Chicago: University of Chicago Press.
O'Toole, Richard, and Robert Dubin
1968 "Baby feeding and body sway: an experiment in George Herbert Mead's 'Taking the role of other'." Journal of Personality and Social Psychology 10:59–65.
Park, Robert E.
1921 Old World Traits Transplanted. New York: Macmillan.
Smith, Richard L.
1971 Reflexive Behavior: An Experimental Examination of George Herbert Mead's Treatment of Vocal Gestures. Master's thesis. Department of Sociology, University of South Carolina, Columbia.
Thomas, W. I.
1927 The Polish Peasant in Europe and America. New York: Alfred A. Knopf.
Thrasher, Frederick
1927 The Gang: A Study of 1,313 Gangs in Chicago. Chicago: University of Chicago Press.
Wirth, Louis A.
1928 The Ghetto. Chicago: University of Chicago Press.

EX CATHEDRA BLUMER OR *EX LIBRIS* MEAD?*

CLARK MCPHAIL
University of Illinois

CYNTHIA REXROAT
University of Florida

American Sociological Review 1980, Vol. 45 (June:420–430)

Blumer (1980) hypothesizes we have misrepresented his views and those of Mead on scientific inquiry and social behavior, and that we have erroneously concluded their methodological perspectives diverge. Ironically, Blumer's paper exemplifies two of the fundamental problems we noted in his methodological perspective. First, he fails to specify the criterion in terms of which he makes the judgment of misrepresentation. Second, he fails to bring systematic evidence to bear on the claims of his hypothesis in relation to some criterion for judgment.

We proceeded differently. We indicated at the outset of our paper (McPhail and Rexroat, 1979:450, especially fn. 1, fn. 2, and fn. 3) that our criterion for judging convergence or divergence between Mead's and Blumer's methodological perspectives would be their written statements. When we attributed a position to Mead we provided citations to or quotations from Mead's statements documenting our claims. Blumer (1980) makes no less than 28 attributions of position, opinion or "thought" to Mead, yet offers documentation for less than one third of those attributions.[1] Instead, Blumer responds as he has in the past to those who offer non-Blumerian interpretations of Mead (e.g., Blumer, 1966; 1967; 1973; 1977: he simply asserts that *his* interpretation of Mead is *the* correct one! The availability of Mead's writings and edited lectures permit interested scholars to decide between a version of Mead *ex cathedra* Blumer or *ex libris* Mead.

In the following sections we examine Blumer's claims about his own ontological, methodological, and theoretical positions, as well as his claims about our misrepresentation. We examine his claims in terms of their internal consistency as well as the evidence from Mead's writings and from our (1979) paper. Our examination strengthens our initial judgment: Mead's and Blumer's methodological perspectives are divergent. Blumer's charges of misrepresentation are misdirected and reflect his own failure to examine what Mead has written and his failure to accurately report and respond to what we wrote.

ONTOLOGICAL DIFFERENCES

In his defense Blumer (1980:409–10 states his own ontological position and then, without citation or quotation, presents it as the ". . . basic tenets of Mead's pragmatism . . . ." We will not repeat our summary discussion of Mead's ontological position but invite readers to examine our synopsis (1979:457–9) or Mead's statements (e.g., [1900] 1964:8, [1903] 1964:54; [1917] 1964:196, 203; [1922] 1964:242; [1927] 1964:315–6, 318; 1932:275; 1934:78; 1936:275, 285; 1938:104–5, 275) and make their own comparison of his position with the one Blumer sets forth.

Blumer (1980) denies he is a "realist" or "Aristotelian essentialist" and proclaims allegiance to Mead's pragmatist position. The terms with which Blumer describes his naturalistic methodology make suspect his denial and his allegiance. On numerous occasions he refers to "the nature of naturalistic study" (p. 413), "the nature of scientific method" (pp. 415, 419), the "natural social world" (p. 412), "the *nature of social behavior* (pp. 416, 419) or "the natural character" of "social behavior" (p. 419). Webster's Third Edition defines "nature" as "the essential character or constitution of something, especially, the essence or

---

* Direct all communications to: Clark McPhail; Department of Sociology; University of Illinois; Urbana, IL 61801.

We thank Charles W. Tucker for commenting on a preliminary draft of this statement.

[1] None of Blumer's references to Mead's writings challenge any of the arguments presented in our paper, as our subsequent remarks will demonstrate.

ultimate form of something." If Blumer means something else by the terms "nature," "natural," or "naturalistic," he should provide us with a set of distinguishing criteria.[2] Otherwise we must conclude that Blumer either disagrees with or fails to understand Mead's (1936:275) contention that "science has gotten away from metaphysical dogmas as to what the nature of things is" and is concerned instead with "the ordering of events it observes."

The "ordering of events" is achieved through the responses—the behaviors—of the investigator and others. Blumer (1980: 410) protests he is not "designating the existence of an implanted, fixed and eternal reality," yet he makes repeated designations of the "nature" or "natural makeup" of human behavior. Such designations attribute the meaning or characteristics to the behavior rather than to the responses the investigator makes to the behavior. The meanings of a behavior are the responses made to it (Mead, [1900] 1964:8) and not the discovery or revelation of its "nature" or "natural makeup" at any one point in time or another.[3] The corroboration of the investigator's responses by another's response coestablishes the presence and meaning of that behavior. Such convergent responses (Mead [1927] 1964:315–6, 318) constitute the objective "stuff" with which science works. It is Blumer's failure to adopt this social behaviorist stance that makes his position so variable and confusing, if not contradictory. It is the same failure which separates Blumer from the pragmatist position with which Mead transcends the realist-idealist gap.

METHODOLOGICAL DIFFERENCES

Antecedents and Initiation of Inquiry. Blumer (1980:411) denies our (1979:451) charge that he presents an "erratic" characterization and analysis of the role of antecedent theory. To support his denial Blumer (1980:411) selectively quotes the first and final phrases, italicized below, from our discussion of his positions. He (1980:411) then writes he is unable to understand how those phrases establish his erratic position. They don't! The excluded phrases do. We wrote (1979:451):

> *On the one hand he is less sanguine than Mead with the adequacy of existing behavioral scientific theories, laws and beliefs.* Extant theories cannot provide benchmarks when the majority of theoretical concepts are too vague and ill-defined to be used in scientific inquiry (Blumer [1954] 1969:138). Blumer says these concepts, at best, may serve as "sensitizing concepts" in a preliminary exploration of the empirical world. This may result in the development of more "definitive concepts." Yet, on the other hand, Blumer ([1940] 1969: 181) warns of the ". . . risk and danger that the [theoretical] concept may coerce judgment and determine what is seen." He (1969:41) *warns investigators to beware "imprisonment" by their antecedent theoretical schemes."*

On the one hand, Blumer ([1940] 1969:181) says theory is so powerful it may coerce judgment, determine what is seen and, thereby, imprison the investigator. On the other hand, he ([1954] 1969:138) says extant theory is inadequate, too vague and ill-defined, to direct scientific inquiry. This is one basis for our judgment that he is erratic on this point. Further, because we understand the term "sanguine" to be defined as "optimistic" or "confident," we judge that Blumer is less sanguine than Mead ([1917] 1964:207; 1938:56) about extant theory as a source of hypothetical solutions to whatever area of activity or experience becomes problematic (Mead, 1936:264–5).

We also conclude Blumer is less confident than we are about applying Mead's methodological perspective to the study of human social behavior. Blumer (1980:416–9) contrasts the physiological movements of human beings with the "natural character" of their social behavior. His discussion of that contrast suggests a distinction Mead refused to draw—namely, that there is some essential characteristic or generic feature of human social behavior which renders it immune to investigation by "the scientific method." Mead ([1906] 1964:61) recognized no fundamental distinction between the researches of the social and biological and physical sciences "in point of method":

---

[2] It does not suffice to define these concepts in terms of one another as Blumer does when he writes (1980:412): "By naturalistic study I mean the study of conduct and group life as these occur naturally in the everyday existence of people . . .".

[3] "Resistance" is not a useful defining characteristic of "the nature" of the empirical world because this turns attention away from the responses of human beings and toward "the nature" of that to which they respond. Some readers may pound their desks when they read these comments and thereby meet with resistance. But resistance varies with response and the use of an electric drill or a laser beam, as compared with a fist, will produce varied resistance from the desk.

> Each is approaching problems which must be solved, and to be solved must be presented in the form of carefully gathered data. For their solution hypotheses must be constructed and tested by means of experiment or observation. With the complexity of the phenomena, of course, the application of the scientific methods will vary. The processes of observation, for example, will vary enormously in the study of a historical problem in the ancient world, and the study of the problem of variation where the material is immediately at hand.

The study of varied and complex human social behaviors in an ongoing situation might require a specialized data collection and analysis technique (for example, film recording) but variation in techniques does not qualify or change "the scientific character of the method" (Mead, 1938:83) which is found "in the careful exactness with which the problem is defined, the data gathered, the hypothesis formulated and the experiment carried out. . . ."

Blumer argues, as we explicitly acknowledge (McPhail and Rexroat, 1979:453, fn. 9–10), that Mead did not limit the application of scientific inquiry to experimental manipulation. Scientific inquiry is the explicit and formal extension of intelligent problem solution to all areas of human inquiry (Mead, [1906] 1964:61; [1917] 1964; [1923] 1964:248–66; 1936). Blumer correctly, but incompletely, notes Mead's reference to the application of scientific method to the historical analysis of disputed biblical dating.[4] He conveniently overlooks Mead's (1938:35) favorable reference to experimental studies and his (1936:400) comments on the merits of observation versus introspective studies of human behavior. It is simply not true, as Blumer (1980:411, fn. 1) claims, that Mead failed to discuss procedures for the study of human behavior. Mead did not write a handbook of research procedures; but, then, neither did Blumer. Both wrote about "methodological principles" and it is our contention that Mead's comments are both more explicit and extensive than Blumer's.

Exploration and Inspection. We (1979:462–3) recognize that exploratory research is the imperative first step in becoming familiar with the phenomena to be explained. We are less sanguine about what Blumer (1980:412) calls "inspection": the "meticulous examination" of the analytical element by "careful, flexible scrutiny of the empirical instances covered by the analytical element."

First, Blumer fails to tell us what one does when one engages in "meticulous examination" or "careful, flexible scrutiny." We think it might involve, at minimum, the description of the phenomena to be explained. Mead (1936:283–4) says the investigator should produce descriptions of as many instances of the problem as possible and, thereby, increase the facts known about the problem. He (1936:350) discusses the use of vocal and nonvocal gestures to discriminate or point out objects and (1936:378–80) to direct conduct with respect to the objects established by such gestures. He (1936:406) emphasizes the detailed information which must be noted when reporting the exceptional event. In this way investigators' activities establish, specify, describe and corroborate the phenomena to be explained. Second, Blumer (1980:413) advocates inspection "with an eye to disengaging the generic nature" of analytic elements. This exemplifies the Aristotelian conception of observation Mead criticized ([1917] 1964:184) because of the value it places on ". . . the insight with which [the investigator] recognizes the nature of the object," analyzes that nature "into essential characters" and therefrom formulates the definition of the object.

---

[4] Mead mentions "biblical dating" as an example of scientific challenge to dogma. Bishop Ulster (c. 1654) calculated the creation of the universe at 4004 B.C. His claims were subsequently challenged in the 19th century. Mead (1936:274) observed that "if you set up a dogma of that sort, science will inevitably undertake to find a problem in it . . . will at once turn about and ask for a justification . . . will look up documents, ask what they are, where they come from, who wrote them, and pull the dogma all to pieces." We are in complete agreement with Blumer on this point: textual analysis can be used to pull extant dogma all to pieces!

Here, again, we confront the fundamental divergence between Blumer's and Mead's treatments of observation. Blumer properly calls for direct observation of the empirical world but he places the emphasis upon discovering or recognizing the "generic nature of analytic elements" through inspection of empirical instances of the analytic category. Thereby Blumer directs attention toward the nature of the phenomenon and away from Mead's ([1900] 1964:8) emphasis on the response to the phenomenon: "*the meaning of the object is derived entirely from our reaction upon it*" (emphasis supplied). The investigator's activities impose order by specifying what must occur before the investigator (and others) can judge that phenomena have occurred, or that one phenomenon is related to, follows from, or occasions another. This is what investigators must do (Mead, 1938:83) if they are to proceed with "careful exactness" to define their problem, gather their data, and submit their hypotheses to the test of observed facts.

Hypotheses. Blumer (1980:412–3) offers six refutations of our (1979) claim that his treatment of hypotheses differs from Mead. We respond in turn. First, Mead (1936:346–91) does use synonyms for the term hypothesis in the lecture to which Blumer refers. However, it is difficult for us to read what Mead has written on scientific inquiry and escape the conclusion that "hypothesis" is his preferred term. "Hypothesis" is the crucial instrument in scientific inquiry, "the escape from the exception" or "the tentative solution to the problem" which must be submitted to empirical test against observed facts.

Second, we juxtapose Mead's term "hypothesis" with Blumer's term "discovery" because the former is an activity advanced against the empirical world by the investigator, whereas the latter is, according to Blumer, something drawn from the nature or character of that world. Again, the Meadian emphasis is on the actions of the investigator whereas the Blumerian emphasis is upon the nature of the world.

Third, Blumer denies our (1979:455) suggestion that he advocates or practices "atheoretical induction." But note his reaction to our summaries of studies of "taking the attitude of other"; that is, the individual's reflexive response to its own gesture. Blumer is concerned with the substance of baby feeding whereas O'Toole-Dubin (1968) merely took that substantive setting as an opportunity to observe and record the behaviors for a central theoretical concept in Mead's (e.g., 1936:360–85) explanation of coordinated behavior.

Fourth, we did not suggest that Blumer (1969:28–31) categorically rejects what he calls "the four customary means" of scientific inquiry. We (1979:454) explicitly summarized the reasons Blumer offers for rejecting most research done in those ways. The fact remains that Blumer consistently finds fault with conventional methods of study which require investigators to put up criteria for and empirical evidence against the claims of their hypotheses. Mead (1936:284) says that investigators must "take the hypothesis that you have formed and see if it will fit with the facts which you have before you." Blumer consistently resists, equivocates, or circumvents this challenge.

For example, Blumer claims we fail to catch the point of his discussion of testing hypotheses. He (1969:29–30) denies the ". . . value of testing hypotheses as the means of establishing the validity of premises when the premises themselves have not been empirically tested." We take it that Blumer is not using the term "premise" as synonymous with the term "assumption" (and, hence, something which is not subject to empirical check) but, rather, is using "premise" to refer to a basic proposition in a theoretical argument. By what process would such a proposition come to receive the empirical test to which Blumer refers? Mead (1936:285) says that one starts with a hypothesis which proposes a solution to one's problem, or an answer to one's question, and then submits that hypothesis to test. "You test it. When you have tested it, it becomes a working hypothesis. And if others test it and it works, it becomes a working theory." Of course, that theoretical premise (or proposition or universal) holds only so long as it is consistent with the last available set of observations. But to claim, as Blumer does, that one should not test hypotheses unless they are derived from empirically validated premises, is to deny the process by which empirically validated premises are constructed.

Fifth, we concede that Blumer's investigator might have problems or interests to guide initial exploration and inspection. However, if those problems or ideas are not explicitly stated "up front," we do not know the criteria in terms of which investigators judge that this or that phenomenon occurs. We are prevented from knowing whether "the guiding idea" was a useful one or was abandoned en route. If it was not useful, that should be reported, and, if it was useful, we need the criteria as instructions for our own exploration of the same phenomenon or for corroboration of the initial investigator's report. Finally, when investigators fail to give attention to and describe their own actions toward the phenomenon in question, they are more likely to conclude, mistakenly, that they have "discovered the nature of something" whereas, in fact, it has been established by their actions.

Sixth, we are not persuaded that Blumer's methodology permits the investigator to specify what was observed such that others can corroborate the initial report. It is clear that Blumer does not require such specification and corroboration. In response to our challenge, Blumer claims that, like Mead, "he too" considers these important matters. But, like Mead (1936:400), we think we are on much safer grounds "to come back to the conduct of the individual if you are going to [make judgments about his position] than to come back to something he reports to you by means of introspection." As we (1979:452, fn. 6) noted earlier, Blumer is neither unaware of the issues of specification and corroboration nor is he unaware of crude procedures with which they could be addressed. But his writings from 1928 through 1979 have given these matters such scant attention, or have discussed them in such obscure locations, that we can only conclude he has considered them unimportant until now.

Consequences of Scientific Inquiry. Blumer (1980) responds consistently to our charges of equivocation on the theoretical antecedents and consequences of scientific inquiry. He consistently omits the substantive illustrations of our contentions and then claims bewilderment when we find him erratic. On the present point, Blumer does concede that objects can be construed as distinctive or unique as well as being construed as members of a general theoretical category sharing common features. But his position still falls far short of Mead's (1936:285) explicit discussion of the process of formulating and empirically generating theoretical universals. Blumer's (1980:414) protest notwithstanding, this point is a clear example of an important difference between the ways Mead and Blumer view the use of hypotheses in scientific inquiry.

FURTHER METHODOLOGICAL DIFFERENCES

Blumer (1980:414–5) claims that our concern with promoting a special mode of scientific inquiry has led us to misconstrue Mead's methodological and theoretical perspective to serve our purpose. How does Blumer know what we "really have in mind" (cf. Mead, 1936:404)? Our initial concerns were with the social organization of individual behavior and experience, with the coordination of the ongoing behaviors of two or more persons into social acts, and with the import of vocal and nonvocal gestures for the organization of individual and social acts. Reading what Mead had to say about those matters led us to read what he had to say about the conduct of scientific inquiry. We were surprised—given his advance billing by Blumer (1969:1)—to find that Mead had so much to say! Thus, if it is possible for anyone accurately to reconstruct their intentions, we would judge that our substantive and theoretical interests led to an examination of Mead's methodological perspective. This is not quite what Blumer has discerned.

Blumer claims that we wish to restrict Mead's characterization and analysis of scientific inquiry to "controlled experiments." To the contrary, we wrote (1979:453): "Mead's analysis of scientific inquiry discusses the examination of hypotheses by means of experimental *and* nonexperimental observations *as well as* with thought experiments" (emphasis supplied). We further wrote (p. 462): "Not every problematic behavior, nor every phase of any problematic behavior, lends itself to experimental investigation." We also noted that Mead (1938:82–3) construed the experimental method as a set of requirements for generating knowledge and not as a data generation technique, and that he recognized that experimental tests of hypotheses were not always practical or possible. We suspect Mead favors experimental method because it involves the investigator's active intervention and manipulation of the world in the solution of problems, whether these involve specific but abstract categories of behavior in the laboratory (1938:35) or larger configurations of behavior in the ongoing life of the community ([1899; 1923; 1929] 1964).

Blumer (1980:415) persists in the claim that we "have done nothing to show that it is impossible to carry out [Mead's (1938:83) criteria for scientific inquiry] in the case of naturalistic study." As we have noted, Blumer *does not require* exacting care in the definition of the problem, the gathering of the data, the formulation or the test of the hypothesis. Blumer merely says his methodology does not prevent, or that it allows, meeting those criteria. We contend it is not sufficient (cf. Mead [1917] 1964:196–7) to leave these matters to the discretion of Blumer's (1980:415) "conscientious scholar."

If Mead's criteria for scientific inquiry can be employed in exploration and inspection, why aren't they? What happens if Mead's criteria are explicitly incorporated into Blumer's naturalistic methodology? If that incorporation alters Blumer's methodology then his perspective and that of Mead are obviously different.[5] If that incorporation does not alter Blumer's methodology, then he was ill-informed when he set out to develop his approach (1969:1) to deal ". . . explicitly with many crucial matters that were only implicit in the thought of Mead."

Blumer (1980:415–6) charges us with a series of "inaccurate renditions of Mead." These charges are without merit or foundation. First, contrary to Blumer's claims, we do not repeatedly assert or imply that the sole source of a scientific problem is an exception to an existing law. We wrote (1979:451) that the problem might involve "an exception to or breakdown of a theory, law, hypothesis, unit of analysis, equation, or some piece of equipment." We also noted (p. 451) "extraordinary events" as well as exceptions to "a law, theory, or belief." We recognize, now as then, that no area or phase of activity or experience is immune to such exception.

Second, Blumer (1980:416) takes exception to our (1979:451–2) discussion of stating "the problem in terms of the conditions of its possible solution." We do not, as he claims, require "an exacting account of prospective research procedure, as in the design of a laboratory experiment." To the contrary, the bulk of our discussion deals with the importance of producing descriptions of the problem to increase one's familiarity with those problematic features with which a hypothesis must deal (cf. Mead, 1936:283–4). Our discussion emphasizes description of the phenomena to be explained and thus parallels what Blumer calls the "exploration" stage of his own methodological approach.

Third, we stated in our paper (1979: 453–4)—and repeat here, as we noted above—our recognition that Mead did not equate scientific and experimental method. He certainly did not demean the importance of nonexperimental observation. Thus, Blumer's (1980:416) quotation of Mead, italicized below, is correct, but incomplete. Mead (1936:283) wrote:

> *There is just as much accurate technique in observation as there is in experimentation.*

---

[5] One inevitable alteration would be to eliminate Blumer's designation "naturalistic" which is antithetical to Mead's ontological and epistemological position (cf. Mead, 1936:275).

> What the experiment does is to take the hypothesis you have formed and see if it will fit with the facts which you have before you and other facts which you can gather.

Fourth, we are still uncertain where Blumer stands on the ontological issue. He writes about "this obdurate or resistant nature of the empirical world that is . . . not . . . immutable" (1980:416). We agree with Mead (1938:275). Reality is a presupposition of all scientific inquiry. The manipulatory phase of the act meets a resistance which answers to the preceding perceptual phase of that act (1938:104–5). But resistance varies as a function of response (cf. fn. 3, herein).

BLUMER'S VIEW OF "THE NATURE OF SOCIAL BEHAVIOR"

The final section of Blumer's paper (1980) contends our methodological perspective squeezes Mead's thought on "the nature of social behavior" badly out of shape. Blumer erroneously states our methodological position and then offers *no* documentation for any of the statements about social behavior he attributes to Mead.

On Methodology. Blumer (1980:416–8) confuses our discussion of controlled observation of behavior with our discussion of controlling the behavior which is observed. We did not suggest that all observation research—let alone all scientific inquiry—must involve the investigator's manipulation or control of the behavior to be observed. Experiments are distinguished by the controlled observation and recording of the consequences of the investigator's manipulation of the limited behavior(s) under study (cf. our discussion, 1979:454). Obviously, not all phenomena lend themselves to experimental investigation.

What distinguishes systematic exploratory-descriptive research? It presumes that some phenomenon is problematic for the investigator. The initial statement of that problem is not discovered "in the nature" of the phenomenon but is stated in terms of the implicit or explicit theoretical language of the investigator. This statement is progressively narrowed and refined through the investigator's increased familiarity with the phenomenon. This may take weeks, months, or even years. (Melbin [1972:93], reports he spent most of two years trying to clarify the issues involved in describing interpersonal behavior.) It requires repeated observation, detailed notes or records, and the examination of both against the initial statement of the problem. Those activities enable the investigator to provide a more explicit specification of the problematic features of the phenomenon. That, in turn, permits the systematic collection of additional data with which to describe the now-specified phenomena to be explained. This progressive narrowing of the scope of the investigation, this recognition of the significance of the investigator's own activities in specifying the problem (and, therefore, the importance of controlling and reporting what he or she has done so that others may know and do the same) are conspicuously absent from Blumer's formulation. In contrast, we judge Melbin's (1972) study of everyday patient-aide interaction in mental hospitals to be an exemplary piece of systematic exploratory-descriptive research because of the attention it gives these issues.

On Social Behavior. Blumer's (1980:416–9) remarks on "social behavior" misrepresent what we (1979) wrote, bear little resemblance to Mead's discussion of the points at issue, and propose a research agenda which is grandiose and naive. First, we do not regard—nor did we suggest—that "reflex movements" (i.e., involuntary acts) are the "major way in which Mead would have us study human social behavior" (Blumer, 1980:416). We (McPhail and Rexroat, 1979:460) use the term "reflexive," not "reflex," to designate those responses an individual makes toward its own behaviors.

Second, we categorically reject the distinction Blumer (1980:417) attempts to draw between "physiological" *(sic)* behavior and social behavior. Mead ([1924–25] 1964:279; 1936:368–85) uses the term social act to refer to the cooperative or coordinated activities of two or more persons. What is coordinated? Physical movements and vocalizations are coordinated! This includes movements of the entire body (e.g., coordinated marching or dancing), movements of portions of the body (e.g., coordinated tactile manipulations), or vocal and nonvocal gestures (e.g., coordinated conversation). It is important to repeat (cf. McPhail and Rexroat, 1979:458, fn. 15) that, in the usage of his times, Mead ([1924–25] 1964:286–7) termed vocal and nonvocal gestures "attitudes" and defined them as "stages in the act as they appear to others, [including] expressions of countenance, positions of the body, changes in breathing rhythms, outward evidence of circulatory changes, and vocal sounds."

What is the source of coordination? Mead (1936:360–85) discussed innate physiological differentiation as the source of complex coordinated behavior among invertebrates. He sought an alternate source for the indefinitely more complex coordination of human behaviors. He sought an explanatory principle which would locate in the activities of each participant in the social act elements of the other's contribution to that act. Mead's (1936:377) principle of organization was the vocal or nonvocal gesture by which one individual can simultaneously indicate to another, and to itself, "the elements which are of importance in co-operative activity" (1936:378). This is accomplished if the gesture calls out similar responses from the other and from the individual, thereby getting into each participant's activities elements of the other's contribution to the social act.

Mead ([1927] 1964:312) called such gestures "significant symbols." These are created, taught, learned, used, and revised by those who participate in a common language community. With such gestures the individual can point out things and give directions to others. Mead ([1922] 1964:244–5) wrote:

> In giving directions, we give the direction to ourselves at the same time we give it to another. We assume also his attitude of response to our request, as an individual to whom the direction has the same signification in his conduct that it has to ourselves.

and, elsewhere (1936:377):

> Through the use of language, through the use of the significant symbol, then, the individual does take the attitude of others. . . . If, now, the individual calls out this attitude in others by a gesture, by a word which affects himself just as it affects others, then he will call out the same attitude in himself as others would. . . . This gives the principal method of organization which, as I have said, we can study from the standpoint of a behavioristic psychology.

This discussion should make clear to others, if not to Blumer, that Mead uses the term "attitude" to refer to observable nonvocal or vocal gestures (even "physical movements"). Mead uses "taking the attitude of the other" (hereafter, TAO) to refer to the individual's response to its own gesture toward another as other responds to that gesture, and, Mead suggests, this can be studied behaviorally.

Third, Blumer is simply wrong, then, when he (1980:417) asserts the O'Toole-Dubin (1968) study "doesn't even come close to catching what Mead saw as involved in taking the attitude of another." O'Toole and Dubin were concerned with the reflexive behaviors of TAO. Their studies, and those of Smith (1971) and Cottrell (1971), attempted to determine if it was possible to observe and record, systematically, "reflexive responses"—the individual's response to its own gesture toward another. It is true that they did not compare the responses of other with the individual's gesture or with the individual's response to its gesture toward other. They therefore examined but a portion of TAO and of the social act. But their studies unquestionably demonstrate it is possible to observe reflexive responses. They thereby substitute systematic inquiry for dogmatic conjecture about TAO. The O'Toole-Dubin and Smith studies unquestionably demonstrate that the individual's reflexive responses to its own "directions" to other correspond to what we could reasonably expect other's responses to be. And there is abundant experimental (e.g., Rigney, 1972; Milgram, 1973) and nonexperimental (e.g., Melbin, 1972) evidence that other will comply with the directions it receives!

Fourth, Blumer's (1980:417) criticism of the TAO studies illustrates another problem with the objectives and consequences of naturalistic methodology. The

O'Toole-Dubin study is berated for addressing but a limited set of questions about the mother's responses to her own gestures toward the infant. Blumer proposes an additional list of questions an investigator would have to answer to provide the "kind of knowledge . . . essential to understanding the mother's *social* behavior toward the infant" (1980: 417). By implication, any research which does not simultaneously address all those questions fails to provide the "essential knowledge" Blumer requires and is, therefore, without merit. Blumer's grandiose objectives for any one piece of research illustrate the error Mead ([1917] 1964:200) referred to as confusing the "scientific attitude of being ready to question anything with an attitude of being willing to question everything at once." An investigator must have some unquestioned and stable ground on which to stand while examining the limited number of phenomena which can be systematically observed at any one time (cf. Miller, 1956).

Faced with problematic complex phenomena, the investigator can flounder in many directions at once or can divide the phenomena into a number of separate and smaller problems for investigation. The TAO observations and experiments address one set of questions regarding the complex social act. Melbin's systematic field observations address another. Couch and his students (e.g., Miller et al., 1975) have addressed related questions. Many important aspects of the social act remain to be examined, but we now know more about the empirical merits of Mead's view than we could have known by attempting to answer a multitude of questions at once.

If such an approach has merit for the comparatively "simpler" social act, it becomes mandatory for the broader and more complex social phenomena to which Blumer alludes. Several years of field research on crowd behaviors lead to the judgment that they are very complex phenomena, requiring many separate research foci (McPhail, 1972): for example, the assembling process (McPhail and Miller, 1973); shifts between elementary forms of individual and collective behavior within assembled gatherings (e.g., McPhail and Pickens, 1975; McPhail, 1978); and the dispersing process (e.g., McPhail, 1969). The examination of individual and collective behavior within gatherings can be further divided into the task of describing the phenomena to be explained (e.g., Pickens's [1975] description of spectator behaviors, or Wohlstein and McPhail's [1979] description of processions, marches, and other forms of collective locomotion), and the task of testing explanations of those phenomena (e.g., McPhail and Wohlstein, 1979).

Blumer's (1980:418–9) closing criticisms suggest that everyday complex social forms and processes will not yield to systematic exploratory-descriptive research. We disagree. The exploratory stages of the crowd research project focused upon overt behaviors because they could be observed and recorded.[6] Theoretical criteria were developed with which to judge the presence and extent of the selected behaviors under examination. This enabled the investigators to train others to make similar judgments. Some of those behaviors were "simple in makeup and brief in duration." Others were so rapid or involved so many participants (e.g., spectator and demonstrator behaviors) that videotape (Pickens, 1975) and film (Smith et al., 1975; Wohlstein and McPhail, 1979; McPhail and Wohlstein, 1979) records were made to permit repeated review, examination, and coding of the behaviors. Other phenomena developed over such extended time periods and spatial areas that, for example, only the termination of assembling, or the origins of dispersing, could be observed. This required the systematic use of interviews, questionnaires, and written narratives to obtain participants' and observers' reports of their own and others' behaviors. Those records were subject to similar criteria for coding judgments noted above. In both cases this permitted quantitative treatment of those judgments. In short, one's characterization of complex social forms and processes, and the methodological recipe one follows, may facilitate their systematic investigation, or may place them beyond the pale of scientific inquiry.

---

[6] It should be noted that the "covert communications" to which Blumer refers cannot be observed until they are reported by people. The reports are overt behaviors and they can be treated accordingly.

CODA

Blumer has been a significant voice in the sociological wilderness of surveys and secondary analyses, urging serious students to observe and become familiar with the behaviors they seek to explain. There is no substitute for exploratory observations but they are not sufficient. Beyond making contact with the empirical world lies the challenge of what the investigator does, once contact is made. The specification and description of the problematic behaviors are not located in the nature of the empirical world but are functions of the investigator's response to that world. The theoretical hypotheses offered as resolutions of the problematic behaviors must be more than guiding ideas. They must be stated in such a way that they can be subjected to test against observed facts and, thereby, accepted, rejected, or revised.

REFERENCES

Blumer, Herbert

[1940] 1969 "The problem of the concept in social psychology." American Journal of Sociology 45:707–19. Pp. 171–82 in Blumer (1969).

[1954] 1969 "What is wrong with social theory?" American Sociological Review 19:3–10. Pp. 140–52 in Blumer (1969).

1966 "Reply to Bales." American Journal of Sociology 72:411–2.

1967 "Reply to Woelfel, Stone and Farberman." American Journal of Sociology 72:211–2.

1969 Symbolic Interactionism: Perspective and Method. Englewood Cliffs: Prentice-Hall.

1973 "A note on symbolic interactionism" [Reply to Huber]. American Sociological Review 38:797–8.

1977 "Comment on Lewis's 'The classic American pragmatists as forerunners to symbolic interactionism.' " Sociological Quarterly 18:285–9.

1980 "Mead and Blumer: the convergent methodological perspectives of social behaviorism and symbolic interactionism" [Reply to McPhail and Rexroat]. American Sociological Review 44:409–19.

Cottrell, Leonard S., Jr.

1971 "Covert behavior in interpersonal interaction." Proceedings of the American Philosophical Society 115:462–9.

McPhail, Clark

1969 "Student walkout: a fortuitous examination of elementary collective behavior." Social Problems 16:441–55.

1972 "Theoretical and methodological strategies for the study of individual and collective behavior." Paper presented at the annual meeting of the American Sociological Association, New Orleans.

1978 "Toward a theory of collective behavior." Paper presented at the University of South Carolina Symposium on Symbolic Interaction, Columbia.

McPhail, Clark, and David Miller

1973 "The assembling process: a theoretical and empirical examination." American Sociological Review 38:721–35.

McPhail, Clark and Robert G. Pickens

1975 "Some explananda of collective behavior." Paper presented at the annual meeting of the American Sociological Association, San Francisco.

McPhail, Clark, and Ronald T. Wohlstein

1979 "The production of collective behavior within gatherings." Paper presented at the annual meeting of the American Sociological Association, Boston.

McPhail, Clark, and Cynthia Rexroat

1979 "Mead vs. Blumer: the divergent methodological perspectives of social behaviorism and symbolic interactionism." American Sociological Review 44:449–67.

Mead, George Herbert

[1899] 1964 "The working hypothesis in social reform." American Journal of Sociology 5:369–71. Pp. 3–5 in Mead (1964).

[1900] 1964 "Suggestions toward a theory of the philosophical disciplines." The Philosophical Review 9:1–17. Pp. 6–24 in Mead (1964).

[1903] 1964 "Definition of the psychical." The Decennial Publications of the University of Chicago. Chicago: University of Chicago Press. Pp. 25–59 in Mead (1964).

[1906] 1964 "The teaching of science in college." Science 24:390–7. Pp. 60–72 in Mead (1964).

[1917] 1964 "Scientific method and individual thinker." Pp. 176–227 in John Dewey (ed.), Creative Intelligence: Essays in the Pragmatic Attitude. New York: Holt. Pp. 171–211 in Mead (1964).

[1922] 1964 "A behavioristic account of the significant symbol." Journal of Philosophy 19:157–63. Pp. 240–7 in Mead (1964).

[1923] 1964 "Scientific method and the moral sciences." International Journal of Ethics 33:275–93. Pp. 267–93 in Mead (1964).

[1924-1925] 1964 "The genesis of the self and social control." International Journal of Ethics 33:229–47. Pp. 248–66 in Mead (1964)

[1927] 1964 "The objective reality of perspectives." Pp. 75–85 in E. S. Brightman (ed.), Proceedings of the Sixth International Congress of Philosophy. New York: Longmans, Green. Pp. 306–19 in Mead (1964).

[1929] 1964 "A pragmatic theory of truth." Studies in the Nature of Truth, University of

California Publications in Philosophy, 11:65–88. Pp. 320–44 in Mead (1964).
1932 Philosophy of the Present. Ed. by Arthur Murphy. LaSalle: Open Court.
1934 Mind, Self and Society. Ed. by Charles Morris. Chicago: University of Chicago Press.
1936 Movements of Thought in the Nineteenth Century. Ed. by Merritt H. Moore. Chicago: University of Chicago Press.
1938 The Philosophy of the Act. Ed. by Charles W. Morris. Chicago: University of Chicago Press.
1964 Selected Writings. Ed. by Andrew Reck. Indianapolis: Bobbs-Merrill.

Melbin, Murray
1972 Alone and with Others: A Grammar of Interpersonal Behavior. New York: Harper and Row.

Milgram, Stanley
1973 Obedience to Authority. New York: Harper and Row.

Miller, George
1956 "The magic number seven, plus or minus two: some limits on our capacity for processing information." Psychological Review 63:81–97.

Miller, Dan E., Robert A. Hintz, Jr., and Carl J. Couch
1975 "The elements and structure of openings." Sociological Quarterly 16:479–99.

O'Toole, Richard, and Robert Dubin
1968 "Baby feeding and body sway: an experiment in George Herbert Mead's 'Taking the role of the other.' " Journal of Personality and Social Psychology 10:59–65.

Pickens, Robert G.
1975 The Explananda of Collective Behavior, Ph.D. dissertation. Department of Sociology, University of Illinois at Urbana-Champaign.

Rigney, Ernest G.
1972 A Behavioral Examination of Compliance. Master's thesis, Department of Sociology, University of South Carolina, Columbia.

Smith, Richard L.
1971 Reflexive Behavior: An Experimental Examination of George Herbert Mead's Treatment of Vocal Gestures. Master's thesis, Department of Sociology, University of South Carolina, Columbia.

Smith, Richard L., Clark McPhail, and Robert G. Pickens
1975 "Reactivity to systematic observation with film: a field experiment." Sociometry 38:536–50.

Wohlstein, Ronald T. and Clark McPhail
1979 "Judging the presence and extent of collective behavior from film records." Social Psychology Quarterly 42:76–81.

MANUSCRIPTS FOR THE
ASA ROSE SOCIOLOGY SERIES

Manuscripts (100 to 300 typed pages) are solicited for publication in the *ASA Arnold and Caroline Rose Monograph Series*. The Series welcomes a variety of types of sociological work—qualitative or quantitative empirical studies, and theoretical or methodological treatises. An author should submit three copies of a manuscript for consideration to the Series Editor, Professor Suzanne Keller, Department of Sociology, Princeton University, Princeton, NJ 08540.

EDUCATION AND FERTILITY: IMPLICATIONS FOR THE ROLES WOMEN OCCUPY*

RONALD R. RINDFUSS
University of North Carolina
LARRY BUMPASS
University of Wisconsin
CRAIG ST. JOHN
University of North Carolina

American Sociological Review 1980, Vol. 45 (June:431–447)

The interplay between education and fertility has a significant influence on the roles women occupy, when in their life cycle they occupy these roles, and the length of time spent in these roles. The overall inverse relationship between education and fertility is well known; but little is known about the theoretical and empirical basis of this relationship. This paper explores the theoretical linkages between education and fertility and then examines the relationships between the two at three stages in the life cycle. It is found that the reciprocal relationship between education and age at first birth is dominated by the effect from education to age at first birth with only a trivial effect in the other direction. Once the process of childbearing has begun, education has essentially no direct effect on fertility; but it has a large indirect effect through age at first birth.

No factor has a greater impact on the roles women occupy than maternity. Whether a woman becomes a mother,[1] the age at which she does so, and the timing and number of her subsequent births set the conditions under which other roles are assumed. Some may deplore this situation and it may be changing, but the dominance of motherhood continues to be a fact for the vast majority of women. While there is clearly variance in this role dominance, the assumption of nonfamilial roles varies markedly with the fact, timing, and extent of maternity.

Education is another prime factor conditioning female roles. Education is expected to impart values, aspirations, and skills which encourage and facilitate nonfamilial roles. It is possible that better-educated women may assume less traditional role patterns than less-educated women with identical fertility histories. However, it is also likely that education affects women's roles through differing patterns of fertility. This paper discusses some of the possible linkages between education and fertility and reports analyses bearing on: (1) the relationship between education and age at first birth, (2) the effects of education on the timing of subsequent births, particularly on the experience of short birth intervals, and (3) educational differences in wanted family sizes.

EDUCATION–FERTILITY LINKAGES

Given the importance of the interplay between education and fertility for the roles women occupy in industrialized

---

Direct all communication to: Ronald R. Rindfuss; Department of Sociology; Hamilton Hall 050A; University of North Carolina; Chapel Hill, N.C. 27514.

This is a revised version of a paper presented at the annual meeting of the American Sociological Association, San Francisco, September, 1978.

This analysis was supported in part by grants from the Spencer Foundation to Ronald R. Rindfuss, from the University Research Council of the University of North Carolina to Ronald R. Rindfuss, from a subcontract to Ronald Rindfuss of NICHD grant No. 10751, from the Graduate School of the University of Wisconsin-Madison to Larry Bumpass, by a Center for Population Research grant No. 05798 to the Carolina Population Center from the Center for Population Research of the National Institute of Child Health and Human Development, and by a Center for Population Research grant No. 05876 to the Center for Demography and Ecology of the University of Wisconsin from the Center for Population Research of the National Institute of Child Health and Human Development. The authors gratefully acknowledge the able research assistance of Barb Witt and Cheryl Knobeloch, and the comments of two anonymous reviewers.

[1] Here, and throughout the paper, we use the term "mother" in its social rather than biological sense. The biological mother is the female who gives birth to the child. The social mother need not be the biological mother; but, typically, the two are the same. It is the social mother that has primary responsibility for the care and nurture of the child. This role need not be occupied by a female, but, typically, it is. Also, the word "children" throughout this paper is used in its social, rather than biological, sense.

societies, there has been surprisingly little attention paid to the causal linkages between the two.[2] In part, this may be because the possible causal connection between fertility and education is exceedingly complex. Some have assumed that education affects fertility (e.g., Westoff and Ryder, 1977; Rindfuss and Sweet, 1977; Cho et al., 1970; Whelpton et al., 1966), and some have argued that fertility also affects education (Waite and Moore, 1978).

Most of the theory and research concerned with education and fertility conceptualizes both in terms of their end products: completed education and children ever born. In fact, children come one at a time (usually), and education is completed a year at a time, sometimes a course at a time. Children can come close together, or at intervals of 10, 15 or even 20 years. Formal schooling can be completed without interruption; or it can be completed after short or long interruptions (Davis and Bumpass, 1976). Models of education and fertility should reflect the fact that education and fertility are processes which take time to complete and which can intercept each other in complex ways.

The overall relationship between education and fertility has its roots at some unspecified point in adolescence, or perhaps even earlier. At this point aspirations for educational attainment as a goal in itself and for adult roles that have implications for educational attainment first emerge. The desire for education as a measure of status and ability in academic work may encourage women to select occupational goals that require a high level of educational attainment. Conversely, particular occupational or role aspirations may set standards of education that must be achieved. The obverse is true for those with either low educational or occupational goals. Also, occupational and educational aspirations are affected by a number of prior factors, such as mother's education, father's education, family income, intellectual ability, prior educational experiences, race, and number of siblings (for example, see Hout and Morgan, 1975).

Occupational and educational aspirations are also reciprocally related to evolving fertility preferences. These fertility preferences include both number and timing preferences, that is, whether a first birth is wanted ever and, if so, when. The number and timing preferences may be related if, for example, a desire for many children leads to a desire to begin childbearing as soon as possible (Bumpass and Westoff, 1970). Moreover, the preference for postponing a first birth may lead to interests in other areas which may then lead to a decision not to have any children. There is evidence that repeated postponement of the first birth is a typical pattern among those who are voluntarily childless (Veevers, 1973). Such preferences for timing are necessarily vague, but nonetheless important. Some young women may wish to have a baby as soon as possible, perhaps to establish an adult identity separate from their parents, or to fulfill strong nurturing needs. Such aspirations among young women are likely to have a negative effect on evolving role and educational aspirations. Similarly, a young woman who is sure she does not want to have a child any time soon, if at all, may expand her role and educational aspirations accordingly. Influences in the opposite direction operate as the threat of early fertility to educational attainment are recognized and fertility desires are adjusted accordingly.

Both of these preference sets (occupational and educational aspirations as well as fertility preferences) influence actual age and education at first birth through a set of intervening variables[3] that include the standard intermediate variables affecting exposure to intercourse, conception risk and gestation and parturition (Davis and Blake, 1956; Bongaarts, 1978).

Adolescents with higher educational and occupational goals may choose social patterns that are less likely to lead to early marriage, that is, "not wanting to go steady or get serious with boys," because they want to go to college. They may be less willing to engage in intercourse because of the threat of possible pregnancy to their educational or career plans. Sexually active adolescents with high educational aspirations may be more likely to try to control the risk of pregnancy through careful contraceptive use.

---

[2] The work of Holsinger and Kasarda (1976) for developing countries is an exception.

[3] In actual practice, we know of no case where all the intermediate variables are adequately measured. Models are evaluated as if there were direct effects, with researchers unable to specify the precise nature of the social and economic effects on fertility as they operate through the intermediate variables.

Adolescent women who desire early motherhood (and presumably early marriage) are likely to follow social patterns that lead to early intensive emotional involvement; and, when sexually active, this group may have relatively low motivation to avoid pregnancy. Such patterns may lead indirectly to lower educational achievement because of an early age at first birth.

Early marriage may have a direct effect on reducing educational attainment,[4] for example, when a girl leaves school in order to be married. These social patterns also have an indirect effect on education through factors affecting pregnancy and early age at first birth.

It should be noted that in the reciprocal relationship between education and age at first birth, the effects of education on age at first birth can only be the result of the intermediate variables discussed above (also, see Davis and Blake, 1956; Bongaarts, 1978) whereas the effect of age at first birth on education may also include a direct effect.

Both age and education at first birth can affect subsequent role and educational aspirations, and subsequent preferences for the timing and number of children. These subsequent aspirations and preferences are also reciprocally related. After the birth of their first child some women may find that they wish to reduce their fertility goals, increase their occupational goals, and return to school. Others who had planned on continuing their education may decide to have more children, or to quickly become pregnant again, either because of great satisfaction in the mother role, or because of a sense that it is an all-consuming role that precludes other options, or because they are not sure of what else to do.

Education, age at first birth, the possibly revised occupational and educational aspirations, as well as timing and number preferences all affect various aspects of the intermediate variables in a process similar to that elaborated above with respect to the period before the first birth. The period prior to the first birth includes an unmarried and sexually inactive period as well as a married interval for most women. For most (but not all) women, the period following the first birth begins within marriage. Some women will not yet be married and others will have married and separated or divorced by the time of the first birth. At the second birth, a woman may be never married, currently married, widowed, divorced or separated (Rindfuss and Bumpass, 1977). Marital instability is an important social factor in the social patterns category in each segment to the extent that it affects other intermediate variables such as frequency of intercourse, periods of abstinence, and use of contraceptives.

Fecundity is largely exogenous to the processes we are examining, though it has a clear effect on the timing of the first birth and may mediate the effect of age at first birth on subsequent fertility.

While these potential intersections in the relationship between education and fertility warrant more intensive study, that is not our purpose in this paper. The point we are attempting to make in the preceding discussion is that the observed relationship between completed education and completed family size is the cumulative outcome of a complex process that involves attitudes and decisions about both education and fertility that may change as time passes or as the woman moves from one stage to the next, and that it is necessary to examine empirically the various stages in the process.

Data

The data used are from the 1970 National Fertility Study (NFS), a multipurpose study based on a national probability sample of 6,752 ever married women under 45 years of age residing in the continental United States (Westoff and Ryder, 1977). Complete birth and pregnancy histories were obtained, thus permitting analysis of age at first birth and of birth intervals. Unfortunately, a complete educational history was not obtained. Only education at interview and education at marriage were obtained. This means that we have to use education at marriage as a proxy for education at first birth. For most women this is a reasonable proxy, since the correlation between age at first birth and age at first marriage is 0.74. In order to check the reasonableness of using education at marriage, we reran all the analyses using education at interview, and results were unaffected. However, it should be recognized that for younger mothers the first birth is likely to precede the first marriage. Finally, it should be noted that there were no questions asked about educational or occupational aspirations during the adolescent and young adult years.

---

[4] Note, however, that Voss (1977) finds a negative effect of age at first marriage on educational attainment. Marini (1978) argues, and we agree, that this finding of Voss is the result of the lack of an adequate instrument for age at first marriage.

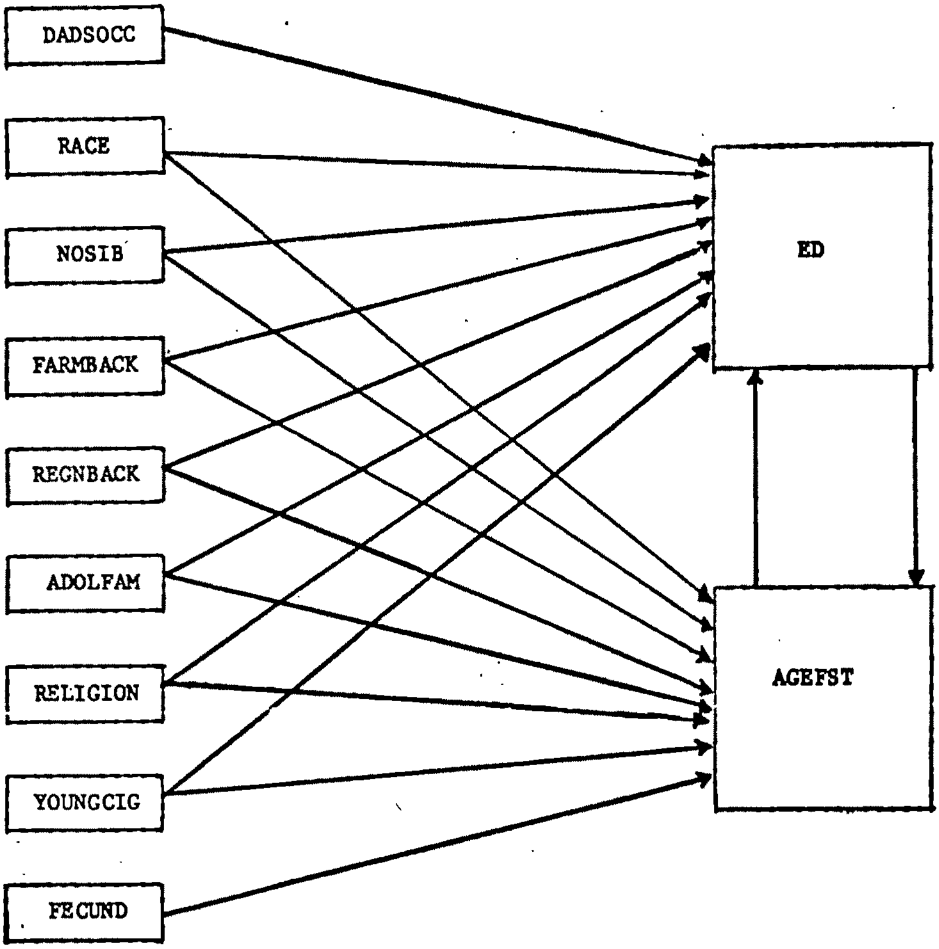
Although not reported in detail here, wherever possible we have also examined data from the 1973 National Survey of Family Growth (FGS) (NCHS, 1978) (a national probability sample of 9,797 women under age 45 who had ever been married or who were never married mothers in 1973), and essentially comparable results were found in both data sets.

Education and Age at First Birth

In the absence of accurate data on the intermediate variables, the relationship between the fertility and educational processes can be conceptualized as a simple causal process. The aspirations, plans, and decisions (and "apparent" nondecisions) leading to an early first birth may result in lowered educational aspirations and achievement. Women who desire and obtain a high level of education may adjust their fertility preferences accordingly. Both the educational and the first birth process are affected by a set of exogenous factors reflecting background characteristics and characteristics of early adolescence. A model of these relationships is shown in Figure 1. The rationale for this set of exogenous variables, and their effects on education and age at first birth, is considered elsewhere (Rindfuss and St. John, 1979); in the present paper we concentrate only on the relationship between education and age at first birth. Table 1 indicates the measurement of these exogenous variables, and the Appendix reports the zero-order correlations among all the variables in Figure 1.

That the relationship between education and age at first birth should be viewed as potentially reciprocal is often overlooked: one direction of causation is usually emphasized to the exclusion of the other. For example, Jaffe (1977:22) asserts: "Pregnancy is the most common cause of school dropout among adolescent girls in the U.S." Others, however, contend that education determines age at first birth; and, further, that women who get pregnant while still in school do so to have an "acceptable" reason for dropping out of school (Cutright, 1973). Since there is considerable overlap in the time when women leave school and the time when they have their first child (median age at first birth is currently about 22, and, of recent cohorts, 25% have their first birth by the end of the 19th year), it is important to investigate the extent to which the educational attainment process and age at first birth process are reciprocally related.

The part of the model shown in Figure 1 of direct interest here is the relationship between education and age at first birth. We allow for a reciprocal relationship between these two variables, with each affected by other variables in the model as well. Age at first birth is computed from the date of respondent's birth and date of birth of respondent's first child. Education, as noted, is education at marriage, not education at first birth. In order to estimate the reciprocal relationships between education and age at first birth, instrumental variables are needed for each of the two endogenous variables—that is, variables are needed which directly affect one of the endogenous variables but not the other, which are not causally determined by the endogenous variables, and which are not correlated with the unspecified source of the endogenous vari-



Corresponding equations:

$$\widehat{ED} = b_0 + b_1 \text{ DADSOCC} + b_2 \text{ RACE} + b_3 \text{ NOSIB} + b_4 \text{ FARMBACK} + b_5 \text{ REGNBACK} + b_6 \text{ ADOLFAM} + b_7 \text{ RELIGION} + b_8 \text{ YOUNGCIG} + b_9 \text{ AGEFST} + U$$

$$\widehat{AGEFST} = c_0 + c_1 \text{ RACE} + c_2 \text{ NOSIB} + c_3 \text{ FARMBACK} + c_4 \text{ REGNBACK} + c_5 \text{ ADOLFAM} + c_6 \text{ RELIGION} + c_7 \text{ YOUNGCIG} + c_8 \text{ FECUND} + c_9 \text{ ED} + V$$

Figure 1. A Model of the Relationship between Educational Attainment and the Beginning of Motherhood

Table 1. Measurement of Variables in the Education and Age at First Birth Model

Variable Label	Variable Name	Units of Measurement	Description	Mean	Standard Deviation
DADDSOCC	Respondent's father's occupation	Duncan's SEI scores	Missing data were given the mean value	30.209	21.370
RACE	Race of respondent	Dummy variable	Blacks are coded 1, all others coded 0	0.099	0.299
NOSIB	Respondent's number of siblings	Actual number is coded		3.889	3.035
FARMBACK	Respondent's farm background	Dummy variable	Coded 1 if respondent grew up on a farm, all others coded 0	0.330	0.470
REGNBACK	Region where respondent grew up	Dummy variable	Coded 1 if respondent grew up in South, all others coded 0	0.357	0.479
ADOLFAM	Household composition when respondent was 14	Dummy variable	Coded 1 if respondent lived with 0 or 1 of her parents at age 14, coded 0 if she lived with both parents at age 14	0.183	0.387
RELIGION	Respondent's religious preferences when growing up	Dummy variable	Coded 1 if Catholic, all others coded 0	0.231	0.421
YOUNGCIG	Whether respondent smoked at a young age	Dummy variable	Coded 1 if respondent smoked before age 16, coded 0 otherwise	0.136	0.342
FECUND	Whether respondent had a miscarriage before the first birth	Dummy variable	Coded 1 if respondent had a miscarriage before the first birth; coded 0 otherwise	0.099	0.298
ED	Respondent's education	Years of schooling completed	This is education at first marriage	11.595	2.360
AGEFST	Respondent's age at first birth	Years		22.012	4.079

Note: Throughout, the usual practice of deleting missing data cases is followed.

able for which it is not an instrument (Duncan, 1975; Heise, 1975). As can be seen from Figure 1, fecundity is used as the instrument for age at first birth and respondent's father's occupation as the instrument for education. Fecundity is measured by whether or not the respondent had a miscarriage prior to her first birth.[5] A miscarriage before the first birth postpones the first birth in a direct and obvious way: it takes time to conceive again and carry that conception to successful parturition. It also gives the woman a second chance if she wants to contracept. The additional time involved as the result of a miscarriage before the first birth can be substantial since approximately one-fourth of the women who have one miscarriage before their first birth have two or more miscarriages before their first birth.

A miscarriage before the first birth should have no effect on education, except indirectly through age at first birth. This would occur only if the woman dropped out or was expelled from school prior to the miscarriage because of the pregnancy. If this were the case, then the miscarriage would be correlated with the disturbances in the education equation and would be unsuitable as an instrument. However, this is unlikely because the vast majority of miscarriages occur in the early months of a pregnancy, before it is obvious to observers that the woman is pregnant, and often before the woman knows that she is pregnant (see National Center for Health Statistics, 1966). If the woman is unmarried, she is unlikely to notify the school that she is pregnant until it becomes absolutely necessary. It is probably in part for this reason that unmarried women often do not seek prenatal care until very late in pregnancy (National Academy of Sciences, 1973). Furthermore, education should not have any effect on whether or not there is a miscarriage before the first birth. The only exception to this statement would involve a woman obtaining an induced abortion in order to complete her education. However, induced abortions are so grossly underreported in United States fertility surveys that reported miscarriages are essentially spontaneous miscarriages.

That respondent's father's occupation affects respondent's educational attainment is well known (Alexander and Eckland, 1974; Blau and Duncan, 1967; Kerckhoff and Campbell, 1977; Sewell and Hauser, 1977) and does not require further elaboration here. We also argue that father's occupation does not have a direct relationship with age at first birth. Rather, we would argue that the relationship is indirect through education. It can be expected that families of an orientation in which the father has a high status job would be more likely to encourage daughters to postpone the first birth than families of an orientation in which the father has a low status job. However, the most likely explicit and implicit justification for this encouragement would be to allow daughters time to complete their education, and thus the effect on age at first birth would be indirect. However, there may also be an intergenerational transmission of norms regarding age at first birth. (Leonetti [1978] provides a good example of this in the case of Japanese-Americans.) To the extent that socioeconomic status directly affects the intergenerational transmission of norms regarding age at first birth—that is, in addition to the indirect transmission through educational aspirations—then respondent's father's occupation would not be a suitable instrument for education. Recent work by Thornton (forthcoming) suggests that there is no direct transmission of fertility norms from parental status. Instead, this influence was transmitted through the education of the offspring.

---

[5] There is some evidence that a history of miscarriage greatly increases the chance that subsequent conceptions will be terminated by a miscarriage (Funderburk et al., 1976; Shapiro et al., 1971). Given the unreliability with which fetal losses are reported in pregnancy histories (Bumpass and Westoff, 1970) and given the fact that very early miscarriages are often unnoticed by the woman, we experimented with alternative and more complex measures of fecundity which incorporated information from the woman's history subsequent to the first birth. However, the simple measure of whether or not the woman had a miscarriage prior to the first birth proved to be the strongest predictor of age at first birth, and this is the measure that has been used in the final models.

In order to examine our assumption that parental socioeconomic status does not have a direct effect on the transmission of norms regarding age at first birth, we examined the determinants of ideal age at first birth. The 1970 NFS included the following question:

"Q. 3: What do you think is the ideal age for a woman to have her *first* child?"

Although this question suffers from all the problems of "ideal" questions (Blake, 1966; Bumpass and Westoff, 1970; Rindfuss, 1974; Ryder and Westoff, 1969) as well as some problems specific to this question (Rindfuss and Bumpass, 1978), it does provide the best measure available for norms regarding age at first birth. Using a sample of recently married women in order to minimize the possibility that the responses to the question would be affected by the cumulative maternal experience of the woman, we find that, after other appropriate factors are controlled, father's occupation has no significant direct effect on the ideal age to have a first birth. This further supports Thornton's results and supports the theoretical argument that parental socioeconomic status influences age at first birth only indirectly through its effect on the offspring's educational aspirations, and thus supports the use of father's occupation as an instrument for education in our model.

However, somewhat less consistent support was found in an examination of the 1971 National Survey of Young Women data. Since father's occupation was not available, the relationship between father's education and ideal age at first birth was considered for this sample of teenagers 15–19 years of age. While most of the association is accounted for by educational aspirations, ideal age at first birth is 0.4 years lower among the children of high school graduates than among those of fathers who attended college, net of other factors. While this modest net effect of father's education cautions our theoretical position, we would expect the net effect of father's occupation on ideal age at first birth to be considerably weaker.

Before presenting the results, it is necessary to discuss some of the variables which are not included in Figure 1 and the possible biases their exclusion might introduce. The first is marriage. Although we recognized the role of age at marriage in the earlier discussion in this paper (especially since it is incorporated in sexual experience), it is age at first birth that is emphasized both there and in our analysis here. Clearly, age at first marriage and age at first birth are closely related, normatively and empirically. However, we feel that the first birth has greater consequences for the life style and roles of the woman (Rindfuss, 1979), and that the effects of the first birth are more permanent than those of first marriage. Marini (1978) has recently argued that age at marriage is more important than age at first birth in the transition to adulthood because age at marriage "usually sets a lower limit on the age at which first birth occurs." We disagree for the following reasons: In the first place, motherhood frequently precedes first marriage. (And this is more likely to be the case the younger the age at first birth.) Second, some people may initiate the serious consideration of marriage on the basis of when they want to begin parenthood, as reflected in the phrase "time to settle down and start a family." The high incidence of premarital intercourse argues against the notion that age at first marriage sets a lower bound on exposure to the risk of conception. Third, "becoming a parent" is the modal response of married parents to the question of what marks the transition to adulthood (Hoffman, 1978). Fourth, parenthood is more permanent than marriage, particularly for women since children tend to stay with the mother following a marital disruption. Preston (1975) has estimated that almost half of the current marriages will end in divorce; thus, women often move in and out of the wife role. Finally, and perhaps most importantly, motherhood roles more severely constrain other life options of a woman than do marital roles, especially during the early childbearing years. For these reasons, our emphasis is on age at first birth. Given the high correlation between age at first birth and age at first marriage, and given that both are affected by similar exogenous variables, we have not included both in the analysis. Furthermore, given the assumptions of the model, the exclusion of age at first marriage will not bias our estimates of the relative importance of the processes leading to educational attainment and to the first birth.

In order to allow women sufficient time to get married (and, thus, be eligible to be in the sample) and to have a first birth, the analysis of the education-age at first birth relationship will be limited to women aged 35–44. Most of those who will ever marry before the end of the reproductive period are married by age 35. For example, the proportion of women ever married increases from 0.873 at ages 25–29 to 0.926 at ages 30–34 to 0.941 at ages 35–39. But the proportion of women ever married increases only slightly to 0.946 at ages 40–44 (U.S. Bureau of the Census, 1972). The same holds true for first births. Most of those who will ever give birth do so by age 35. For example, 79.2% of the birth cohort of 1930–1934 had a live birth by ages 25–29, 87.7% did so by ages 30–34, and 90.2% had a live birth by ages 35–39. This percentage increased only slightly to 90.8% by ages 40–44. Less than 3% of the women in this birth cohort who had a live birth had it after age 35 (Heuser, 1976).

Childless women are excluded from this analysis at age at first birth. Only a small proportion (less than 10%) of the married women in these cohorts remained childless (Heuser, 1976). To the extent that postponement leads to voluntary childlessness (Veevers, 1973), this exclusion could lead to a weaker estimated effect of age at first birth than actually exists. However, childlessness in these cohorts was primarily a product of fecundity impairments.

The model shown in Figure 1 includes background characteristics, aspects of early adolescence, and the reciprocal relationship between education and age at first birth. Period factors are not included, and this needs to be kept in mind when interpreting our results. The respondents in this analysis were aged 35–44 in 1970. Taking 15 as the youngest age at first birth and 35 as the oldest means that these women were having their first births from 1941 to 1970. During this long period, there were a number of events affecting the timing of fertility, including World War II, the Korean War, and the Vietnam War. Those women who postponed their first birth were, of course, exposed to more of these period factors, which could affect the timing of their first birth. Since so little is known about the nature of period factors that affect the timing of fertility (Rindfuss et al., 1978), they cannot be explicitly included in the analysis. Furthermore, the younger women in our sample experienced the period factors at different ages than the older women in the sample. To see if this would affect our results, we ran the model separately for women aged 35–39 and 40–44. The results were virtually identical for the two groups.

The work of Easterlin (1962; 1966 and 1973) and others suggests that the financial status of the respondent's family of orientation while the respondent was an adolescent will affect the age at which she has her first child. Unfortunately, we do not have a direct measure of the respondent's parents' financial status while the respondent was an adolescent. However, a number of background variables in the model, such as race, number of siblings, farm background, regional background, and family composition when respondent was 14, indirectly control for the respondent's family's financial situation.

Further, the model shown in Figure 1 also does not include the labor force experiences of women. As noted earlier, labor force experiences and aspirations are likely to affect, and be affected by, childbearing and childbearing preferences. In fact, there is a long literature on this relationship (see Waite and Stolzenberg, 1976; and Smith-Lovin and Tickamyer, 1978, for recent summaries of this literature). Unfortunately, adequate labor force participation information is not available.

Estimation of the effects shown in Figure 1 was accomplished by using two-stage least squares regression analysis (Goldberger, 1964; Johnston, 1972). The estimates were made using ordinary least squares in two steps, making the appropriate corrections as outlined by Hout (1977). The results are shown in Table 2.

Table 2. Metric and Standardized Coefficients Measuring the Reciprocal Relationship between Education and Age at First Birth, 1970 NFS[a]

Independent Variable	Dependent Variable	Metric Coefficient	Standardized Coefficient
Education	Age at First Birth	0.741*	0.429*
Age at First Birth	Education	0.075	0.130
	Correlation of Disturbances (U and V): −0.255		

[a] N = 1,766.

* Significant at 0.05.

This table shows only the results for the endogenous variables; results for the complete model are reported and discussed elsewhere (Rindfuss and St. John, 1979).

The effect of education on age at first birth is significant—both statistically and substantively. Each additional year of schooling results in the delay of the first birth by approximately three-quarters of a year. However, the effect of age at first birth on education is not statistically significant; and even if it were, the effect would be trivial substantively.

The results shown in Table 2 are based on the assumption of linear effects. It might be argued that the effect of age at first birth on education is not linear. The inclination to have a birth at a very young age may have more serious effects on educational plans than the preference to have a child at a later age. The potential conflict between school and motherhood is greatest at the younger ages at first birth. This suggests that a nonlinear age at first birth effect on education should be specified. Such a specification should force a difference of a year at the younger ages at first birth to be larger than a difference of a year at the older ages at first birth. We used three different transformations of age at first birth (AGEFST) to explore this possibility: (1) LN (AGEFST), (2) 1/AGEFST, and (3) $1/(\mathrm{AGEFST})^2$. The model shown in Figure 1 was reestimated for each of these three transformations. In each case the results are the same as the linear model: age at first birth does not have a significant effect on educational attainment.

Furthermore, there is some evidence to suggest that the family building process may be different for whites and blacks. For example, blacks have higher illegitimacy rates than whites (NCHS, 1977), and blacks appear to rely more heavily on relatives to take temporary, but primary, care of children born to young mothers than whites (Rindfuss, 1977). In order to check for a potential interaction with race, we reran the analysis separately for whites and blacks.[6] The important point for the present analysis is that, for both blacks and whites, education has a strong and significant effect on age at first birth, but age at first birth has an insignificant effect on education. Thus, our results are unaffected by any racial interaction.

In the relationship between education and age at first birth, the principal direction of causality is from education to age at first birth. Those who have recently examined the relationship between education and age at first marriage have found corroborating results (Marini, 1978; Alexander and Eckland, 1978), namely, that education has a much stronger effect on age at first marriage than age at first marriage has on education. Given the sheer amount of time the mother role requires in contrast to the wife role, the timing of the first birth has greater consequences for the roles women occupy. Yet, it is interesting to note that (ignoring the differences between the samples used here and those used by Marini [1978] and Alexander and Eckland [1978]), age at first marriage appears to have a somewhat greater effect on education than age at first birth. Even though age at first birth has a greater effect on the roles occupied by women, age at first marriage could have a stronger effect on educational attainment because first marriage schedules are younger and more compact than first birth schedules. Thus, more marriages take place during the years in which women are in school.

---

[6] Other nonwhites were not included.

The finding that age at first birth has only a very small effect on educational attainment may seem paradoxical, given the social policy concern with the pregnant girls who have to drop out of school and face reduced social opportunities as a consequence. Such a fate is unquestionably experienced by some women, particularly those among the 3% to 6% of the American cohort that have had a first birth before age 17. But the fact is that the vast majority of women do not get pregnant while they are enrolled in school. Even among those who do become mothers at ages at which society expects one to be in school, the direction of causality might run from education to fertility. Zelnik and Kantner (1978) and Ross (1978) suggest that a significant minority of premarital pregnancies were intentional. To further explore this issue, we compared the age at leaving school[7] with age at first birth for women who become mothers at age 17 or younger. If leaving school and the first birth occur in the same year, it is ambiguous which process dominates. But for those who left school more than a year before their first birth, one can assume that the educational process is affecting the fertility process. Surprisingly, more than 40% of the women who had a first birth at age 17 or less dropped out of school at least a year prior to becoming a mother—which suggests that even at the very young ages at motherhood, the fertility process is being affected by the educational process.[8] Further, there is longitudinal evidence showing a negative relation between educational aspirations and age at first birth (Marshall and Cosby, 1977; Card and Wise, 1978, Table 3), which suggests that many of those who have a first birth while they are of school age do so after deciding not to continue in school—and, perhaps, do so to justify dropping out of school. Finally, Haggstrom and Morrison (1979) find that among teenagers who do not drop out of high school, the effects of adolescent parenthood on subsequent educational aspirations are extremely small when other appropriate factors are controlled. All of this does not mean that fertility never truncates education, but only that it does so rarely. In the vast majority of the cases, education and educational aspirations determine age at first birth.

It is important that scientific discourse clarify the difference between a social policy concern that requires amelioration and the characterization of the overall process in which that concern is embedded.

Education and the Lengths of Birth Intervals

As discussed in the first section of this paper, we would expect to find a variety of reasons why women with more education would want to avoid very short birth intervals and we would expect them to be more effective at implementing their preferences. In this section we examine the relationship between education and the probability of having a short interbirth interval. Unlike the previous section, here, we assume that the direction of causality runs from education to the length of birth intervals.[9]

The birth history information contained in the 1970 NFS allows us to compute the length of each birth interval. Given the well-known difficulties involved in the analysis of birth intervals (see Bumpass et al., 1977, for a fuller discussion), we ini-

---

[7] Age at leaving school was computed by assuming a normal starting age, and assuming that education is obtained one year at a time.

[8] To further explore this issue, and to explore whether a gating mechanism existed, we reran the two-stage least squares analysis for women who became mothers at a young age. Although caution is necessary in interpreting such an analysis because the variance of the endogenous variables has been reduced, age at first birth does not have a significant effect on education.

[9] It should be noted, however, that it is possible that, for some women, short interbirth intervals prevent the return to school. Virtually nothing is known about returning to school after becoming a mother, although there has been some research on education after marriage. Approximately one in five women attend school after marriage; but the average addition to their educational attainment is relatively small: 1.0 years (Davis and Bumpass, 1976). Whether this schooling takes place before or after the start of childbearing is unknown. In order to minimize the possibility of education after the first birth being affected by the pace of fertility, we have primarily used education at marriage (rather than education at interview) for this analysis.

Table 3. Differentials in the Proportion Experiencing Birth Intervals of 18 Months or Less, for All Second, Third and Fourth Birth Intervals Begun 1959–1968, by Education, Gross and Net[a] Percentages: 1970 NFS

Education at Marriage	Second Birth Interval N	Gross	Net	Third Birth Interval N	Gross	Net	Fourth Birth Interval N	Gross	Net
Total	2612	25		2236	19		1551	17	
1–8	155	33	31	168	32	29	164	29	25
9–11	657	28	26	592	21	18	433	20	17
12	1218	24	23	1016	16	17	670	15	16
13–15	388	22	24	312	17	20	202	11	14
16+	194	20	29	149	16	21	82	13	18

[a] Adjusted through a dummy variable regression analysis for the effects of race, religion, region, age at first birth, marital status at first birth, contraceptive use before the first birth, planning status of first birth and smoking before age 16.

tially constructed life tables for each birth interval. These preliminary life tables were constructed for intervals begun in the period 1959–1968. By restricting the analysis to intervals begun in this period, we avoid a young-age-at-initiation bias (see Rindfuss and Bumpass, 1979).

The preliminary life table analyses showed the expected positive relationship between education and length of intervals. However, this conclusion is based on a bivariate analysis, and there are numerous other factors affecting the length of birth intervals (e.g., Bumpass et al., 1978), and the effects of these factors should be controlled. Unfortunately, the sample size of the 1970 NFS (or the 1973 FGS) is far too small to permit the simultaneous control of all these factors by using conventional life table techniques. Consequently, we used regression analysis to examine the probability of giving birth within a relatively short time interval—specifically, the probability of giving birth within 18 months of the previous birth. Because the life table results suggested that the differences in interbirth interval length are greater between adjoining categories at the lower educational categories than at the higher educational categories, we used a variant of multiple regression analysis, Multiple Classification Analysis (Andrews et al., 1973), to see if this pattern continued when other factors were controlled. The results are summarized in Table 3.

Controlling for other factors that affect the length of interbirth intervals eliminates much of the relationship between education and the probability of having a short birth interval. Compare the gross and net columns for the second, third and fourth birth intervals.[10] The difference which remains after controlling for other variables is primarily between those with a grade school education and all others. Given that those with only a grade school education are a small proportion of the population, and since the proportion with only a grade school education is declining, the principal result to emerge from Table 3 is that, when the effects of other factors are controlled, the respondent's education at first marriage has essentially no effect on the probability of having a short second, third or fourth birth interval.[11]

EDUCATION AND FERTILITY PREFERENCES

As discussed earlier in this paper, educational preferences and fertility preferences affect each other; and, since neither is fixed, their interrelationship develops over time. To examine adequately this complex set of interrelationships would require longitudinal data of the kind not currently available. However, in the absence of the appropriate longitudinal data, it is still possible to examine part of the process by looking at the effect of education at marriage on fertility preferences at time of interview. Framed this way, the causal direction is essentially unambiguous.

---

[10] We follow the standard convention of indexing birth intervals by the order of the fertile pregnancy terminating the interval. Thus, the second birth interval is the interval terminated by the second fertile pregnancy.

[11] The results in Table 3 are based on all birth intervals. Thus, both wanted or intended intervals and unwanted or unintended intervals are included. To make sure that the relationships shown in Table 3 were not the result of differences in fertility intentions, we calculated a set of life tables for "intended" intervals, excluding the following two types of intervals: (a) closed intervals that were closed by an unwanted birth, and (b) open intervals where the respondent indicates she does not intend to have another child. These results (not shown) are virtually identical to those shown in Table 3. Also, in order to see if the finding was sensitive to the particular measure of education used, we reran the analysis using respondent's education at interview, and then we reran it again using respondent's husband's education at respondent's first marriage. These alternative analyses lead to the same conclusions.

Education at marriage can affect fertility preferences in two ways. First, education at marriage can have a direct effect on fertility preferences. Insofar as increased education makes a larger variety of roles available to women, we could expect education to have a direct and negative effect on fertility preferences. In addition, specific topics covered while in school might have a direct negative effect on fertility preferences. Second, education at marriage can have an indirect effect on fertility preferences through its effect on age at first birth. As shown earlier, higher levels of educational attainment result in older ages at first birth. An older age at first birth, in turn, leads to longer intervals between births (Bumpass et al., 1978). Thus, education leads to older ages at any given parity; and older ages at any given parity have a negative effect on the probability of wanting another child (Rindfuss and Bumpass, 1978).

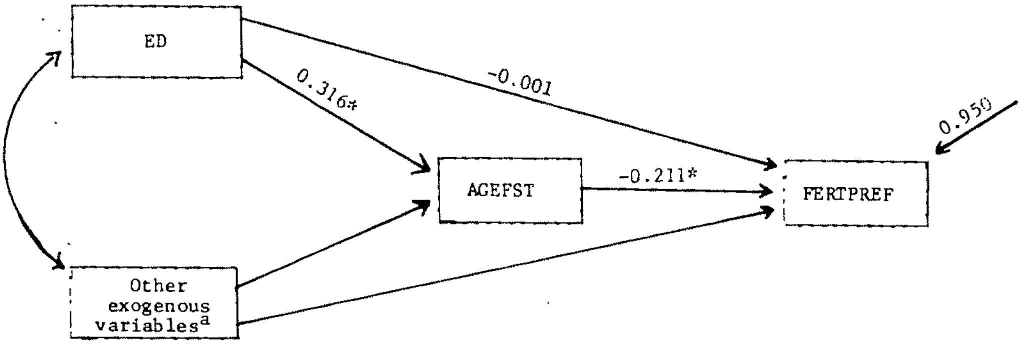
The measure of fertility preferences used here, FERTPREF, is the sum of the number of "wanted" children the woman had had by the time of the interview plus the additional number of children she intended to have. For each live birth, the woman was asked a series of questions to determine whether or not, before that child was conceived, she wanted to have a birth of that order at some time during her reproductive life (see Westoff and Ryder, 1977, for a more detailed description). Such a series of questions minimizes the possibility of post factum rationalization of unwanted births (Rindfuss, 1974). The additional number intended is obtained from a question asking the respondent how many additional children she intended. This fertility preference measure is coded in numbers of children and has a mean of 2.9, and a standard deviation of 1.5.[12]

Because one of our interests is in the mediating effect of age at first birth, the sample being analyzed is limited to mothers, that is, women who have had at least one live birth. As in the previous two sections, in order to allow women sufficient time to get married and have a first birth, younger women are excluded from the analysis. The analysis in this section, like the age at first birth analysis, will be restricted to respondents aged 35–44 at the time of the interview. Because the full set of questions used in constructing our fertility preference measure was not asked of postmarried women (i.e., those widowed, divorced or separated at the time of the interview), the analysis will be limited to currently married women. Finally, for ease of presentation, the set of exogenous variables to be used here, in addition to education at first marriage, is exactly the same as those shown in Figure 1 and described in Table 1. We have experimented with other sets of exogenous variables and with other definitions of the sample, and the results are similar in all cases.

The results are summarized in Figure 2. In order to focus on the education-fertility preference relationship, only the direct and indirect effects of education are shown. It can be seen that the direct effect of education on fertility preferences is trivial and insignificant. Virtually all of the effects of education at marriage on fertility preferences operates through age at first birth. Furthermore, the importance of age at first birth in influencing fertility preferences at time of interview should be underscored. Although it is not shown in Figure 2, age at first birth has a stronger direct effect on fertility preferences measured at time of interview than any of the listed exogenous variables. Thus, it appears that education affects fertility preferences by sorting women into various ages at first birth.

---

[12] It should be noted that there is little variance in fertility preferences. Three-fourths of the sample gave a preference of 2, 3 or 4. This, of course, reduces the possibility of any variable significantly affecting fertility preferences.



TOTAL EFFECT: ED → FERTPREF = -0.058

[a] The other exogenous variables in the model are: DADSOCC, RACE, NOSIB, FARMBACK, REGNBACK, ADOLFAM, RELIGION, YOUNGCIG, AND FECUND. See Table 1 for a description of the measurement of these variables.

[b] N4h1 1,551.

[c] significant at 0.01.

Figure 2. A Model[a] of the Relationship between Education at Marriage and Fertility Preferences (Standardized Coefficients)[b]

For approximately four-fifths of these women, education at first marriage is the same as education at interview; but one-fifth of these women have attended school since their first marriage (Davis and Bumpass, 1976). For many women, this school attendance takes place a considerable time after the first marriage. For example, for women first married between 1951 and 1955 who returned to school after marriage, 62% last attended school 10 or more years after the first marriage. This additional schooling could affect fertility preferences, or could be affected by fertility preferences. We do not have the appropriate data to sort out these possibly reciprocal influences. But we did rerun the analysis in Figure 2 using education at interview instead of education at marriage, and the results are suggestive. The finding, as before, is that most of the relationships between education and fertility preferences operate through age at first birth. However, the direct relationship between education and fertility preferences is somewhat larger when education at interview is used than when education at first marriage is used. Without being able to sort out the potential reciprocal effects, we can only speculate that education after marriage operates to provide options that would not otherwise be available, or is itself a response to (or simultaneous with) a decision to terminate childbearing earlier than planned. This issue is something that warrants further examination.

CONCLUSION

To summarize, the reciprocal relationship between education and age at first birth is dominated by the effect from education to age at birth, with only a trivial effect in the other direction.

Once the process of childbearing has begun, education has essentially no direct effect on that process. Education has little direct effect on either the length of interbirth interval or on fertility preferences. Work by Vaughn and her colleagues (1977) shows that education has no direct effect on contraceptive efficacy. However, education has a significant indirect effect on these various components of fertility because it is the major determinant of age at the beginning of childbearing; in fact, education has a substantially greater influence on age at first birth than any other variable (Rindfuss and St. John, 1979). Thus, it is the postponing of motherhood that produces the oft-observed negative bivariate relationship between education and children ever born.

The powerful mediating effect of age at first birth is of interest in its own right. Older ages at first birth lead to longer interbirth intervals (Bumpass et al., 1978), more effective contraceptive use (Vaughn et al., 1977), and preferences for fewer children (as shown in the previous section of this paper).

These results, particularly if they are supported by future research on more recent cohorts, raise a set of interesting policy issues about which we can only speculate at present. Because the postponement of something is always more amenable to policy initiatives than its prevention, policies aimed at influencing age at first birth would be more likely to succeed than policies aimed at directly influencing children ever born. Furthermore, how adolescents spend their time has been accepted (although not universally) as something governments can legitimately influence—the military draft system is the most obvious example.

We began with the observation that a major way education might affect the roles women occupy is through altering the structure of childbearing experience, given the dominance of mother roles. We conclude that such educational effects as we can identify are explicable more in terms of education's effect on age at first motherhood than in terms of other values or aspirations that might derive from advanced schooling.

APPENDIX

ZERO/ORDER CORRELATIONS AMONG BACKGROUND FACTORS, EARLY ADOLESCENT CHARACTERISTICS, EDUCATION AND AGE AT FIRST BIRTH: 1970 NFS, WOMEN AGED 35–40 WITH 1 + CHILDREN

	DADSOCC	RACE	NOSIB	FARMBACK	REGNBACK	ADOLFAM	RELIGION	YOUNGCIG	FECUND	ED	AGEFST
DADSOCC	1.000										
RACE	−.144	1.000									
NOSIB	−.244	.156	1.000								
FARMBACK	−.323	.088	.274	1.000							
REGNBACK	−.129	.315	.150	.218	1.000						
ADOLFAM	−.056	.150	−.039	−.030	.071	1.000					
RELIGION	.053	−.152	.014	−.149	−.292	−.052	1.000				
YOUNGCIG	−.043	.030	.028	−.060	−.011	.067	−.010	1.000			
FECUND	.037	.035	.002	−.032	−.027	.018	−.002	.009	1.000		
ED	.370	−.222	−.328	−.185	−.211	−.157	−.012	−.171	.038	1.000	
AGEFST	.186	−.189	−.115	−.118	−.177	.111	.098	−.122	.216	.380	1.000

Note: Definitions, means and standard deviations for these variables are found in Table 1.

REFERENCES

Alexander, K. L. and B. K. Eckland

1974 "Differences in the educational attainment process." American Sociological Review 39:668–82.

1978 "Family formation and educational attainment: alternative models." Manuscript.

Andrews, F. M., J. N. Morgan, J. A. Sonquist, and L. Klem

1973 Multiple Classification Analysis. Ann Arbor: Institute for Social Research.

Blake, J.

1966 "Ideal family size among white Americans: a quarter of a century's evidence." Demography 3:154–73.

Blau, P. M. and O. D. Duncan

1967 The American Occupational Structure. New York: Wiley.

Bongaarts, J.

1978 "A framework for analyzing the proximate determinants of fertility." Population and Development Review 4:105–32.

Bumpass, L. L., R. R. Rindfuss and R. B. Janosik

1978 "Age and marital status at first birth and the pace of subsequent fertility." Demography 15:75–86.

Bumpass, L. L. and C. F. Westoff

1970 The Later Years of Childbearing. Princeton: Princeton University Press.

Card, J. J. and L. L. Wise
1978 "Teenage mothers and teenage fathers: the impact of early childbearing on the parents' personal and professional lives." Family Planning Perspectives 10:199–205.

Cho, L. J., W. H. Grabill, and D. J. Bogue
1970 Differential Current Fertility in the United States. Chicago: Community and Family Study Center.

Cutright, P.
1973 "Timing the first birth: Does it matter?" Journal of Marriage and the Family 35:585.

Davis, K. and J. Blake
1956 "Social structure and fertility: an analytic framework." Economic Development and Cultural Change 4:211–35. .

Davis, N. J. and L. L. Bumpass
1976 "The continuation of education after marriage among women in the United States: 1970." Demography 13:161–74.

Duncan, O. D.
1975 Introduction to Structural Equation Models. New York: Academic Press.

Easterlin, R. A.
1962 "The American baby boom in historical perspective." National Bureau of Economic Research, Occasional Paper 79. New York.
1966 "On the relation of economic factors to recent and projected fertility changes." Demography 3:131–53.
1973 "Relative economic status and the American fertility swing." Pp. 170–223 in Eleanor Bernert Sheldon (ed.), Family Economic Behavior. Philadelphia: J. B. Lippincott.

Funderburk, S. J., D. Guthrie and D. Meldrum
1976 "Suboptimal pregnancy outcome among women with prior abortions and premature births." American Journal of Obstetrics and Gynecology 126:55–60.

Goldberger, A. S.
1964 Econometric Theory. New York: Wiley.

Haggstrom, G. W. and P. A. Morrison
1979 "Consequences of parenthood in late adolescence: findings from the national longitudinal study of high school seniors." Paper presented at the annual meeting of the Population Association of America, Philadelphia, April.

Hout, M.
1977 "A cautionary note on the use of two-stage least squares." Sociological Methods and Research 5:335–46.

Hout, M. and W. R. Morgan
1975 "Race and sex variations in the causes of the expected attainments of high school seniors." American Journal of Sociology 81:364–94.

Heise, D. R.
1975 Causal Analysis. New York: Wiley.

Heuser, R. L.
1976 Fertility Tables for Birth Cohorts by Color. Rockville, Md.: National Center for Health Statistics.

Hoffman, L. W.
1978 "Effects of the first child on the woman's role." Pp. 340–67 in W. B. Miller and L. F. Newman (eds.), The First Child and Family Formation. Chapel Hill: Carolina Population Center.

Holsinger, D. B. and J. D. Kasarda
1976 "Education and human fertility: sociological perspectives." Pp. 154–81 in R. G. Ridker (ed.), Population and Development. Baltimore: Johns Hopkins University Press.

Jaffe, F. S.
1977 "View from the United States." Pp. 19–29 in D. Bogue (ed.), Adolescent Pregnancy. Chicago: Community and Family Study Center.

Johnston, J.
1972 Econometric Methods. 2nd ed. New York: McGraw-Hill.

Kantner, J. F. and M. Zelnik
1972 "Sexual experiences of young unmarried women in the United States." Family Planning Perspectives 4:9–17.

Kerckhoff, A. C. and R. T. Campbell
1977 "Black-white differences in the educational attainment process." Sociology of Education 50:15–27.

Leonetti, D. C.
1978 "The biocultural pattern of Japanese-American fertility." Social Biology 25:38–51.

Marini, M. M.
1978 "Transition to adulthood." American Sociological Review 43:483–507.

Marshall, K. P. and A. G. Cosby
1977 "Antecedents of early marital and fertility behavior." Youth and Society 9:191–212.

National Academy of Sciences
1973 Infant Death: An Analysis by Maternal Risk and Health Care. Washington, D. C.: National Academy of Science.

National Center for Health Statistics
1966 Infant, Fetal and Maternal Mortality, U. S., 1963. Series 20, No. 3. Washington, D. C.: U. S. Government Printing Office.
1977 Vital Statistics of the United States, 1973, Volume 1—Natality. Washington, D. C.: U. S. Government Printing Office.
1978 "National survey of family growth, cycle I: sample design, estimation procedures, and variance estimation." Vital and Health Statistics Series 2, No. 76.

Preston, S.
1975 "Estimating the proportion of American marriages that end in divorce." Sociological Methods and Research 3:435–60.

Rindfuss, R. R.
1974 Measurement of Personal Fertility Preferences. Ph. D. dissertation, Department of Sociology, Princeton University.
1977 "Methodological difficulties encountered in using own-children data: illustrations from

the United States." East-West Population Institute Paper Series. Honolulu: East-West Population Institute.
1979 "Changes in the timing of fertility: implications for industrialized societies." Unpublished manuscript.

Rindfuss, R. R. and L. L. Bumpass
1977 "Fertility during marital disruption." Journal of Marriage and the Family 39:517–28.
1978 "Age and the sociology of fertility: How old is too old?" Karl Taeuber, Larry Bumpass and James Sweet (eds.), Social Demography. New York: Academic Press.
1979 "The analysis of childspacing: illustrations from Korea and the Philippines." Presented at the annual meeting of the Population Association of America, Philadelphia, April.

Rindfuss, R. R. and J. S. Reed and C. St. John
1978 "A fertility reaction to a historical event: southern white birthrates and the 1954 desegregation ruling." Science 201:178–80.

Rindfuss, R. R. and C. St. John
1979 "Social determinants of age at first birth." Manuscript.

Rindfuss, R. R. and J. A. Sweet
1977 Postwar Fertility Trends and Differentials in the United States. New York: Academic Press.

Ross, S.
1978 The Youth Values Project. Washington: The Population Institute.

Ryder, N. B. and C. F. Westoff
1969 "Relationships among intended, expected, desired and ideal family size: United States, 1965." Center for Population Research Working Paper (March). Washington, D.C.

Sewell, W. H. and R. M. Hauser
1977 Education, Occupation and Earnings. New York: Academic Press.

Shapiro, S., H. S. Levine and M. Abramowicz
1971 "Factors associated with early and late fetal loss." Advances in Planned Parenthood 6:45–58.

Smith-Lovin, L. and A. R. Tickamyer
1978 "Nonrecursive models of labor force participation, fertility behavior and sex role attitudes." American Sociological Review 43:541–57.

Thornton, A.
Forthcoming "The difference of first generation fertility and economic status on second generation fertility." Journal of Population: Behavioral, Social, and Environmental Issues.

U. S. Bureau of the Census
1972 Census of the Population: 1970 Marital Status, PC(2)–4C. Washington: U. S. Government Printing Office.

Vaughn, B., J. Trussell, J. Menken, and L. Jones
1977 "Contraceptive failure among married women in the U.S., 1970–1973." Family Planning Perspectives 9:251–7.

Veevers, J. E.
1973 "Voluntary childless wives: an exploratory study." Sociology and Social Research 57:356–65.

Voss, P. R.
1977 "Social determinants of age at first marriage in the United States." Paper presented at the annual meeting of the Population Association of America, St. Louis, April.

Waite, L. J. and K. A. Moore
1978 "The impact of an early first birth on young women's educational attainment." Social Forces 56:845–65.

Waite, L. J. and R. M. Stolzenberg
1976 "Intended childbearing and labor force participation of young women: insights from nonrecursive models." American Sociological Review 41:235–52.

Westoff, C. F. and N. B. Ryder
1977 The Contraceptive Revolution. Princeton: Princeton University Press.

Whelpton, P. K., A. A. Campbell, and J. E. Patterson
1966 Fertility and Family Planning in the United States. Princeton: Princeton University Press.

Zelnik, Melvin and J. F. Kantner
1978 "First pregnancies to women aged 15–19: 1976 and 1971." Family Planning Perspectives 10:11–20.

SOME EXPERIMENTAL INVESTIGATIONS OF THE PRINCIPLES OF DIFFERENTIAL ASSOCIATION THROUGH DELIBERATE MANIPULATIONS OF THE STRUCTURE OF SERVICE SYSTEMS*

D. A. ANDREWS
Carleton University

American Sociological Review 1980, Vol. 45 (June:448–462)

The paper reviews a series of examinations of three key principles of differential association: the contingency principle, the socioemotional principle, and the principle that criminal acts occur as a function of an excess of definitions favorable to violation of the law. The studies sampled a variety of correctional clients in a variety of correctional settings and in each there were explicit attempts to exert direct experimental control over the factors of theoretical interest as well as competing factors. In total, the findings support both the causal and the practical significance of differential association while documenting (a) structural effects on the contingencies within the service system, and (b) the effects of those contingencies on the criminal attitudes and behavior of individuals. The discussion critically contrasts such systematic testing of assumptions with related programs which have been operating for years with limited theoretical and practical returns. The paper suggests, in total, that systematic program evaluation provides the tools—and the opportunity—for a bridging of the gaps between general sociology and general psychology and between social science theory and social service.

This paper reviews a series of deliberate and explicitly experimental investigations of the causal and, hence, practical significance of certain principles of differential association theory (DA). The principles were divided into two broad sets: the conditions which promote criminal learning (differentials in exposure to criminal and anticriminal patterns within intimate personal groups) and the conditions under which criminal learning becomes evident in criminal behavior (criminal behavior occurs when there is an excess of favorable definitions). Since controlled variations in the learning conditions were induced by way of structural interventions, the studies also speak to DA's frequently applauded ability to provide a theoretically consistent perspective at both the structural and individual levels (Akers, 1973; Cressey, 1960). An additional feature is the suggestion and documentation of an operational means of examining the "excess" principle as well as a means of experimentally analyzing the causal significance of symbolic interaction of the intrapersonal kind. Finally, and hopefully without attempting to blind the reader to the obvious limits of the specific studies, the review relates to a number of more general and recurring issues in the social sciences: the need to narrow the gap between social science theory and the design and evaluation of social services; the potential of a behavioral orientation as a complement to the still-dominant perspectives of symbolic interactionism and group dynamics theory; and the potential of an explicit, yet theoretically consistent, interdisciplinary approach to program evaluation. The

---

* Direct all communications to: D. A. Andrews; Department of Psychology; Loeb Building; Carleton University; Ottawa, Canada K1S5B6.

The studies were funded by a number of different agencies over the last seven years: the Planning and Research Branch of the Ontario Ministry of Correctional Services; the Canadian Penitentiary Service, the Correctional Consultation Centre and the Research Division of the Ministry of the Solicitor General of Canada; the Law Reform Commission of Canada; the Canada Council and currently the Social Science and Humanities Research Council of Canada. Thanks to the many colleagues and students who have contributed to the studies—in particular, Don Kennedy, Steve Wormith, Colin Farmer, Flo Hughes, and Jerry Kiessling; and Wendy Daigle-Zinn, Judith Hughes, Marina Kouri, Gordon Young, Roberta Russell, David Wayne, Brian Grant and Susan Mickus. Among those correctional professionals whose cooperation made the studies possible, thanks in particular to Roger Dupuis, John Braithwaite, Bill Jackson, Mort Smyth, Frank Kaar, Gerry Brown, Paul Gendreau, Andy Birkenmayer, Leah Lambert, Jody Gomber, and, again, Jerry Kiessling. Thanks to the prisoners, probationers, and volunteers who participated in the programs and agreed to allow specifics of the programs in which they participated to be subject to random assignment.

paper opens with a summary of the current status of DA, reviews the series of investigations which test it, and closes with some recommendations and conclusions regarding DA and the more general issues just noted.

The Status of Differential Association: Toward Tests of Causal Significance

DA has been the preeminent perspective on criminal conduct for most of its 40 years (Sutherland, 1939; Sutherland and Cressey, 1970). Many have argued that its survival value has more to do with ambiguity and untestability than with intellectual rigor and empirical support (for a review see Nettler, 1978). Others have evaluated DA relative to its competitors and on this basis its survival appears to be that of the fittest (Akers, 1973; Cressey, 1960; Glaser, 1962). More recently, a number of cross-sectional and predictive validity studies have confirmed Glaser's (1962) contention that thoughtful attention to association with both criminal and anticriminal patterns would be rewarded by impressive gains in the predictability of criminal indices (Akers et al., 1979; Andrews et al., 1979; Buikhuisen and Hoekstra, 1974; Empey and Lubeck, 1971; Glaser, 1974; Harris, 1975; Linden and Hackler, 1973). However, the increasingly positive picture clouds when one looks for specific tests of causal and practical significance within an experimental (as opposed to descriptive) framework.

The promise of evidence regarding causal significance was inherent in systematic, empirical explorations of a "clinical sociology" (Cressey, 1955; Empey and Rabow, 1961), specifically, evaluations of guided group interaction (Empey and Erickson, 1972) and related programs (Hackler and Hagan, 1975) for delinquent or high-risk youths. The data are now in from a number of such experiments and the evidence is overwhelmingly clear. The massive and intrusive group and related programs were either no more effective or less effective than comparison programs such as routine individual probation supervision (Stephenson and Scarpitti, 1974). More disturbing than the negative results has been the utter failure of the evaluations to provide even one explicit and direct examination of the DA-based assumptions on which the programs were designed. Cressey (1955) stated that the focus of the programs should be the attitudes, values, and beliefs of groups rather than of individuals; and he adapted Cartwright's (1951) group dynamics principles to the correctional scene. For over 20 years, with the possible exception of Klein (1971), there was not one controlled test of the importance of group cohesion nor of the competing structural means of inducing an anticriminal focus within groups (Andrews, 1979a; 1979b).

The paucity of theoretical, empirical, and practical gains can be traced to two related factors. Just as Schwartz (1961) warned in this journal in his response to Empey and Rabow's (1961) guidelines for guided group interaction, the clinical sociology tradition has been characterized by an inattention to, or lack of appreciation of, the efforts of other disciplines. The lack of cross-discipline exchange is clearly evident, even today, when one examines how many respected sociologists have reacted to the negative outcome of the grand experiments. Schur (1973:170) advises that as long as treatment must be offered, then "available evidence favors emphasizing relatively unstructured group sessions." Hackler (1978) advises a deemphasis of controlled evaluation and more careful attention to basic principles governing human behavior. What principles?—"cognitive dissonance" and "group interaction." Stephenson and Scarpitti (1974) conclude (somewhat lamely) that some incidental learning has occurred regarding program implementation and that, hopefully, more will be learned in the future. The conclusion is inescapable that those who have worked in the symbolic interaction and group dynamics traditions have been unaware of, or uninterested in, the behavioral revolution that has occurred over the last 15 years in the delivery and evaluation of social services. The self-critical yet self-correcting behavioral literature includes an increasing number of controlled and positive evaluations of correctional programs (for reviews, see Andrews, 1979a; 1979b; Gendreau and Ross, 1978). The problem is not unlike that documented by Hirschi and Hindelang (1977) with reference to the inability of sociology to incorporate the well-documented relationship between IQ and delinquency.

A second and related reason for the failure of clinical sociology to advance significantly either understanding or service has to do with the style of the research. The evaluators compared structurally complex programs in terms of ultimate impact on recidivism without systematically documenting the effects of program structure on the actual process or contingencies of treatment, without documenting the relationship between process and the intermediate attitudinal gains thought to be mediating behavior change, and without documenting the extent to which the attitudinal gains (or losses) were associated with recidivism. The tenets of symbolic interactionism and group dynamics theory were accepted without challenge, as was the programmer's ability to create the appropriate conditions through structural means. Andrews and Kiessling (1979) have noted elsewhere the confusion possible when the links among structure, process, intermediate gain, and ultimate outcome are not specified and monitored.

Given this background of impressive heuristic and predictive value—yet an inadequate documentation of causal and practical significance—the most promising recent development has been the extension of DA's power and applicability through links with behavior theory (Adams, 1973; Akers, 1973; Andrews et al., 1974; Andrews et al., 1979; Burgess and Akers, 1966; and Howell's, 1972, use of Homans, 1961). Through behavioral reformulations, DA is freed from the descriptive and antiexperimental tradition of classical symbolic interactionism. The collections of Cressey and Ward (1969) and Rubington and Weinberg (1973) are probably most representative of that tradition in the study of deviance and, in the well-over-100 papers sampled, there is only one example of a controlled experimental study. The behavioral orientation is strongly experimental, yet not insensitive to the potential of descriptive investigations or to other methodologies (Bijou et al., 1968). It is much more flexible and open to cognitive factors (Bandura, 1969; Meichenbaum, 1977) than its most derisive and ideologically committed critics in the area of deviance (Taylor et al., 1973) would have us believe. It promises, indeed demands, the systematic and empirical examination of DA's most basic tenets without necessarily severing ties to models—both of man and of behavioral influence—which emphasize reason and choice as well as interpersonal concerns.

The purest (and yet the weakest) of the operant reformulations of DA is that of Adams (1973). He systematically purged DA of any "mentalistic concepts," deemphasized the social nature of the learning and performance processes, and, in short, produced what Nettler (1978) would call a "true but trivial" statement. Statements to the effect that the acquisition, maintenance, and modification of criminal and noncriminal behavior are governed by similar principles do not constitute a theory of criminal behavior. They are statements concerning a general theory of behavior. Similarly, empirical documentations of the fact that the criminal and noncriminal behaviors of offenders and nonoffenders are under antecedent and outcome control do not constitute convincing support for a behavioral version of DA. Such demonstrations expand the literature on the applicability of behavioral principles and techniques to corrections, but they do not meaningfully advance our understanding of the causal significance of DA principles.

Experimental evidence relevant to DA, regardless of the theory and technology of behavioral influence associated with it, should involve tests of one or more of the key principles. The following principles are the ones investigated in the series of studies to be reviewed:

(a) Criminal learning occurs by way of association with criminal patterns and relative isolation from anticriminal patterns. In behavioral terms, this is the contingency factor and refers to the criminal versus anticriminal nature of the patterns to which the individual is exposed (modeling) and the consequences for the individual (reinforcement and punishment) of exhibiting criminal and anticriminal patterns (Andrews et al., 1974).

(b) The principal part of learning occurs within intimate personal groups. This is the quality of interpersonal communication factor or the socioemotional/relationship factor. In behavioral terms, the most effective model is one who, among other things, controls a wide variety of high-quality reinforcers (or punishers) and who rewards the other for exhibiting demonstrated behaviors (Bandura, 1969). We will assume that the dimensions on which "intimate personal groups" are defined are the same dimensions on which variations in the reinforcing and punishing value of another's response may be classified (Homans, 1961). For example, an interpersonal situation characterized by mutual liking and respect, and by understanding and open communication is an "intimate" one, one within which the behaviors of the other are attended to (with the potential of enhanced modeling effects), and one in which the other is a potential source of powerful reinforcers and punishers.

(c) A person engages in criminal acts ("becomes criminal") when there is an excess of definitions of the situation favorable to violation of the law over definitions of the situation unfavorable to violation of the law. In behavioral terms, this is the self-management factor, a subset of the more general factor of discriminative stimulus control over criminal acts. There may well be situations or individuals wherein, or for whom, the objective properties of the immediate situation discount any set of motives, rationalizations, or beliefs which the individual brings with him (Wells, 1978), but that is not the concern of the studies reviewed. We assume that the products of the learning which has occurred by way of the contingency and relationship factors—products in the measurable form of attitudes, values, and beliefs—may (in some situations and under some conditions) be causally associated with criminal performance. Generally, there will be a greater correspondence between cognitions and behavior among individuals who engage in self-monitoring, self-instructing, and self-consequating than among individuals who less systematically practice such measurable and trainable self-management skills (Bandura, 1969; Meichenbaum, 1977).[1]

In summary, the contingency and relationship principles have to do with the conditions under which criminal learning occurs, while the self-management principle has to do with the conditions under which criminal learning is translated into criminal acts. The causal implications of these principles may be inferred from controlled studies in which (a) deliberately induced variations in the learning situation are monitored for effects on criminal learning and, (b) deliberately induced shifts in the balance of favorable and unfavorable definitions are monitored in relation to criminal activity. Of particular interest to the general sociologist is the fact that the variations on the contingency and relationship dimensions may be induced through structural interventions within those miniature social systems which constitute the specific settings for intervention. Mayer (1972) has provided a compelling analytic base for the investigation of structural approaches to social problems, but, reflecting the state of the art and the science, he did not present one experimentally derived example. In the studies reviewed here, the dominant interventions were those of changing the membership composition of the treatment system and/or changing the role composition of the systems.

The value of any empirical study depends upon how well the factors of theoretical interest—as well as competing factors—have been brought under experimental control. Since it is naive to expect that any single study could discount all competing factors or could introduce simultaneous controlled variation on all factors of theoretical interest, integrated research programs—rather than isolated studies—are indicated. Each study, within the set to be reviewed, made a systematic attempt to control for competing factors not well attended to by other studies in the set and/or to induce systematic variation in relevant factors which had been held constant in the other studies. In addition to such general problems of internal validity, there are also problems of external validity which can be dealt with only through systematic replication and extension to different types of subjects, settings, manipulations, and measures. Explicit attempts were made to sample from different settings and subject groups, but there are three obvious limits on generalizability: all of the studies involved official offenders who were serving formal sentences at the time of the studies; every study made use of the same attitude battery—a positive feature in terms of comparability within the set, but a negative feature in terms of external generalizability; and all studies were completed by the same research team—or at least by a professionally related group of researchers. Regarding the second limitation, we find that some of the studies have supplemented the attitudinal measures with recidivism data.

---

[1] The designation of differential association or DA is used throughout this paper to refer to these principles. However, readers familiar with differential association literature will recognize that it is the behavioral reformulation that is being tested here, rather than strictly the original theory as stated by Sutherland. The behavioral reformulation as stated above adds three elements to the original: the specification of modeling and reinforcement in the "contingency factor"; the linking of "intimacy" to the schedule and quality of modeling and reinforcement; and the linking of the causal significance of "definitions" to discriminative stimulus control through self-management.

The Contingency Principle

The first set of studies involved evaluations of a coparticipant role for student and citizen volunteers in institution-based group counseling with the adult-male residents of two minimum-security prisons. Prior to these studies, several controlled evaluations of short-term structured group counseling had produced evidence that groups composed of prisoners and a nonprisoner leader were having effects on various attitudes and behaviors, including institutional adjustment (Andrews and Young, 1974), interpersonal skills and self-esteem (Daigle-Zinn and Andrews, forthcoming), and knowledge of legal rights (Wayne and Andrews, 1978). However, Wayne and Andrews (1978) were unable to obtain effects on the attitudinal measures most obviously relevant to the notion of definitions favorable versus unfavorable to violations of the law—that is, on measures having to do with respect for the law, courts, and police; with tolerance for law violations; and with identification with offenders. It appeared that the differentials in the quality of exposure to criminal and anticriminal expressions (differentials so well-documented by Buehler et al. [1966] at a girl's training school) were also present in our counseling groups. Thus, a structural change was introduced for those counseling groups which focused upon attitudes and beliefs regarding the law and law violation—specifically, a change in membership composition through the introduction of noncriminal others as codiscussants.

The Community Groups involved from four to seven citizen volunteers (primarily college students) and from four to seven prisoners, interacting for one evening a week for a period of eight weeks. Each group had a nonresident leader whose operational guidelines were to encourage open, warm, and frank communication while structuring discussions around preselected topics such as the function of rules, the validity and limits of common rationalizations for law violations, and a social learning perspective on self-control. All participants had responded to announcements at their respective institutions that Community Groups were being established. Details on the institutional settings and group procedures have been provided elsewhere (Andrews et al., 1974; Andrews and Gendreau, 1976).

The first study (Andrews et al., 1973) suggested that the Community Group model provided not only a potentially important service but also a vehicle for the systematic evaluation of the causal significance of DA. Inspection of the Client factor in Table 1 shows that the volunteers and prisoners differed, first, in the expected (and theoretically relevant) ways on the attitudinal measures of respect for the law, courts, and police; tolerance for law violations; and identification with criminal others. Secondly, participating residents showed reduced tolerance for law violation and reduced identification with offenders, at posttest, relative to a nonparticipant comparison group of residents, while the participating citizen volunteers were showing increased identification with criminal others relative to a waiting-list control set of volunteers. The triple interactions presented in Table 2 have since been replicated in detail by

Table 1. A Summary of the Analyses of Variance in Criminal Sentiments by Type of Client, Program, and Pre-Post (Study I)

		Law, Courts, Police		Tolerance for Law Violations		Identification With Criminal Others	
Source	*df*	*ms*	*F*	*ms*	*F*	*ms*	*F*
Client (A)	1	6055.2	47.47***	684.45	22.66***	201.61	14.27***
Program (B)	1	8.45	<1.0	1.80	<1.0	10.61	<1.0
A × B	1	33.80	<1.0	20.00	<1.0	13.61	1.30
Error (b)	36	127.57		30.20		10.46	
Pre-Post (C)	1	12.80	<1.0	33.80	4.68*	3.61	1.67
A × C	1	8.45	<1.0	1.80	<1.0	0.11	<1.0
B × C	1	24.20	<1.0	0.45	<1.0	0.31	<1.0
A × B × C	1	18.05	<1.0	31.25	4.33*	52.81	24.49***
Error (w)	36	25.85		7.21		2.16	

* $p < .05$.
** $p < .01$.
*** $p < .001$.
Note: The Program factor in Study 1 was Community Group versus Routine Institution Treatment (N=40:10 subjects for each Client-Program combination).

Wormith (1977). However, our first study did not directly vary exposure to anticriminal patterns—a more crucial element of Sutherland's theory than exposure to noncriminal others—and, by virtue of the use of nonparticipant controls, the changes may simply have reflected an attention-placebo or demand effect.

The second study (Andrews et al., 1977) was an explicit attempt to hold attention and association with noncriminal others constant while varying the opportunity for the exposure of anticriminal patterns. Volunteers and prisoners were randomly assigned to either a Community Discussion Group (codiscussant roles) or a Community Recreation Group (companion roles). In the latter, the participants engaged in various table-top games with no programmed opportunity to expose their sentiments regarding the law. There was a built-in replication with residents drawn from the two physically separate institutions which were located on the same grounds, one for first incarcerates and the other for recidivists.

Table 3 provides a summary of the analyses of variance in attitude change (postscores minus prescores). Within both institutional samples, any statistically reliable attitudinal gains on the indices of definitions unfavorable versus favorable

Table 2. Mean Criminal Sentiments at Pre- and Posttreatment By Type of Client and Program Participation: A Summary of the Pre-Post Effects in Study I

	Pre- *(n=10)*	Post- *(n=10)*	*F*	*p*
	Identification with Criminal Others			
Program Participants				
Prisoners	20.8	18.7	10.22	.005
Volunteers	16.9	17.9	2.32	.20
Nonparticipant Controls				
Prisoners	19.7	21.1	4.54	.05
Volunteers	17.4	15.4	9.27	.005
	Tolerance for Law Violations			
Program Participants				
Prisoners	31.8	32.0	<1.0	*ns*
Volunteers	24.0	26.1	3.06	.10
Nonparticipant Controls				
Prisoners	29.1	32.1	6.24	.025
Volunteers	25.8	25.7	<1.0	*ns*

Table 3. A Summary of Analyses of Variance in Changes in Criminal Sentiments By Type of Client, Program, and Institution (Study II)

	Law, Courts, Police			Tolerance for Law Violations	
	df	*ms*	*F*	*ms*	*F*
Client (A)	1	153.14	1.20	217.56	6.70*
Program (B)	1	606.14	4.75*	9.00	<1.0
Institution (C)	1	31.64	<1.0	1.56	<1.0
A × B	1	40.64	<1.0	12.25	<1.0
A × C	1	415.14	3.25*	10.56	<1.0
B × C	1	6.89	<1.0	144.00	4.44*
A × B × C	1	26.27	<1.0	40.06	1.23
Error (b)	55	127.54		32.45	

* $p<.05$.
Note: The Program Factor was Community Discussion Group versus Community Recreation Group. There were no significant effects on the Identification with Criminal Others scale (N=64).

Table 4. Mean Change in Criminal Sentiments of the Codiscussants and the Companions (Study II)

	Codiscussants (n=32)	Companions (n=32)	F
Law, Courts, Police	6.09	−0.06	4.75*
Tolerance for Law Violations[a]	−3.44	0.31	6.97*

* $p<.05$.

[a] Effect reliable only within the institution for first incarcerates.

to law violations were found following participation in the discussion groups but not found following participation in the recreation groups (Table 4). In fact, the changes found among prisoners in the recreation condition were no different from those found within a waiting-list control group of prisoners. Overall, the findings discounted an effect due to attention or to simple exposure to noncriminal others but, given the focused nature of the discussion condition, there was still the possibility of a Testing × Treatment interaction.

The Relationship Principle

The third study (Andrews et al., n.d.[b.]) was an experimental investigation of the socioemotional factor as well as an attempt to discount the possibility of a Testing × Treatment interaction. All residents participated in focused discussion groups, but the specific groups to which they were randomly assigned varied in terms of the interpersonal skills of participating citizens. Goodman's (1972) behavioral assessment technique was employed to obtain preassignment measures on the volunteer's openness, warmth, and understanding as rated by peers. Two types of Community Groups were formed, one including citizens who were above the median on peer ratings and the other including citizens who received below average peer ratings.

Table 5 provides a sample of the findings from the third study. The membership composition manipulation was effective in influencing the socioemotional climate in that the more-skilled volunteers and the prisoners in their groups reported

Table 5. The Effects of the Interpersonal Skills Level of Volunteers on the Socioemotional Climate and Interaction Process Within Groups and on Changes in the Criminal Sentiments of Prisoners (Study III)

	Groups With the Less Skilled Volunteers. (Prisoners $n=18$; Volunteer $n=20$)		Groups With the More Skilled Volunteers. (Prisoner $n = 18$; Volunteer $n = 21$)			
	Mean	SD	Mean	SD	F	p
Socioemotional Climate (Participants Ratings)						
Openness	12.8	1.7	14.2	1.8	10.29	.002
Interaction Process						
Opinions (Asking for)	3.5	5.0	0.5	1.5	7.56	.008
Suggestions (Given by Volunteers)	6.6	7.4	11.6	5.8	4.67	.04
Prisoner Attitude Change						
Law	−1.3	6.3	2.1	5.8	2.75	.10
Courts	1.9	5.2	0.1	6.0	<1.0	*ns*
Police	−1.5	4.6	1.8	4.2	4.61	.04
				MANOVA	5.00	.006
Tolerance for Law Violations a	2.9	5.7	−4.7	8.1	5.72	.03
b	−2.4	2.3	1.0	4.5	4.22	.05

Note: There was a statistically reliable Interpersonal Skill × Prisoner Anxiety interaction on the Tolerance change scores, F (1/32) = 8.81, $p<.006$. Similar interactions on the Law and Identification scales failed to reach reliable levels. The "a" values are for low anxiety prisoners; the "b" values, for high anxiety prisoners.

more open and emotionally frank communication than did the less-skilled volunteers and their prisoner coparticipants. The two types of groups did not differ on the Bales's (1950) interaction process measure of expressed opinions but, as indicated in Table 5, apparently the volunteers and residents in the less-skilled groups had to make more direct requests for the expression of personal opinions. The skilled volunteers were more likely to take a leadership role in the discussions by offering suggestions than were the less-skilled volunteers. Note that successful manipulation of the relationship factor was completed without concomitant variation of the contingency factor, in that peer ratings on interpersonal skills were statistically independent of the volunteers' pretested scores on the criminal orientation measures.

The outcome supported the relationship principle—particularly among those prisoners who were below the resident average on a pretested trait measure of anxiety (Table 5). Generally, the posttreatment changes on the attitudinal measures of a criminal orientation were most strongly in the anticriminal direction among those prisoners who had interacted with the more interpersonally skilled volunteers. However, it appeared that the confrontation format may have been dysfunctional with the more anxious residents, a finding anticipated by other group programs (Sarason and Ganzer, 1973).

The third study provided an additional opportunity to examine the relationship principle, an opportunity which was not reported in Andrews et al. (n.d.[b.]). At the end of the next-to-last session of the groups, individual volunteers and prisoners were asked to report on the extent to which there had been obvious divisions between volunteers and prisoners during the sessions. A group-dynamics perspective (Cressey, 1955) and social learning theory yield the prediction that the *amount* of attitude change would be a positive function of cohesion. However, the more interesting question is whether the *direction* of the relationship between cohesion and change depends upon the type of client. The first study (Tables 1 and 2), in conjunction with the contingency principle, had suggested that volunteers and prisoners tend to become more alike in their criminal sentiments as a function of exposure to each other. According to the relationship principle, the prisoners should become more anticriminal in their attitudes, under association conditions of high perceived cohesion, while the citizen volunteers should move toward less anticriminal positions. An examination of the correlations between perceived cohesion and attitude change suggested that just such an interaction was present. The correlation of cohesion with changes in the tolerance for violations measure was $-.35$ ($p<.05$) within the prison sample but .26 ($p<.10$) within the volunteer sample. For changes in identification with offenders, the correlations were $-.33$ ($p<.10$) and .34 ($p<.05$) for the prisoners and volunteers respectively.

The Interaction of the Contingency and Relationship Principles

The studies had shown that anticriminal learning on the part of prisoners was a function of programmed differentials in the exposure of criminal and anticriminal patterns and that anticriminal learning was most evident under program conditions designed to promote positive interpersonal interactions. What was required was an examination of impact when the contingency factor and the socioemotional factor varied in combination. Specifically, DA predicts that there is a positive or negative relationship between the socioemotional factor and criminal learning, depending upon the relative exposure of criminal and anticriminal patterns.

The fourth study (Andrews et al., forthcoming) was conducted in two medium-security penitentiaries and involved adult recidivists serving sentences of over two years. The prisoners were randomly assigned to two types of groups: one, of the Community Group format; and the other, a discussion group focusing on the same topics but without citizen volunteers as coparticipants. Table 6 presents a summary of the findings. Important to the research program—but not unexpected—was the finding that the volunteers, known to be more anticriminal in

Table 6. The Effects of Volunteer Participation on the Socioemotional Climate and the Anticriminal Contingencies Established Within Groups, on Changes in the Criminal Sentiments of Prisoners, and on the Correlations between Socioemotional Climate Ratings and Changes (Study IV)

	Community Groups				Resident Groups				*F*
Socioemotional Climate									
(Prisoners' Ratings)									
Openness (Institution A)	13.50				10.43				6.35*
Openness (Institution B)	11.75				16.75				17.98**
Procriminal Arguments	1.63 (Volunteers)				2.63 (Residents)				34.91**
Prisoner Attitude Change									
Law, Courts, Police (Institution B)	.25				−9.75				7.98*
	Partials				Partials				
Openness Ratings in relation to Attitude Change	*r*	r^a	r^b	r^c	*r*	r^a	r^b	r^c	*Z*
Law, Courts, Police	.89**	83	86	85	−62	−56	−47	−29	3.24***
Tolerance for Law Violations	−75**	−61	−79	−88	34	30	28	48	1.94*

* $p<.05$.
** $p<.01$.
*** $p<.001$.
[a] Controlling for Institution.
[b] for Prescores.
[c] for "faking good."
Note: See text for discussion of interactions (Prisoner N=16; Volunteer N=16).

their pretested attitudes, did in fact express more anticriminal sentiments within the groups than did the prisoners. During selected sessions participants were asked to provide arguments favorable versus unfavorable to specific violations, such as illegal parking, burglary, and armed robbery. Content analyses of typed transcripts prepared from videotaped portions of these sessions confirmed that the arguments of the prisoners were more favorable to each of the three types of violations than were those of the volunteers. Note that the presence of the volunteers had no detectable effect on the expressed arguments of the prisoners during the groups' meetings, but it was the case that anticriminal sentiments had a greater exposure within the Community Groups than within the Resident Groups.

The effects of volunteer presence on the socioemotional climate and on prisoner attitude change were less clear. Within one of the institutions, the presence of volunteers was associated with more open and frank discussion, according to the ratings by prisoners; the effect was exactly opposite within the other institution. Similarly, the effects on prisoner attitude change were less than impressive. In only one of the institutions, and on only one of the criminal sentiment scales, was there a statistically reliable effect of volunteer presence. However, the impact of the two types of groups on the relationship between socioemotional indices and attitude change was more crucial to DA theory than was the impact of the groups on either, separately. Inspection of the correlations in the bottom portion of Table 6 reveals that interaction predicted by DA was evident. Within the resident-only discussion groups, the groups in which the criminal expressions had been less well-balanced by anticriminal expressions, positive prisoner evaluations of the socioemotional evaluations were associated with procriminal changes on the attitude measures. Within the Community Groups, positive socioemotional evaluations were associated with anticriminal changes. The reversal of effects, dependent upon type of group, was evident within both institutions and remained when statistical controls were introduced for prescores and any generalized tendency to "fake good."

Given the consistency of the above set of findings, from study-to-study and with reference to DA, it was time to manipulate experimentally both the relationship and the contingency factors and to monitor effects on behavioral indices. In conjunction with an attempt to produce a broad comparison of the volunteer and professional supervision of adult probationers (Andrews et al., 1979), the volunteers (n = 60) and professionals (n = 14) were pretested on a number of self-report attitude and personality scales, including three which were relevant to the contingency and relationship factors implicated by DA: the Hogan Empathy (EMP) scale which has excellent psychometric credentials as a measure of interpersonal skills (Grief and Hogan, 1973); the Gough (1969) Socialization (SOC) scale which is one of the more reliable and valid personality predictors of criminal activity and, hence, a solid measure of a conventional orientation; and the measure of Identification with Criminal Others (ICO) which had been successfully employed in the Community Group studies and whose positive correlation with the Hogan Empathy Scale had been discovered with volunteers in still another study (Andrews et al., n.d.[a.]). The prediction was now obvious: the most effective probation officers would be those who were interpersonally skilled (above the median on EMP), conventional in orientation (above the median on SOC), and not too enamored of criminal others (below the median on ICO).

Probationers (n = 190) were randomly assigned to either a volunteer or professional officer, and to officers within each category without reference to the officers' scores on EMP, SOC or ICO. The managers of the probation program, the officers and their clients, and the research staff were "blind" with reference to the personality scores of the officers prior to case assignment, as well as during the supervision and data collection periods.

The first question was to determine whether the membership composition manipulation of the contingency and relationship factors actually had measurable effects on probation supervision. The answer was an unqualified yes (Table 7). Program managers, including the screening interviewers and both the preservice and inservice trainers of volunteers, gave their most positive evaluations to those volunteers who were the more interpersonally skilled. The probationers assigned to the more interpersonally skilled officers—volunteer or professional—reported more open and warm relationships with their officers than did the probationers assigned to the less interpersonally skilled officers. Probationers' reports on the amount of real help and assistance offered by their officers varied positively with officer SOC scores. Finally, during audiotaped supervision sessions, it was the more conventionally socialized officers who were making more anticriminal statements—and who were most likely to approve of their probationer's anticriminal expressions and disapprove of their probationer's procriminal expressions. The more conventionally socialized officers were also less likely to offer noncontingent friendly expressions.

The in-program recidivism of the probationers was analyzed in a 2 (Officer EMP) × 2 (Officer SOC) × 2 (Officer ICO) format, with the professional status of officers and the probationers' age and sex as covariates. The main effects of the officer factors were not statistically reliable but there was an EMP × SOC interaction ($p<.004$). In a different setting, with different operational definitions of the contingency and relationship factors, and

Table 7. The Effects of Officer Empathy and Socialization on the Socioemotional Climate and Anticriminal Contingencies During Probation Supervision: Pearson *r*'s (Study V)

	Empathy	Socialization
Managers' Ratings of Volunteer Officers (N=60)		
Screening Ratings	.37**	.17
Training Ratings	.33**	.11
Quality of Supervision (Officers' Ratings, N=49)		
Relationship (Socioemotional)	.27*	.14
Helping	.19	.33**
Quality of Supervision (Probationers' Ratings, N=46)		
Relationship (Socioemotional)	.29*	.13
Helping	.14	.32**
Officer Behavior During Audiotaped Sessions with Probationer (N=48)		
Anticriminal Modeling	(*ns*)	.36*
Anticriminal Reinforcement	(*ns*)	.45**
Friendly Expressions	(*ns*)	−.40**

* $p<.05$.
** $p<.01$.

Table 8. Proportion of Probationers "Recidivating" by Probation Officer Empathy and Socialization Levels (Study V)

	Socialization		
Empathy	Low	High	$p<$
Low	.163 (37)	.418 (31)	.02
High	.298 (48)	.148 (72)	.05
$p<$	.10	.01	

Note: The recidivism rates were adjusted for probationer age, sex and professional status of officer. The unadjusted rates yield the same pattern of results. The number of probationers on which the proportions were based are recorded in parentheses.

with a different measure of anticriminal influence, the interaction plotted in Table 8 was a direct confirmation of the Andrews et al. (forthcoming) findings, previously outlined in Table 6. With anticriminal contingencies in effect (high SOC officers), the relationship factor (officer EMP) was negatively related to recidivism: the probationers assigned to the more empathetic officers "recidivated" at lower rates than did probationers with the less empathetic officers. With the less anticriminal contingencies in effect (low SOC officers), officer EMP scores were positively related to probationer recidivism. The particularly poor performance of the probationers with the low-EMP/high-SOC officers (the austere moralistic officers in Grief and Hogan's [1973] terms) represents the first convincing empirical confirmation of Cressey's (1955) and Empey and Rabow's (1961) fear of establishing "rejection of the rejectors."

A close inspection of Table 8 might suggest that both the low-EMP/low-SOC and high-EMP/high-SOC combinations represented relatively neutral conditions, while the other two conditions were associated with the active production of criminal behavior. Note that there was not a significant difference in outcome between the low-EMP/low-SOC and high-EMP/high-SOC conditions. We expected, however, that the recidivism rates for the high-EMP/high-SOC officers were inflated by the fact that EMP and ICO are positively correlated: the more empathetic officers identified most with offenders ($r = .23, p<.05$). Thus, and in view of the a priori significance of ICO, officers in the high-EMP/high-SOC categories were further split on the basis of ICO. Relative to the probationers assigned to the other types of officers, the probationers of the high-EMP/high-SOC/low-ICO officers "recidivated" at significantly lower rates (Table 9). However, splitting the low-EMP/low-SOC officers on ICO had no such effect on the recidivism of their probationers.

To maintain continuity with the Community Group studies and to compare behavior (recidivism) with verbalization, probationer attitude change was also monitored as a function of officers' interpersonal skills and conventional orientation. Consistent with the Community Group studies and DA, it was those probationers assigned to the high-EMP/high-SOC officers who not only "recidivated" least, but also showed both the greatest increases in respect for the law, courts, and police, and the greatest reductions in tolerance for law violation and identification with offenders. The attitudinal effects were evident regardless of probationers' age, sex, prescores, "faking good" or the professional status of their officers (Table 9).

Table 9. Mean Change in the Criminal Sentiments of Probationers and Recidivism by Officer Empathy and Socialization Levels (Study V)

	High Empathy, High Socialization Officers	Other Officers	*F*	*p*
Law, Courts, Police	4.41	1.23	4.98	.03
Tolerance for Law Violations	−2.35	−0.35	5.81	.02
Ident. with Criminal Others	−1.19	−0.09	3.51	.06
Recidivism (%)	5.37	25.2	5.19	.02

Note: The tabled mean attitude change scores have been adjusted for prescores, probationers' age and sex, changes on the "faking good" scale, and professional status of officers. See text regarding the recidivism effects.

In sum, the studies have provided strong and consistent evidence that the contingency and relationship principles of DA have causal significance with reference to both criminal attitudes and criminal behavior. The self-management factor remains to be reviewed.

The Self-Management Principle

A recurring problem for reviewers of DA has been that of defining "excess." The suggestion here is that one way we can avoid the problem of operational counts of the number of criminal and anticriminal definitions is by focusing, instead, on *changes* on measures relevant to definitions. The focus then becomes that of examining the behavioral correlates of induced *shifts* in the balance of definitions. This can readily be done, assuming we have measures of definitions at two separate points in time, the theoretical and technical wherewithall to induce the changes, and an independent measure of criminal behavior at time two and/or at a later follow-up period.

Toward this end, Andrews et al. (1979) examined the correlations between probationer attitude change and recidivism with statistical controls introduced for probationers' prescores, the professional status of the probationer's officer, and probationer age and sex. In a battery of 24 attitude and personality scales, only six measures of change were reliably predictive of reduced recidivism: more positive attitudes toward the law, courts, and police (−.20, $p<.01$); decreased tolerance for law violations (.15, $p<.05$); decreased identification with offenders (.15, $p<.05$); increased socialization (−.17, $p<.05$); decreased psychopathy (.18, $p<.01$); and decreased wish to exert control over others (.14, $p<.05$). While there may be discussion about the relevance of the latter two change-measures to DA, the first four rather clearly represent shifts in the direction of definitions unfavorable to violation of the law. The multiple correlation between attitude change and recidivism was .48 ($p<.05$), a figure of impressive magnitude, given the well-known problems associated with self-report measures and the fact that we did not have an independent measure of self-management skills.

The above-noted finding is generally consistent with DA—but not sufficiently convincing when one adopts a behavioral perspective on self-management, since the functional process has not been identified. Recall the assumption that changes at the level of attitudes and beliefs would be most evident at the behavioral level when the individual possesses and practices self-management skills. In a study which might well chart an exciting new course for symbolic interactionism, Wormith (1977) attempted to isolate experimentally the conditions under which attitude and belief change become evident in deviant versus conventional behavior. In a 2 × 2 factorial design, he independently manipulated the quality of an attitude-change program to which prisoners were exposed, as well as the programmed opportunity for enhancement of self-management skills. The attitude-change manipulation involved random assignment of prisoners to one of two types of Community Groups: one, in which the volunteers were trained to explicitly model and reinforce anticriminal expressions; and the other, a routine Community Group. The self-management program involved systematic training of prisoners in behavioral principles, including self-observation, self-instruction, and self-consequation. The comparison level of the self-management training factor was participation in a Community Recreation Group. Wormith monitored the behavioral effects of the four program conditions on two reliable institutional-conduct indices involving prosocial versus antisocial changes in participants' behavior toward staff and peers. The analyses revealed a clear Attitude Training × Self-Management Training interaction on prisoner behavior outside of the training context. Those residents who received self-management training, plus high-level attitude training, showed behavioral changes in the prosocial direction; while those who received self-management training, in combination with the lower-level attitude change program, exhibited behavioral changes in the antisocial direction. The Wormith study, like the other

Community Group studies, awaits followups regarding impact on postprogram recidivism.

Summary and Conclusions

The studies and findings have suggested and supported a number of general conclusions regarding how social and behavioral scientists may approach matters concerning the causal and applied significance of their theories:

(a) The Community Group and Ottawa Probation projects, in conjunction with the findings of other controlled evaluations of correctional programs reviewed elsewhere (Andrews, 1979a; 1979b), provide impressive evidence in total that the principles of DA have causal significance and that the principles are of immediate importance to those involved in the design and management of correctional programs. Without due and joint consideration of the contingency and relationship factors, correctional programming may inadvertently produce not simply *zero* impact—but perhaps *negative* impact—on correctional clients. Similarly, although the data-base is still very light on the point, it may be worse than unproductive to focus on criminal learning without equal attention being paid to the processes by which learning is translated into performance.

(b) The analytic and technological power as well as the conceptual flexibility that a behavioral orientation can bring to the study of deviance is too great to be glibly dismissed, as has been done by sociologists strongly committed to symbolic interactionism. Similarly, a commitment to the development of a "clinical sociology," a commitment still strongly associated with the group dynamics tradition, cannot continue to blind programmers and evaluators to the paucity of their returns, relative to complementary behavioral approaches.

(c) Systematic, controlled, and theoretically integrated research programs should precede the design and implementation of large-scale and intrusive interventions in the lives of individuals. If a few small, but careful, investigations of the assumptions underlying guided-group interaction programs had been completed prior to their wide-scale implementation, then many young persons would have been spared the massive intrusion which such programs represent, relative to routine probation. In rejecting the clinical tradition, the clinical sociologists also overlooked a key ethical principle: the explicit testing of the assumptions underlying intervention as opposed to ideological commitment to those assumptions.

(d) Glaser's (1962; 1974) increasingly impassioned pleas for a bridging of the gap between theory and practice will be more systematically adhered to as practitioners recognize the guidance which theory can provide, and as theorists recognize the potential of rigorous program evaluation as a means of testing the causal significance of theoretical principles. The studies reviewed spoke to both concerns.

(e) Program-evaluation research provides an opportunity for sociologists to examine the effects on individual behavior of controlled variations on basic structural factors such as the membership, role, and status composition of social systems. Some of the studies in the review provided concrete examples of the Durkheimian "social fact"—albeit, examples derived from social systems so miniature that Durkheim might have yawned.

(f) Program-evaluation research provides a unique opportunity for interdisciplinary (yet theoretically integrated) research on how structure maintains the contingencies within social systems, and on how those contingencies, in turn, influence the attitudes and behavior of the individuals within the system. This was a major implication of the Burgess and Akers (1966) reformulation of DA; and the studies reviewed in the present paper provide evidence relevant to both levels of analysis. Hopefully, it will be not just sociologists and psychologists who participate in such research. The emerging but still distinctly atheoretical policy and management sciences might do well to consider that it is primarily through role changes (for example, by the training of workers on practice-relevant dimensions), status changes (for example, by policy statements on rights and obligations), and changes in membership composition (for example, by selecting workers on practice-relevant dimensions) that significant organizational change may bring about more cost-efficient attainment of publicly stated goals. An ongoing study of probation services is examining the effects of simultaneously controlled variation of each of these three major elements of social structure (Andrews and Kiessling, 1978).

REFERENCES

Adams, R.
1973 "Differential association and learning principles revisited." Social Problems 20:458–70.

Akers, R. L.
1973 Deviant Behavior: A Social Learning Approach. Belmont, California: Wadsworth.

Akers, R. L., M. D. Krohn, L. Lonza-Kaduce, and M. Radosevich
1979 "Social learning and deviant behavior: a specific test of a general theory." American Sociological Review 44:635–55.

Andrews, D. A.
1979a The Friendship Model of Voluntary Action and Controlled Evaluations of Correctional Practices: Notes on Relationships with Behaviour Theory and Criminology. Toronto: Ontario Ministry of Correctional Services.
1979b The Dimensions of Correctional Counselling and of Supervisory Process in Probation and Parole. Toronto: Ontario Ministry of Correctional Services.

Andrews, D. A., G. Brown, and J. S. Wormith
1974 "The community group: a role for volunteers in group counselling within correctional institutions." Pp. 34–43 in Proceedings of the Canadian Congress of Criminology and Corrections. Ottawa: Canadian Criminology and Corrections Association.

Andrews, D. A., C. Farmer, and J. Hughes
Forthcoming "Citizen participation in structured group counselling with incarcerated adult recidivists." Crime and/et Justice.
n.d.(a.) 'High and low empathy volunteers in structured group counselling with incarcerated adult recidivists." Unpublished manuscript.

Andrews, D. A., and P. Gendreau
1976 "Undergraduate training and correctional service." Professional Psychology 7:21–30.

Andrews, D. A., and J. J. Kiessling
1978 "The selection and training of citizen volunteers for intensive versus nonintensive probation supervision." A study in progress.
1979 An Introduction to Reports in the CaVIC Series and a Summary of Selected CaVIC Contributions and Findings. Toronto: Ontario Ministry of Correctional Services.

Andrews, D. A., J. J. Kiessling, R. J. Russell, and B. A. Grant
1979 Volunteers and the One-to-One Supervision of Adult Probationers: An Experimental Comparison with Professionals and a Field Description of Process and Outcome. Toronto: Ontario Ministry of Correctional Services.

Andrews, D. A., J. S. Wormith, W. J. Daigle-Zinn, D. J. Kennedy, and S. Nelson
n.d.(b.) "High and low functioning volunteers in group counselling with anxious and nonanxious incarcerated offenders." Unpublished manuscript.

Andrews, D. A., J. S. Wormith, D. J. Kennedy, and W. J. Daigle-Zinn
1977 "The attitudinal effects of structured discussions and recreational association between young criminal offenders and undergraduate volunteers." Journal of Community Psychology 5:63–71.

Andrews, D. A., and J. G. Young
1974 "Short-term structured group counselling and prison adjustment." Canadian Journal of Criminology and Corrections 16:5–13.

Andrews, D. A., J. G. Young, J. S. Wormith, C. S. Searle, and M. Kouri
1973 "The attitudinal effects of group discussions between young criminal offenders and community volunteers." Journal of Community Psychology 1:417–22.

Bales, R. F.
1950 Interaction Process Analysis. Cambridge, Mass.: Addison-Wesley.

Bandura, A.
1969 Principles of Behavior Modification. New York: Holt, Rinehart, and Winston.

Bijou, S. W., R. F. Peterson, and M. H. Ault
1968 "A method to integrate descriptive and experimental field studies at the level of data and empirical concepts." Journal of Applied Behavior Analysis 1:175–91.

Beuhler, R. E., G. R. Patterson, and J. M. Furniss
1966 "The reinforcement of behavior in institutional settings." Behavior Research and Therapy 4:157–67.

Buikhuisen, R. L., and H. A. Hoekstra
1974 "Factors related to recidivism." British Journal of Criminology 14:63–9.

Burgess, R. L., and R. L. Akers
1966 "A differential association-reinforcement theory of criminal behavior." Social Problems 14:128–47.

Cartwright, D.
1951 "Achieving change in people: some applications of group dynamics theory." Human Relations 4:381–92.

Cressey, D. R.
1955 "Changing criminals: the application of the theory of differential association." American Journal of Sociology 61:116–20.
1960 "Epidemiology and individual conduct: a case from criminology." Pacific Sociological Review 3:47–58.

Cressey, D. R., and D. A. Ward
1969 Delinquency, Crime and Social Process. New York: Harper.

Daigle-Zinn, W. J., and D. A. Andrews
Forthcoming "Role-playing versus didactic discussion in short-term interpersonal skill training with young incarcerated offenders." Canadian Journal of Criminology.

Empey, L. T., and M. L. Erickson
1972 The Provo Experiment: Evaluating Community Control of Delinquency. Lexington, Mass.: Lexington Books.

Empey, L. T., and S. G. Lubeck
1971 The Silverlake Experiment: Testing Delinquency Theory and Community Intervention. Chicago: Aldine-Atherton.

Empey, L. T. and J. Rabow
1961 "The Provo experiment in delinquency rehabilitation." American Sociological Review 26:679–95.

Gendreau, P., and Bob Ross
1978 Effective Correctional Treatment: Bibliotherapy for Cynics. Toronto: Ontario Ministry of Correctional Services.

Glaser, D.
1962 "The differential association theory of crime." Pp. 425–42 in A. R. Rose (ed.), Human Behavior and Social Processes. Boston: Houghton-Mifflin.
1974 "Remedies for the key deficiency in criminal justice evaluation research." Journal of Research in Crime and Delinquency 10:144–54.

Goodman, G.
1972 Companionship Therapy. San Francisco: Jossey-Bass.

Gough, H. G.
1969 Manual for the CPI. Palo Alto: Consulting Psychologist.

Grief, E. B., and R. Hogan
1973 "The theory and measurement of empathy." Journal of Counseling Psychology 20:280–4.

Hackler, J. C.
1978 The Prevention of Youthful Crime: The Great Stumble Forward. Toronto: Methuen.

Hackler, J. C., and J. L. Hagan
1975 "Work and teaching machines as delinquency prevention tools: a four-year follow-up." Social Services Review 49:92–106.

Harris, A. R.
1975 "Imprisonment and the expected value of criminal choice: a specification and test of aspects of the labelling perspective." American Sociological Review 40:71–87.

Hirschi, T., and M. J. Hindelang
1977 "Intelligence and delinquency: a revisionist review." American Sociological Review 42:571–87.

Homans, G. C.
1961 Social Behavior: Its Elementary Forms. New York: Harcott.

Howell, J.
1972 "A comparison of probation officers and volunteers." Ph.D. dissertation, Department of Sociology, University of Colorado. University Microfilms, No. 72–25, 179.

Klein, M. W.
1971 Street Gangs and Street Workers. Englewood Cliffs: Prentice-Hall.

Linden, E., and J. C. Hackler
1973 "Affective ties and delinquency." Pacific Sociological Review 16:27–46.

Mayer, R. R.
1972 Social Planning and Social Change. Englewood Cliffs: Prentice-Hall.

Meichenbaum, D.
1977 Cognitive Behavior Modification. New York: Plenum.

Nettler, G.
1978 Explaining Crime. New York: McGraw-Hill.

Rubington, E., and M. S. Weinberg (eds.)
1973 Deviance: The Interactionist Perspective (2nd ed.). New York: Macmillan.

Sarason, I. G., and V. J. Ganzer
1973 "Modeling and group discussions in the rehabilitation of juvenile delinquents." Journal of Counseling Psychology 20:442–9.

Schur, E. M.
1973 Radical Nonintervention: Rethinking the Delinquency Problem. Englewood Cliffs: Prentice-Hall.

Schwartz, E. P.
1961 "Discussion." American Sociological Review 26:695–6.

Stephenson, R. M., and F. R. Scarpitti
1974 Group Interaction as Therapy: The Use of the Small Group in Corrections. Westport, Conn.: Green Wood Press.

Sutherland, E. H.
1939 Principles of Criminology. Philadelphia: Lippincott.

Sutherland, E. H., and D. R. Cressey
1970 Principles of Criminology (3rd ed.). New York: Lippincott.

Taylor, I., P. Walton, and J. Young
1973 The New Criminology: For a Social Theory of Deviance. London: Routledge and Kegan Paul.

Wayne, D., and D. A. Andrews
1978 "Short-term structured group counselling and knowledge and attitudes toward the law." Unpublished manuscript.

Wells, L. E.
1978 "Theories of deviance and the self-concept." Social Psychology 41:189–204.

Wormith, J. S.
1977 "Converting prosocial attitude change to behaviour change through self-management training." Ph.D. dissertation, University of Ottawa.

SOCIAL CLASS AND PSYCHOLOGICAL DISTRESS*

RONALD C. KESSLER
University of Michigan

PAUL D. CLEARY
Rutgers University

American Sociological Review 1980, Vol. 45 (June:463–478)

Although a negative relationship between socioeconomic position and psychological distress has consistently been documented in community surveys, we know very little about the determinants of this relationship. The dominant interpretation argues that distress is caused by exposure to stressful life experiences, that lower status people are highly exposed to this sort of experience, and that statistical adjustment for differential exposure can account for the higher rates of distress among lower status people. In this paper, a different interpretation is emphasized. It is shown that differential exposure to stress, while clearly of some importance, accounts for only a minor part of the status/distress relationship, and that a far more central role is played by class differences in responsiveness to stress—that is, by the fact that lower status people are more likely than middle and upper status people to develop symptoms of distress when exposed to problematic life experience. Several plausible interpretations of this differential responsiveness are presented and an analysis strategy developed to evaluate their relative contributions. Results of an empirical evaluation document the importance of social origins and mobility experiences as determinants of differential responsiveness to stress.

In this paper, we consider the relationship between socioeconomic status and mild forms of psychological distress[1] as measured by symptom-screening scales in community surveys of mental illness. The comparatively higher rates of distress in the lowest social strata in these surveys is one of the most consistently documented findings in the literature on psychiatric epidemiology (Dohrenwend, 1970; 1973; Dohrenwend and Dohrenwend, 1969; Fried, 1975; Langner and Michael, 1963; Myers et al., 1974). Yet, despite the fact that a great deal of empirical and theoretical work has focused on this relationship, we know very little about the factors accounting for the higher rates of distress among lower class people (Liem and Liem, 1978; Wheaton, 1978).

Over the last decade, research on this problem has focused almost exclusively on the hypothesis that greater exposure to stressful life experiences accounts for the higher rates of distress in the lower classes (Dohrenwend, 1970; Myers et al., 1972; 1974). This hypothesis is based on evidence that emotional distress can result from exposure to stressful experiences of either a traumatic nature (Dohrenwend, 1975; Myers et al., 1972) or a more stable and enduring nature (Pearlin and Johnson, 1977; Pearlin and Lieberman, 1978), and

---

* Direct all communications to: Ronald C. Kessler; Department of Sociology; University of Michigan; 3012 LSA Building; Ann Arbor, MI 48109.

This research was supported by Grants 1 RO3 MH32490-01, 5 T32 MH14641-02, and 1 R03 MH33927-01 from the National Institute of Mental Health. We thank Jerome K. Myers and his associates for generously allowing us to use their data in this paper. Their research was supported by PHS Contract No. 43-67-743 and Research Grant No. MH15522 from the National Institute of Mental Health. We thank Jay Goldstein for computer assistance, and William Eaton, Charles N. Halaby, David Mechanic, Gillian Stevens, Blair Wheaton, and two anonymous reviewers for helpful comments on earlier drafts.

[1] The terms "distress" and "emotional disorder" are used throughout this paper because they are terms used in the literature and are familiar and meaningful to most readers. We would agree with Clausen (1979:97) however, who asserts that "... what we call 'mental disorder' consists of a variety of behaviors, disordered thought processes, and abnormal emotional states: some reflect the psychological influence of organic disease or genetic constitution; some are reactions to intense psychological stress; some are inadequate or maladaptive personal strategies; and some are deviant modes of behaving, thinking, and feeling that are common to members of particular subcultures." While a full discussion of these issues is beyond the scope of this paper, we would like to make it quite clear that many of the phenomena which are commonly referred to as mental illness are not necessarily, in our opinion, "illness" and that the measure of psychological disorder which will be described later in this paper does not necessarily reflect psychiatric "illness." Similarly, although some terms, such as "vulnerability," imply inadequate coping or defense, it is not necessarily the case that the experience of distress in response to environmental events is a dysfunctional response.

on the plausible assumption that lower class people are more highly exposed to experiences of both sort.

Beginning with the pioneering work of Langner and Michael (1963), efforts to support empirically this hypothesis have failed. It is true that most research on stressful life experiences finds lower class people exposed to more of the life problems that have been shown to precipitate emotional distress (Dohrenwend and Dohrenwend, 1970; Myers et al., 1974). However, statistical adjustments for these differences only partly account for the higher rates of emotional distress among lower class people. Lower class respondents are more likely than people of higher class positions to exhibit symptoms of distress at any given level of exposure to stressful experiences (Langner and Michael, 1963; Kessler, 1979; Brown et al., 1975). Furthermore, the higher the level of stress, the greater the class differences in distress. The implication of these findings is that class differences in emotional distress are partly due to class differences in the influence of stress on psychological functioning. Lower class people are, for some reason(s), more responsive to stress than people of higher class positions.

For a decade after being identified in the work of Langner and Michael (1963), the observed class difference in responsiveness to stress was virtually ignored in the psychiatric epidemiological literature. Only recently have several investigators made attempts to replicate this finding in independent data sets. All of these efforts support the position that lower class people are particularly responsive to stress (Brown et al., 1975; 1977; Dohrenwend, 1973; Kessler, 1979). Indeed, a recent investigation has suggested that this differential responsiveness to stress is the key component accounting for the relationship between social class and emotional disorder (Brown and Harris, 1978).

In the following discussion, we review several plausible interpretations of this class difference in responsiveness to stress, describe an analysis strategy for partially disentangling the influences suggested by these interpretations, and illustrate the usefulness of this strategy in an empirical application.

The Determinants of Responsiveness to Stress

It is generally agreed that the impact of stress on emotional functioning is mediated by characteristics of both the individual and his environment (Dohrenwend and Dohrenwend, 1970). The individual defines the significance of a potentially distressing situation, and this appraisal determines whether or not the situation will be experienced as threatening (Lazarus et al., 1974). Personality characteristics then act to multiply or reduce the impact of subjectively appraised stress. Anxiety proneness (Andrews et al., 1978) and low threshold for uncertainty (Smith et al., 1978), for instance, have both been shown to increase emotional arousal associated with demanding situations. High self-esteem (Kaplan, 1970) and perceived personal control (Johnson and Sarason, 1979), on the other hand, have both been shown to reduce perceptions of threat (Chan, 1977). In addition, concrete coping responses can vary in their effectiveness in routinizing stressful experiences (Pearlin and Schooler, 1978). All of these comprise what has come to be called "social competence," the individual's overall level of psychosocial functioning as seen in his ability to deal with the ongoing stresses associated with living (Smith, 1968).

The individual's environment is implicated in this coping process, both indirectly and directly. Experiences in living indirectly influence coping by helping shape the appraisals, beliefs, values, and behavior dispositions that make up one's coping repertoire. That is, the environment helps shape the individual's *intrapsychic resources* for coping. The environment can also influence responsiveness to stress more directly, by providing *social resources* for acting on the problem situation that creates stress. We are thinking here not only of financial and political power but also of guidance concerning coping options and of emotional support, both of which can be extremely important in helping a person deal effectively with a problem (Cobb, 1976; Miller and Ingham, 1976; Henderson, 1977; Henderson et al., 1978).

With these considerations as a background, it is relatively easy to offer a plausible explanation for the fact that stressful life experiences have their most damaging psychological effects among lower class people. This is so because lower class people are disadvantaged in their access to both intrapsychic resources and social resources. On the intrapsychic side, we know that feelings of self-worth and control over the environment are both positively related to class (Pearlin and Schooler, 1978; Rosenberg and Pearlin, 1978), as are several other intrapsychic characteristics thought to play some part in coping (Dohrenwend and Dohrenwend, 1970; Kohn, 1976; 1977). On the social side, we know that the poor are disadvantaged not only in terms of objective resources, like money and political power (Antonovsky, 1979), but also in their comparatively limited access to supportive social relationships and stable community ties (Dohrenwend and Dohrenwend, 1970; Liem and Liem, 1978).

Yet, while we know something about the broad outlines of the coping process and something about the coping resources differentially available to persons of varying class position, it is less clear how these various resources come to play a part in the comparatively greater emotional responsiveness of the poor. What are the relative roles of intrapsychic and social resources? And, if it seems that intrapsychic resources play a central part, how can we account for the fact that these resources are comparatively less available to the poor? These are centrally important questions, the answers to which (or, at least, more information about which) are needed for a more complete understanding of class differences in responsiveness to stress than we presently possess.

Contending Perspectives on Class Differences in Responsiveness to Stress

For researchers concerned with the relationship between social class and psychological disorder, the major research issue over the last several decades has been to disentangle the relative importance of two contending perspectives on this relationship: what have come to be known as the social-selection and social-causation perspectives (Dohrenwend, 1975; Liem and Liem, 1978; Wheaton, 1978). The former argues that people with psychological disabilities are more common in the lower socioeconomic strata because of natural competitive conditions that distribute people across the class structure according to their abilities. The social-causation argument, by comparison, holds that the life conditions to which lower class people are subjected lead to comparatively high levels of emotional disorder. In short, then, the contending hypotheses are: (1) that one's mental state helps determine his social position; and (2) that one's social position helps determine his mental state.

It is of some importance to recognize that the social-selection argument, as stated in this general form, is completely agnostic as to the relative importance of nature and nurture in bringing about psychological disorder. There has been a tendency, in the literature, to associate the social-selection argument with a genetic theory of causation (Dohrenwend and Dohrenwend, 1969; Dohrenwend, 1975). While it is true that if genetic factors play a significant role in mental disorders known to be associated with social class, then the only plausible way of understanding this fact is by positing a selection process. However, it is also perfectly conceivable that selection takes place on the basis of learned intrapsychic frailties distributed randomly (with respect to social class origins) among children who subsequently drift into the lower class as adults.

The central premise of the selection argument is that some chronic disorder *or* predisposition to disorder causes downward mobility. The traditional version of the argument has been associated with research on schizophrenia, where the symptoms of the disorder themselves are thought to be primarily responsible for downward drift (Dunham, 1965; Turner et al., 1970). Since it is inconceivable that a similar causal process (that is, symptoms themselves serving as a causal force and persisting chronically through time) could lead to downward mobility when less debilitating forms of disorder are involved, it is now believed that selection processes are of little importance in explaining the negative relationship between social class and scores on symptom screening scales in community surveys (Wheaton, 1978). However, it is perfectly conceivable that the factors which influence an individual's ability to compete successfully for social position also play a part in the ability to cope with stress. If this is so, and if we remember that class differences in mild forms of distress appear to be due largely to differences in *responsiveness* to stress, then the selection of competent copers out of the lower class and the selection of incompetent copers into the lower class could play important parts in accounting for class differences in the less debilitating psychological disorders.

We should note here that the social-selection argument tacitly assumes that intrapsychic resources are primarily responsible for class differences in responsiveness to stress. The social-causation perspective, by comparison—at least as it applies to class differences in responsiveness to stress—argues that social class largely determines the life experiences to which one is exposed, thus in large part shaping one's coping repertoire and determining access to social resources.

While the potential importance of social resources in coping with stress is obvious, several social-causation theorists have argued that class operates on responsiveness primarily by shaping intrapsychic resources. The culture of poverty theorists (Lewis, 1966), for instance, argue that individuals born into poverty go through a socialization experience tied to traditional lower class values, thus instilling in them outlooks and orientation to life that are associated with inadequate defenses and coping styles. According to this thinking, social class makes its mark on the individual primarily through childhood socialization, although the intrapsychic resources instilled in the individual during these critical formative years have a continuing influence throughout life (Langner and Michael, 1963; Liem and Liem, 1978). Other social-causation theorists, however, have argued that objective conditions of lower class existence, rather than lower class traditions, primarily determine the content of socialization experiences (Kohn, 1976; 1977). If this is true, an argument can be made that socialization is a lifelong process, one that is shaped as much by adult experiences as by those in one's childhood. This position has, in fact, been advanced (Levinson et al., 1978).

A Critical Contrast

To our knowledge, these various contending perspectives on class differences in responsiveness to stress have not been compared empirically. The approach we have taken, and which is described in this section, goes only part way in this task, leaving unresolved some important issues. Nonetheless, it succeeds in reducing, considerably, the range of plausible interpretations.

Our strategy consists of contrasting the relative influences of comparable stresses on members of different social classes on the basis of intergenerational mobility histories. Consider the patterns of intergenerational mobility diagrammed in Table 1, in which we have arbitrarily made distinctions among upper (u), middle (m), and lower (l) levels of socioeconomic status. Although all the perspectives predict that members of the lower stratum will be most vulnerable to distressing life experiences, they predict different patterns of relative influence within the more refined mobility subdivisions.

The childhood socialization perspective suggests that lower class *origins* will be associated with low resistance to stress and that the impact of a particular stress on some measure of emotional functioning will be large in cells 7, 8, and 9 in Table 1 relative to other cells—that is,

Table 1. A Hypothetical Mobility Table

	Respondent's Social Position		
	Upper	Middle	Lower
Parents' Social Position			
Upper	1	2	3
Middle	4	5	6
Lower	7	8	9

among persons born into the lower stratum, irrespective of their destinations. Consequently, if we find empirically that an observed class difference in responsiveness to some stress cannot, at least in part, be traced to differences in responsiveness among those with different class *origins,* controlling for mobility and destinations, a childhood socialization interpretation can be discounted.

The social resources perspective emphasizes differences in access to resources that can be effective in dealing with stress, thus suggesting that one's current class position (cells 3, 6, and 9) will be associated with low resistance to stress. Consequently, if we find that an observed class difference in responsiveness is not, at least in part, traceable to differences in class *destinations,* controlling for origins and mobility experiences, a social resources interpretation is discredited.

The social-selection perspective emphasizes drift into the lower classes, thus suggesting that *downward intergenerational mobility* (cells 2, 3, and 6) will be associated with responsiveness to stress. Consequently, if it can be documented empirically that social origins are not associated with responsiveness to a particular stress, we can discount the possibility that selection effects are responsible for the greater emotional impact of this stress on the poor.

The strength of this decomposition approach lies in its ability to reject particular interpretations. We learn much less here about the forces that are at work than about those that are not. For instance, if class differences in responsiveness can be traced to current social position and can be shown to be unrelated to both social origins and mobility experiences, we then can, with some confidence, reject the hypotheses that early childhood socialization or social-selection account for class differences in responsiveness. Yet we learn nothing from these data about the relative importance of adult socialization experiences and current social resources in helping the socially advantaged cope with stress.[2]

This illustration presages a pattern of relationships to be reported here, at which point we will have more to say about it. The important point to make at *this* stage of this discussion, though, is that while our approach is only partially successful in disentangling the various perspectives on responsiveness reviewed earlier, it can be effective in reducing considerably the range of plausible interpretations. Furthermore, although the strength of the approach lies in its ability to reject certain otherwise plausible interpretations, it can be augmented (in certain instances) with other data so that we can arrive at some relatively clear understanding of the causal processes at work in a particular instance.

Data and Procedures

Data: The data used to evaluate our strategy come from the New Haven longitudinal survey of mental disorder (Myers et al., 1972; 1974). The sample consists of 720 respondents who were interviewed both in 1967 and 1969, from an original random sample of 938 adults residing in New Haven, Connecticut.[3] These interviews dealt heavily with experiences that might have led to mild anxiety or depression.

---

[2] The major difficulty in making inferences about processes that might be at work lies with the socialization perspective. As we noted above, a case can be made for the premise that any life experience may function as a socialization experience. Consequently, the socialization perspective is consistent with virtually any pattern of origin/destination/mobility effects. And, so, the influence of social origins might be due to heredity—but it might instead be due to socialization; the influence of downward mobility might represent a social-selection process—but it might instead be due to socialization into a pattern of learned helplessness (Seligman, 1975).

We do believe that, at times, it is possible to infer that one process rather than another is at work. Yet we are on much less certain ground here than we are in rejecting an interpretation that is inconsistent with the decomposition results. For instance, if class differences in responsiveness to stress can be traced to differences associated with social origins and not to mobility or to destinations, we can, with some confidence, reject the hypothesis that class differences in access to social resources importantly influence responsiveness. But we cannot be clear, on the basis of these patterns alone, about the relative importance of socialization tied to social origins and genetic factors that might be heavily concentrated in the lower class. However, an additional consideration argues in favor of the socialization hypothesis. If we are to believe that genetic predispositions to disorder are concentrated in the lower class, then we must also believe that selection factors are at work. If this is so, we would expect some sort of downward mobility effect in the data. That such an effect does not exist argues against the genetic hypothesis.

The criterion measure of distress used here is Macmillan's (1957) symptom checklist scale as modified by Gurin et al. (1960). This scale is very similar in content to other "screening scales" of mild distress used in community surveys and can be interpreted as a measure of subjectively experienced mood disorder. In all, the scale is made up of 20 statements about bodily feelings that indicate moods of depression and anxiety through psychophysiological symptoms. These symptoms were selected from among those most frequently presented by patients in treatment.

It should be emphasized that this scale is not intended to measure clinically defined depression or anxiety when administered in a normal population such as this one. Clinical definitions require making qualitative distinctions between "cases" and "normals" on the basis of information about constellations of symptoms. No such distinction is made here. Rather, scores on the scale are used in this paper to order respondents on a continuum of reported magnitude of subjectively experienced distress. Scores on the scale range from 20 (all of the symptoms are reported as occurring "often") to 80 (symptoms are reported as occurring "never").

Socioeconomic status is measured by Hollingshead's Index of Social Position, a five-point scale which is a weighted function of occupation and education created specifically to describe the class structure of New Haven (Hollingshead and Redlich, 1958).[4] As a means of highlighting the contrasts between the lower and upper class strata, we have combined the upper three categories of this index (I–III) in our analysis. This combined category includes not only people who are normally considered members of the upper class but also those normally considered members of the middle class. Category IV on the scale consists largely of skilled and semiskilled workers and their spouses, while category V consists of unskilled workers, their spouses, and persons who are principally supported by some form of public assistance.

In preliminary analyses, we examined several stress measures and found two with significantly higher influences on the Gurin scores of lower class than higher class respondents.[5] The first of these is an index of reported undesirable life events experienced over the two years between data collections. The index consists of 17 events[6] weighted by the life change ratings reported in Holmes and Masuda (1974) and then summed to give an overall score for each individual. The second is an index of physical health problems, made up from a symptom checklist. Since the Gurin scale of emotional stress consists largely of psychophysiological symptoms, great care was taken in constructing this health problems index. The interview schedule contained a list of some 44 physical health complaints. Some of these were clearly psychosomatic. To create the health problems index we selected a subset of 16 complaints for which a psychosomatic component is probably of only minor importance. Larger weights were given to those that were most serious. Each respondent's score is a sum of these weighted complaints.[7]

---

[3] According to Myers et al. (1972:93), "8 percent refused to be reinterviewed; 11 percent had moved out of the area; and 4 percent had died."

[4] A reviewer of an earlier draft pointed out that the decision to combine education and occupation into a single index partially masks intragenerational downward mobility between educational attainment and later job attainment. This decision was made because we wished to focus here on intergenerational mobility. Nonetheless, the reviewer's point is well taken and a more detailed analysis than that presented here could proceed in two parallel steps—first studying intergenerational mobility and then intragenerational mobility.

[5] Five stress measures were examined in all. One (an index of desirable life events) had no meaningful influence on Gurin scores in any of the three social status subsamples examined. Two others (an index of ambiguous life events and an index of financial problems) had significant, but comparable, influences on Gurin scores in all three subsamples.

[6] The 17 events, and their weights in the index, are as follows: (1) moved to a worse neighborhood, 114; (2) widowed, 880; (3) divorced, 630; (4) separated, 541; (5) trouble with in-laws, 195; (6) serious injury or accident, 426; (7) death of a loved one, 380; (8) demoted or changed to a less responsible job, 228; (9) laid off temporarily, 305; (10) business failed, 205; (11) trouble with boss, 174; (12) out of work for over a month, 305; (13) fired, 384; (14) financial status a lot worse than usual, 288; (15) foreclosure, 248; (16) detention in jail, 474; and (17) loss of driver's license, 34.

Procedures: The origin, destination, and mobility patterns of interest in Table 1 can be described completely by eight dummy variables. However, in the New Haven data, two of the downward mobility cells (from I–III to IV, n = 14 and from I–III to V, n = 4) are small, requiring the use of only six dummy variables to describe mobility completely. A variety of ways exists to assess the importance of these contrasts. Our approach was to begin with origin, destination, and mobility effect patterns and to refine our contrasts in light of preliminary findings.

Consider Table 2, which provides a schematic view of the mobility table and our parameterization. The additive model of upper (u), middle (m), and lower (l) status origin and destination effects on average symptoms ($\overline{G}_{ij}$) in the ij^th cell is:

$$\overline{G}_{ij} = b_1 + b_2D(P_m) + b_3D(P_l) + b_4D(R_m) + b_5D(R_l), \qquad (1)$$

where P represents the social status of one's parents, and R the social status of the respondent; where b_1 is the mean in cell P_1R_1 (upper status origins and upper status destinations); where $D(P_i)$ is 1 in the i^th row, 0 elsewhere; and $D(R_j)$ is 1 in the i^th column, 0 otherwise. The parameters of this model estimate the expected cell means—due to the effects of origins, destinations, and the additive component of mobility. (The latter is confounded here with the origin and destination effects. See Halaby and Sobel, 1979.)

With the two additional degrees of freedom in the table, parameters can be estimated to distinguish (a) movers from stayers and (b) upward from downward mobility. Parameters for the influence of degree of mobility (one-step or two-step in these data) cannot be estimated, since they are embedded in the mobility effects and main effects of origin and destination (Halaby and Sobel, 1979). The fully specified model is:

Table 2. Schematic Diagram of Dummy Contrast Coding

	Respondent's Social Position			
	Upper (I–III)	Middle (IV)	Lower (V)	
Parents' Social Position				
Upper (I–III)	PuRu	×	×	(Pu)
Middle (IV)	P<R		P>R	(Pm)
Lower (V)	P<R	P<R		(P1)
	(Ru)	(Rm)	(R1)	

$$\overline{G}_{ij} = b_1 + b_2D(P_m) + b_3D(P_l) + b_4D(R_m) + b_5D(R_l) + b_6D(P \neq R) + b_7D(P < R). \qquad (2)$$

When we create interaction terms representing the products of these dummy variables with a measure of stress and enter these terms along with the main effects in (2), the within-cell slopes of symptoms on stress are recoverable. Just as cell means are partitioned in (2) into the main effects of origin, destination, and nonadditive mobility, the slopes of Gurin scores on stress in each cell are partitioned into origin, destination, and nonadditive mobility components in this more complete model. By examining these components, we can make inferences about the various perspectives on responsiveness to stress.

Results: Table 3 presents the results of three regression analyses, one each for the upper (Index of Social Position I–III), middle (IV), and lower (V) status subgroups. In each subgroup, Time 2 (1969) Gurin scores are regressed on Time 1 (1967) Gurin scores and two indicators of stressful life experience.[8] The first indi-

---

[7] These 16 complaints are: (1) eye trouble, (2) ear trouble, (3) sinus trouble, (4) throat trouble, (5) bronchitis, (6) pneumonia, (7) tuberculosis, (8) boils and abscesses, (9) diabetes, (10) kidney trouble, (11) bodily injury, (12–14) operations, with coding for up to three operations for each respondent, (15) cancer or tumors, and (16) tooth trouble, not including routine prophylaxis. Double weights were assigned to complaints 6, 7, 12, 13, and 14 and a triple weight to complaint 15.

[8] Note that each prediction equation includes the Time 1 (1967) Gurin score as a predictor of the Time 2 Gurin score. This inclusion addresses the possibility, raised by several critics of life events research (Brown, 1974; Rabkin and Struening, 1976; Dohrenwend et al., 1978), that emotionally distressed people often select themselves into stressful situations. For

Table 3. Subgroup Regressions of Time 2 (1969) Gurin Scores on Time 1 (1967) Scores and Two Measures of Stressful Life Experiences†

	Social Positions I–III			Social Positions IV			Social Positions V		
Predictors	b	β	X	b	β	X	b	β	X
Gurin I	.563*	.578	73.35	.464*	.487	72.18	.428*	.430	70.57
HP	.412	.068	.68	−1.699*	−.224	.73	−1.441*	−.239	1.12
WTUE	−.277	−.056	37.01	−.274	−.084	76.74	−.689*	−.191	133.22
Constant	32.659	–	–	41.020	–	–	43.942	–	
R^2		.330			.353			.367	
(n)		(147)			(286)			(189)	

* P≤ .05.

† Source: New Haven panel data (Myers et al., 1975), restricted to respondent with valid data on both his/her current Social Position and parents' Social Position. All regressions include controls for age, sex, and marital status. Slopes for WTUE are inflated by a factor of 100.

cator is the index of undesirable life events (WTUE). The second indicator is the index of physical health problems (HP). Excluded from the analysis are 124 respondents who failed to report either their own social positions or those of their parents.

There is a clear pattern of increased influence of stress as we move from higher to lower social positions. Neither physical health problems nor undesirable events significantly influence reports of emotional distress among people in positions I–III. Health problems exert a much greater influence in the status IV subsample, one that is both statistically significant in that subsample ($t = 4.523$, $p \leq .05$) and also significantly higher than the comparable coefficient in the I–III subsample ($t = 2.145$, $p \leq .05$). Finally, in the lower status subsample both physical health problems and undesirable events have significant effects on emotional functioning ($t = 3.809$, $t = 3.171$, respectively, both $p \leq .05$), although the WTUE coefficient does not differ significantly from the comparable coefficient in the middle status subsample ($t = 1.249$).[9]

---

instance, a visible emotional depression might lead to being fired from one's job. Anxiety might cause symptoms of physical illness. In the face of these possibilities, one cannot assume that observed relationships between stress and distress are due solely to the causal impact of stress on distress. Fortunately, the fact that we are working with panel data makes it possible for us to adjust for the confounding of causal direction by controlling Time 1 distress. In this way, we can include events that might be partially confounded by selection effects in our indexes and partial out these effects by means of statistical controls.

Over the course of the two years, symptoms of mild distress are relatively stable in the subsamples, with coefficients of stability between .43 and .58. This suggests that a good part of the variance in distress in this sample is due to disorders of considerable duration. Note, though, that both the standardized stability coefficients and the metric autoregression coefficients increase in magnitude as we move from lowest to highest class subsamples. These patterns argue against the suggestion that higher rates of disorder among the poor are partially due to a greater degree of stable emotional problems in this sector of society (Dohrenwend and Dohrenwend, 1965). If anything, it would seem that the stability of distress is greater among members of the higher social statuses.

[9] One reviewer of an earlier version of this paper suggested that the differential slopes observed in Table 3 might reflect a nonlinearity in the relationships between the stress indicators and Gurin scores. This could come about if, as is true for our measures of WTUE, mean levels of the stress increase substantially as we move from the higher to lower social positions. To evaluate this possibility, we estimated the following polynomial regression equation for the sample as a whole:

$$
\begin{array}{rl}
G_2 = & .496\ G_1 \\
 & (.523) \\
 & -1.500\ \text{WTUE} \\
 & (-.183) \\
 & +1.021\ \text{WTUE}^2 \\
 & (.063)
\end{array}
$$

The metric coefficients are presented in the equation, with standardized coefficients in parentheses beneath them. The linear effects of G_1 and WTUE are both significant. The quadratic coefficient is insignificant, which means that no single bend-point in the regression line captures a significantly greater degree of association between undesirable events and Gurin scores than that modelled in the linear relationship.

It is conceivable that this polynomial test overlooks subtle nonlinearities in the data. Therefore, we also estimated a series of 15 spline regressions, each

These slope differences are extremely important when considered in relation to the total difference in Gurin scores reported by respondents in the three strata. We can completely account for the significant difference in average Gurin 2 scores between the I–III and IV subsamples (means of 74.10 and 73.04, respectively) by standardizing on the slope difference associated with health problems.[10] The comparable difference in Gurin scores in the I–III and V subsamples (means of 74.10 and 71.58, respectively) is halved when we standardize on the slope differences associated with health problems and life events. These results clearly show that the differential influences are important components of the overall relationship between social position and distress.[11] However, it is worth noting that differences in average Gurin 2 scores between positions IV and V (means of 73.04 and 71.58, respectively) are reduced by only 18% when we standardize on slope differences associated with HP and WTUE. This means that slope differences are much more important in explaining the comparatively higher symptom levels of persons in social positions IV–V compared to those in positions I–III than in accounting for differences between the two comparatively lower status IV and V positions.

We now turn to an evaluation of the roles played by social-causation and social-selection factors in determining these slope differences. We will demonstrate here that mobility-related factors largely account for the comparatively greater influence of undesirable events on lower status respondents in this sample. Origin-related factors appear to be primarily responsible for the comparatively greater influence of physical health problems.

The differential influence of undesirable life events: Several models were estimated to evaluate the interactive influence of social origins, destinations, and mobility with our scale of undesirable life events on Gurin scores of distress. We began with a baseline model of predictors: Gurin 1 (1967), WTUE, and the six dummy origin/destination/mobility variables ($R^2 = .292$). Interaction terms to test the influences of origins, destinations, and mobility histories were sequentially added to this model. Only the mobility interaction model meaningfully improved on the baseline ($R^2 = .303$, $F_{2,585} = 3.385$, $p \leqslant .05$).[12]

The parameter estimates for the mobility interaction model are presented in Table 4. Here we see that upward mobility modifies the influence of life events on distress. Upwardly mobile people are *less* highly influenced by events than the nonmobile ($t = 2.529$, $p \leqslant .05$), differing in their responsiveness to each unit of WTUE by .598 on the Gurin scale. This difference remains significant when we

---

using a successively higher point on the scale as the bend-point of the spline. The results of this more detailed analysis are available on request. There was no evidence in these results of a significant nonlinearity.

[10] In other words, if people in the IV subsample were no more responsive to the effects of HP on their emotional functioning than people in the I–III subsample, their adjusted Gurin 2 scores would not differ significantly from those observed among people in the I–III subsample.

[11] This is not to say that these particular slope differences are the only components of importance in accounting for class differences in distress. There are doubtlessly a great many forces at work here, some of which work to the advantage (and some to the disadvantage) of people in the different social status positions. We are merely focusing on two of these and noting that, all else equal, these importantly contribute to the greater distress, observed empirically, among the poor.

[12] Having found that social origin and destination interactions are not significant in statistical terms, we know that the significant mobility interaction cannot completely be determined by structural mobility—that part of mobility which is related to the distribution of origin and destination statuses. However, it is possible that some part of the mobility interaction could be attributed to structural mobility and it would be informative to determine exactly how much (McClendon, 1977).

We failed in our efforts to separate the effects of structural and circulation mobility in this particular data set, each component proving too unstable to allow the estimation of a meaningful interaction parameter. We suspect, though, that it would be possible to perform a decomposition of this sort in a data set containing a more refined measure of social position and a larger sample size. This is a task we plan to pursue in our future work. In the present analysis, results are presented without controls for the origin/destination interactions, thus presenting an overall assessment of both structural and circulation mobility effects.

Table 4. Parameter Estimates for the Interactive Mobility Model of WTUE Influence†

Predictors	b	β
Gurin 1	.467*	.498
WTUE	−.786*	−.246
D(P_m or P_l)	1.256	.053
D(P_l)	.898	.059
D (R_m or R_l)	−1.371	−.078
D (R_l)	.971	−.061
D (P<R)	−1.904	−.124
D (P>R)	2.573	.093
WTUE×D (P<R)	.598*	.126
WTUE×D (P>R)	.034	.004
Constant	40.008	
R^2		.300

* $P \leq .05$.

† Slopes for WTUE are inflated by a factor of 100.

introduce controls for sex, marital status, age, and income (.564, t = 2.379, p ≤ .05). There is no meaningful difference in influences of life events on the stable and downwardly mobile (t = .089).

These findings are inconsistent with the childhood socialization perspective and with the social resources perspective, since neither origin nor destination status is meaningfully related to responsiveness to stress. Instead, we find that upwardly mobile people are comparatively unresponsive to stress. These upwardly mobile people are, by definition, not members of the lowest class and, empirically, are less common in the IV subsample than in the I–III subsample, thus creating the aggregate picture of comparatively higher responsiveness as we move down the status hierarchy.

This pattern is consistent with a variant of the social-selection argument—that competent people born into the lower class move up the class hierarchy as they reach adulthood. It is also consistent, though, with an adult socialization interpretation—that the experience of success associated with upward mobility creates the sort of assertive coping skills needed to avoid the psychological damage that can result from undesirable life events. There is no completely convincing way to disentangle these two interpretations in the data. However, we favor the adult socialization argument on the grounds of additional considerations. If selection processes are really at work, we would predict that the remaining members of social position V—both those who were born in this social class and those who have been unable to advance socially as adults—would be highly influenced by stress, as compared to the rest of the sample. A process of this sort would appear as an exacerbating effect of position V origins, after controlling for the ameliorating effect of upward mobility. There is no evidence of such an effect, however, in our data, making us suspicious of a selection interpretation of the mobility pattern.

The plausibility of an adult socialization interpretation is suggested by the literature on human development, which emphasizes the importance of adversity for the creation of competence, especially when this adversity is encountered early in one's life (Coles, 1967; Elder, 1974). According to this reasoning, success in overcoming adversity—one important instance of which is rising above one's disadvantaged social origins—can help create in an individual the sort of personal characteristics—feelings of self-esteem, confidence, perseverance—that are the stuff of competent problem management. Successful mastery of new challenges can help the individual grow in self-esteem and confidence, both of which have been shown to be important coping resources in adulthood (Pearlin and Schooler, 1978). By the same token, failures in meeting the challenges of developmental problems can lead to feelings of powerlessness, to self-abasement, and to the use of strategies for dealing with the world that are ineffective in achieving beneficial responses to life problems (Moriarty and Toussieng, 1976; Murphy and Moriarty, 1976).

How much of these complex series of successes, failures, and alterations in self-feelings are due to inborn characteristics, how much to early socialization experiences, and how much to pure luck, are matters difficult to disentangle. However, it is quite clear from additional analyses of the basic relationship that the interactive influence of upward mobility in dampening the impact of life events is associated with social competence. We find support for this interpretation, first, in Table 5, where we examine the relationship between exposure to stressful events and the

Table 5. The Influence of WTUE on the Use of Tranquilizing Medications, Separately in Subsamples†

Subsample	b	β	R²	(n)
Upwardly Mobile	.002	.008	.000	230
Not Upwardly Mobile	.027*	.129	.017	366
Not Upwardly Mobile and members of strata I–IV	.039*	.141	.020	167

* P ≤ .05.

† Slopes are inflated by a factor of 100.

Table 6. Estimates of the WTUE × (P<R) Parameter in the Interactive Mobility Model of WTUE Influence, Separately in Subsamples†

Subsample	b	β	R²	(n)
Chief Breadwinners	.685*	.185	.362	310
Not Chief Breadwinners	−.151	−.014	.227	286
Male	.660*	.185	.351	252
Female	.318	.042	.257	344

* P ≤ .05.

† Slopes are inflated by a factor of 100.

use of tranquilizer drugs. We know that competent people are characterized by active problem-solving styles and less competent people are more likely to rely on the sort of coping devices that help them live with stress and make its effects less severe (Moos and Tsu, 1976). These latter are the styles characteristic of people who use alcohol to help them forget their problems (Pearlin and Radabaugh, 1976), and who withdraw from normal social interaction defensively (Elder, 1974). They are also the styles that characterize people who use tranquilizing medications to help them deal with their life problems (Radelet, 1977). If our interpretation of the upward mobility specification is correct, we would expect that upwardly mobile people would be comparatively unlikely to make use of these drugs when confronted with stressful life events.

The results in Table 5 support this speculation. Among the upwardly mobile, there is no meaningful relationship between exposure to undesirable life events and the use of tranquilizing drugs. There is a significant relationship between the two among other members of the sample ($t = 2.481$, $p < .05$) and among other members of the sample in the same social positions as the upwardly mobile ($t = 1.827$, $p = .080$).

Other evidence consistent with this same argument is presented in Table 6. Our analysis here is motivated by Elder's (1974) work on childhood deprivation, where a stronger relationship between deprivation and adult competence was reported for men than women. We reasoned that a similar sort of process might be at work in the relationship between upward mobility and competence. Since women can enhance their social positions by marriage rather than by success in the world of work, we would expect some upwardly mobile women to have developed fewer of the skills associated with mastery than upwardly mobile men. On the other hand, we would expect women who made their ways to the top through their own successes in the business world to be equally as competent (and perhaps more so) as men who had the same successes.

In order to test these speculations, we estimated a model of the sort presented in Table 4 separately in subgroups of the sample defined by sex and by work status. We predicted that the relative resilience of upwardly mobile people to life events would be most clearly present among men and among workers. The results in Table 6 are consistent with these expectations. There is no meaningful interactive influence of mobility and WTUE on distress among women nor among people who are not the breadwinners in their families. This interaction is strong, though, among males and among those who are breadwinners.

Taken together, these various results suggest that the comparatively lower reaction to stressful life experiences of upper status people in this sample is due to the presence among them of extremely competent copers who have been upwardly mobile from lower class origins.[13]

---

[13] It is worth noting that several other perspectives predict a relationship between social mobility and emotional distress. One of these, associated with Durkheim's discussion of anomie, has been used in recent work to account for the greater degree of mental illness in socially disintegrated communities (Leighton, 1974) and among the upwardly mobile goal-strivers (Kleiner and Parker, 1963). This per-

There is no evidence that the objective conditions of existence are of assistance in helping these people deal with the emotional problems caused by stressful life events or that cultural conditions associated with their social origins are of importance in modifying their responses to stressful events.

The differential influence of physical health problems: Several models were estimated to evaluate the interactive influence of origins/destinations/mobility and physical health problems on Gurin scores of distress. We began with a baseline model of predictors: Gurin 1, HP, and the six dummy origin/destination/mobility variables (R^2 = .301). Terms for the interactions of origins, destinations, and mobility histories with HP were added sequentially to this model. The results of these analyses are very different from those found in our analysis of life events: (1) There is no meaningful modification of the relationship between physical health problems and distress associated with mobility. (2) There is some evidence that origins and (perhaps) destinations modify the influence of health problems on distress. The best overall model is one including the interactive effects of both origins and destinations (R^2 = 3.12), a model that significantly improves on the baseline model ($F_{4,583}$ = 3.183, $p \leqslant .05$).

Table 7. Parameter Estimates for the Interactive Origins/Destinations Model of HP Influence

Predictors	b	β
Gurin 1	.427*	.456
HP	.964	.145
D (P_m or P_1)	2.424	.103
D (P_1)	.849	.056
D (R_m or R_1)	−.832	−.047
D (R_1)	−1.240	−.078
D (P<R)	−1.738	−.113
D (P>R)	2.672	.097
HP × D (P_m or P_1)	−1.665*	−.241
HP × D (P_1)	.231	.029
HP × D (R_m or R_1)	−1.069	−.157
HP × D (R_1)	−.066	−.008
Constant	41.962	
R^2		.312

* $P \leqslant .05$.

The parameter estimates of the origins/destinations interaction model are presented in Table 7. Here we see that the influence of physical health problems on distress is highest for those with strata IV–V origins (t = 2.104, $p \leqslant .05$). This association is not changed by controls for sex, marital status, age, and income, the metric coefficient becoming −1.679 (t = 2.292, $p \leqslant .05$) rather than −1.665 as in Table 7. There is a similar, although statistically insignificant, modification associated with destinations in strata IV or V (t = 1.428). Our suspicion is that this destination effect would be significant in a larger data set or in one in which there was a greater degree of mobility than in this sample. As it is, we cannot reject the hypothesis that origins and destinations are equally important in a pooled model of "status" effects (the sum of origins and destinations). (Data available on request.)

Leaving aside the possible importance of destinations, the data are consistent with the childhood socialization perspective, which views social origins as important in shaping patterns of interpretation and coping that can magnify or reduce the influence of stress on emotional functioning. Before pursuing this pattern in more detail, it is worth mentioning that significant social origin effects have seldom been reported in the literature on mobility and mental disorder (Eaton and Lasry, 1978). Past research, however, focused on the main effects of origins rather than on interactive effects. Even in our own analysis, we find no significant main effects of social origins on Gurin scores. We believe this failure to detect a main effect of origins, even when an interaction of

---

spective would seem to have little direct importance as an explanation of the greater amount of neurotic distress in the lower class, though.

Other perspectives on the effects of mobility all emphasize direct effects, conceptualizing mobility experiences as stresses. Some view stresses associated with mobility as most extreme on the upwardly mobile (Eaton and Lasry, 1978), while others see them as more extreme for the downwardly mobile (Parker and Kleiner, 1966). They all share the assumption, though, that the mobility experiences have direct effects—rather than modifying effects—on distress.

Although it has been important in earlier analysis of the selection-causation argument to consider the possible biasing effects due to mobility related stresses (Wheaton, 1978:386–7), we distinguish this sort of effects from those we are considering by examining both main and interaction effects of mobility and stress on psychological distress.

origins and health problems is highly significant, is due to the fact that origins are associated with emotional functioning primarily through association with response styles that modify the effects of some stresses on functioning. Origins determine beliefs, values, and behavior styles that exert lifelong influences on one's ability to deal with stress. These effects are indirect and, consequently, will not be detected clearly in analyses of main effects.

Some inferential support for this view exists in the data gathered about health care behavior. We speculated that upper status people who were born in the lower classes might retain part of their lower class health care behaviors, thus accounting for the continuing influence of origins on the HP–distress relationship. To examine this possibility, we focused on respondents who had not seen a physician for at least one year (virtually all those who had seen a physician in the year reported that they had a "personal physician," the variable of interest to us). We then looked only at people in the upper social strata. Of these, 80% of those born in the upper strata reported having a personal physician, but only 48% and 38% of those born in strata IV and V, respectively, reported having a personal physician. These differences are meaningful in statistical terms ($t = 1.99$ for the I–III vs. IV contrast; $t = 2.18$ for the I–III vs. V contrast). However, the smaller percentages among the upwardly mobile are not meaningfully different from those of people in the lower class strata, who vary (from 36% to 55%) in their probabilities of having a personal physician. Clearly, the upwardly mobile members of the upper strata have retained one important component of the health care practices characteristic of their lower class origins, thus accounting for the fact that their origins—but not their current social positions—have a modifying effect on the relationship between health problems and distress.

The literature on illness behavior clearly shows that socialization experiences of children born into lower class families can modify their interpretations of health problems. Lower class people have been shown to be particularly concerned about the possibility of becoming ill (Johnson and Ware, 1976), and to see themselves as particularly susceptible to illness (Ware and Karmos, 1976). Furthermore, this literature suggests that these concerns are modified by cultural factors (Mechanic, 1978:261–6).

There is good reason to believe that these differing interpretations of illness can create exaggerated emotional responsiveness to health problems. For instance, Zborowski (1952:28) argues that

> in Jewish families, where not only a slight sensation of pain but also each deviation from the child's normal behavior is looked upon as a sign of illness, the child is prone to acquire anxieties with regard to the meaning and significance of these manifestations.

Support for this general interpretation is found in Table 8, where we report HP × D(P_m or P_1) coefficients estimated separately in subgroups as part of the social origins interaction model. We hypothesized that meaningful specifications should be found across subsamples defining what we considered different cultural conditions in the families of origin, if the significant interaction in Table 7 is actually due to socialization experiences in the childhood of lower class respondents. As we see in Table 8, specifications of this sort do exist. Origins modify the influence of health problems on emotional functioning more among Jews than Protestants, more among Protestants than Catholics, and more among persons whose mothers were native-born than among those whose mothers were foreign-born. Clearly, some features of lower class childhood are important in making these people differentially respon-

Table 8. Estimates of the HP × D (Pb or Pc) Parameter in the Interactive Origins/Destinations Model of HP Influence, Separately in Subsamples

Subsample	b	β	R^2	(n)
Mother Native-born	−2.584*	−.350	.290	(345)
Mother Foreign-born	−1.337	−.203	.336	(251)
Catholic	−.948	−.138	.256	(384)
Protestant	−2.034*	−.299	.341	(172)
Jew	−6.909*	−.913	.734	(29)

* $P \leq .05$.

sive to the emotional effects of illness in adulthood. These features apparently vary across certain cultural dimensions. And they are seemingly unaffected by the social positions achieved by these individuals as adults.

Discussion: We have not attempted to go far toward understanding the specific strengths that are important in helping one deal with stressful life events or with physical health problems. That was not our purpose here. Rather, we have attempted to make the case that social class differences in emotional distress are determined importantly by class differences in responsiveness to life problems. The data presented here clearly support this view, thus arguing against those who have focused exclusively on the differential exposure hypothesis of class differences. This latter hypothesis plays an important part, to be sure: Lower class people are exposed to more of the stressful events and situations that can lead to emotional distress than their middle and upper class counterparts. But we have argued, and demonstrated in our data, that a key part is also played by the fact that lower class people are more responsive emotionally to the stresses they experience, even when these stresses are no more common among them than among persons who occupy higher social positions.

Although we examined only two stresses, it is still clear that the modifiers associated with undesirable life events and physical health problems are quite distinct. This fact suggests that the resources that can be used to ameliorate distress vary from one problem situation to another. For instance, our results suggest that the resources available to people in socially advantaged positions do not modify the distress that can accompany undesirable life events. We suspect, though, that more careful analysis of subtypes of events would prove this statement more true of some events than of others.

Our analysis does not allow us to generalize about the specific types of problems that are reduced in their subjective stressfulness by the presence of objective resources, or about those that seem most highly influenced by social origins, or about those influenced by mobility processes. It would be valuable to know just what sort of problems these are and, in addition, to consider these in relation to the problems to which people in different class positions are equally responsive. In our own data, for instance, we found to our surprise that financial problems have very similar effects on the Gurin scores of respondents in the three social position subsamples. By itself, it is difficult to make sense of this absence of a differential impact when, on the face of it, it would seem that financial problems should have very different meanings for people whose access to broader financial resources varies. Our suspicion, though, is that this finding would take on more meaning if we were able to imbed it in the context of information about a wider variety of other problems—some of which have more damaging psychological effects on the poor than on the rich, some of which have comparable effects across class lines. The general analysis strategy used here provides a paradigm by means of which this type of information might be gained. We plan to pursue this line of research in our future work.

REFERENCES

Andrews, Gavin, Christopher Tennant, Daphne M. Hewson and George E. Vaillant
1978 "Life event stress, social support, coping style, and risk of psychological impairment." Journal of Nervous and Mental Disease 166:307–16.

Antonovsky, Aaron
1979 Health, Stress, and Coping. San Francisco: Jossey-Bass.

Brown, George W.
1974 "Meaning, measurement, and stress of life events." Pp. 217–43 in B. S. and B. P. Dohrenwend (eds.), Stressful Life Events: Their Nature and Effects. New York: Wiley.

Brown, George W. and Tirril Harris
1978 Social Origins of Depression: A Study of Psychiatric Disorder in Women. New York: Free Press.

Brown, G. W., T. Harris and R. Copeland
1977 "Depression and loss." British Journal of Psychiatry 130:1–18.

Brown, George W., Maire Ni Bhrol-chain and Tirril Harris
1975 "Social class and psychiatric disturbance among women in an urban population." Sociology 9:225–54.

Chan, Kwok Bun
1977 "Individual differences in reactions to stress and their personality and situational determinants: some implications for community mental health." Social Science and Medicine 11:89–103.

Clausen, John A.
1979 "Mental disorder." Pp. 97–112 in H. E. Freeman, S. Levine and L. G. Reeder (eds.), Handbook of Medical Sociology. 3rd ed. Englewood Cliffs: Prentice-Hall.

Cobb, Sidney
1976 "Social support as a moderator of life stress." Psychosomatic Medicine 38:300–14.

Coles, Robert
1967 Children of Crisis, Volume I: A Study of Courage and Fear. Boston: Little, Brown.

Dohrenwend, Bruce P.
1975 "Sociocultural and Social-Psychological Factors in the Genesis of Mental Disorders." Journal of Health and Social Behavior 16:365–92.

Dohrenwend, B. S.
1970 "Social class and stressful events." Pp. 313–9 in E. H. Hare and J. K. Wing (eds.), Psychiatric Epidemiology. New York: Oxford.

Dohrenwend, Barbara Snell
1973 "Social status and stressful life events." Journal of Personality and Social Psychology 9:203–14.

Dohrenwend, B. P. and B. S. Dohrenwend
1965 "The problem of validity in field studies of psychological disorder." Journal of Abnormal Psychology 70:52–69.

Dohrenwend, Bruce P. and Barbara S. Dohrenwend
1969 Social Status and Psychological Disorders. New York: Wiley.

Dohrenwend, Barbara Snell and B. P. Dohrenwend
1970 "Class and race as status-related sources of stress." Pp. 111–140 in Sol Levine and Norman A. Scotch (eds.), Social Stress. Chicago: Aldine.

Dohrenwend, B. S., Larry Krasnoff, Alexander R. Askenasy, and Bruce P. Dohrenwend
1978 "Exemplification of a method for scaling life events: the PERI Life Events Scale." Journal of Health and Social Behavior 19:205–29.

Dunham, H. Warren
1965 Community and Schizophrenia: An Epidemiological Analysis. Detroit: Wayne State University Press.

Eaton, William W. and Jean-Claude Lasry
1978 "Mental health and occupational mobility in a group of immigrants." Social Science and Medicine 12:53–8.

Elder, Glen H., Jr.
1974 Children of the Great Depression. Chicago: University of Chicago Press.

Fried, Marc
1975 "Social differences in mental health." Pp. 135–92 in J. Kosa and I. Zola (eds.), Poverty and Health: A Sociological Analysis, rev. ed., Cambridge: Harvard University Press.

Gurin, Gerald, Joseph Veroff, and Sheila Feld
1960 Americans View Their Mental Health. New York: Basic.

Halaby, Charles N. and Michael E. Sobel
1979 "Mobility effects in the workplace." American Journal of Sociology 85:385–416.

Henderson, Scott
1977 "The social network, support and neurosis: the function of attachment in adult life." British Journal of Psychiatry 131:185–91.

Henderson, S., D. G. Bryne, P. Duncan-Jones, S. Adcock, R. Scott and G. P. Steele
1978 "Social bonds in the epidemiology of neurosis: a preliminary communication." British Journal of Psychiatry 132:463–6.

Hollingshead, August B. and Fredrick C. Redlich
1958 Social Class and Mental Illness: A Community Study. New York: Wiley.

Holmes, Thomas H. and Minoru Masuda
1974 "Life change and illness susceptibility." Pp. 45–72 in B. S. Dohrenwend and B. P. Dohrenwend (eds.), Stressful Life Events. New York: Wiley.

Johnson, Shawn A. and John E. Ware, Jr.
1976 "Income group differences in relationships among survey measures of physical and mental health." Health Services Research 11:416–29.

Kaplan, Howard B.
1970 "Self-derogation and adjustment to recent life experiences." Archives of General Psychiatry 22:324–31.

Kessler, R. C.
1979 "Stress, social status, and psychological distress." Journal of Health and Social Behavior 20:100–8.

Kleiner, Robert J. and Seymour Parker
1963 "Goal-striving, social status, and mental disorder: a research review." American Sociological Review 28:160–203.

Kohn, Melvin L.
1976 "The interaction of social class and other factors in the etiology of schizophrenia." American Journal of Psychiatry 133:177–80.
1977 Class and Conformity: A Study of Values, 2nd ed. Chicago: University of Chicago Press.

Langner, Thomas S. and Stanley T. Michael
1963 Life Stress and Mental Health. New York: The Free Press.

Lazarus, R. C., J. R. Averill, and E. M. Opton, Jr.
1974 "The psychology of coping: issues of research and assessment." Pp. 249–315 in George V. Coelho, David A. Hamburg, and John E. Adams (eds.), Coping and Defending. New York: Basic Books.

Leighton, Alexander H.
1974 "Social disintegration and mental disorder." Pp. 411–23 in G. Caplan (ed.), American Handbook of Psychiatry, Vol. II. New York: Basic Books.

Levinson, Daniel J., C. N. Darrow, E. B. Klein, M. H. Levinson, and B. McKee
1978 The Seasons of a Man's Life. New York: Ballantine.

Lewis, Oscar
1966 "The culture of poverty." Scientific American 16:19–25.

Liem, Ramsay and Joan Liem
1978 "Social class and mental illness reconsidered: the role of economic stress and social support." Journal of Health and Social Behavior 19:139–56.

McClendon, McKee J.
1977 Structural and exchange components of vertical mobility. American Sociological Review 42:56–74.

Macmillan, A. M.
1957 "The health opinion survey: technique for estimating prevalence of psychoneurotic and related types of disorder in communities." Psychological Reports 3:325–9.

Mechanic, David
1978 Medical Sociology. 2nd ed. New York: Free Press.

Miller, P. and J. Ingham
1976 "Friends, confidants and symptoms." Social Psychiatry 11:51–8.

Moos, Rudolf H. and Vivien D. Tsu
1976 "Human competence and coping: an overview." Pp. 3–16 in Rudolf H. Moos (ed.), Human Adaptation. Lexington: D.C. Heath.

Moriarty, Alice E. and Povl W. Toussieng
1976 Adolescent Coping. New York: Gruen and Stratton.

Murphy, Lois Barclay and Alice E. Moriarty
1976 Vulnerability, Coping, and Growth. New Haven: Yale University Press.

Myers, Jerome K., Jacob J. Lindenthal, Max P. Pepper and David R. Ostrander
1972 "Life events and mental status: a longitudinal study." Journal of Health and Social Behavior 13:398–406.

Myers, Jerome K., Jacob J. Lindenthal, and Max P. Pepper
1974 "Social class, life events and psychiatric symptoms: a longitudinal study." Pp. 191–206 in B. P. and B. S. Dohrenwend (eds.), Stressful Life Events. New York: Wiley.

Parker, Seymour and Robert S. Kleiner
1966 Mental Illness in the Urban Negro Community. New York: Free Press.

Pearlin, Leonard I. and Joyce S. Johnson
1977 "Marital status, life-strains and depression." American Sociological Review 42:704–15.

Pearlin, Leonard I. and Morton I. Lieberman
1978 "Social sources of emotional distress." Pp. 217–48 in R. Simmons (ed.), Research in Community and Mental Health. Greenwich: JAI.

Pearlin, Leonard I. and Clarice W. Radabaugh
1976 "Economic Strains and the Coping Functions of Alcohol." American Journal of Sociology 82:652–63.

Pearlin, Leonard I. and Carmi Schooler
1978 "The structure of coping." Journal of Health and Social Behavior 19:2–21.

Rabkin, Judith G. and Elmer L. Struening
1976 "Life events, stress and illness." Science 194:1013–20.

Radelet, M. L.
1977 "Social factors influencing the medicalization of anxiety: a study of tranquilizer use." Ph.D. dissertation, Dept. of Sociology, Purdue University.

Rosenberg, Morris and Leonard I. Pearlin
1978 "Social class and self-esteem among children and adults." American Journal of Sociology 84:53–77.

Seligman, M. C.
1975 Helplessness: On Depression, Development, and Death. San Francisco: Freeman.

Smith, M. B.
1968 "Competence and socialization." Pp. 270–320 in J. A. Clausen (ed.), Socialization and Society. Boston: Little, Brown.

Smith, R. E., J. H. Johnson and D. G. Sarason
1978 "Life change, the sensation seeking motive and psychological distress." Journal of Consulting and Clinical Psychology 46:348–9.

Turner, R. Jay, Leslie S. Dopkeen and Gary P. Labreche
1970 "Mental status and schizophrenia: a study of incidence and outcome." Journal of Abnormal and Social Psychology 76:110–6.

Ware, John E., Jr., and A. H. Karmos
1976 "Development and validation of scales to measure perceived health and patient role propensity." School of Medicine, Southern Illinois University.

Wheaton, Blair
1978 "The sociogenesis of psychological disorder: reexamining the causal issues with longitudinal data." American Sociological Review 43:383–403.

Zborowski, Mark
1952 "Cultural components in responses to pain." Journal of Social Issues 8:16–30.

ACCESS TO INTERORGANIZATIONAL NETWORKS AS A PROFESSIONAL RESOURCE*

JON MILLER
University of Southern California

American Sociological Review 1980, Vol. 45 (June:479–496)

Six social service delivery systems for juvenile offenders were structured as interorganizational resource networks on the assumption that this would facilitate the development of a firm community base for their activities. The access that individual practitioners had to these program networks was seen theoretically as a measure of their integration into a larger social collectivity, a conceptualization that tied the analysis to a Durkheimian view of social structure. The specific hypothesis that was tested was that a strategic network position would be associated with more extensive professional ties to important institutions and decision-making centers in the surrounding community. In four of the six programs the hypothesis was confirmed; network centrality, measured sociometrically, was in fact a better predictor of the participants' community contact and community activism than their personal attributes, the technical resources and skills they brought to their work, or the formal status and autonomy they had achieved in the agency that employed them. In one of the two remaining programs, the hypothesis was not confirmed and, in the other, a reversal was evident: individuals with central network positions were less involved in the community than those who were relatively isolated from the network. The results are offered as evidence of how social network analysis can bridge levels of inquiry and provide new ways of approaching questions derived from traditional sociological theory.

INTRODUCTION

It is an established theme that organizations cannot be understood fully without evaluating the ways in which they deal with the interorganizational and community environments in which they are found (Aldrich, 1971; Aldrich and Pfeffer, 1975; Perrow, 1979). The surrounding context determines the resources and outlets that are available, and also the points of cooperation, conflict, and competition that must be dealt with. (For empirical treatments of these propositions see Simpson and Gully, 1962; Aiken, 1974; Yuchtman and Seashore, 1967; Seashore and Yuchtman, 1967; Aiken and Hage, 1968; Hall et al., 1977; Knoke and Rogers, 1978; Bick and Müller, 1978; Stern, 1979.)

This survey of service delivery systems adds a dimension to this viewpoint. The central proposition, derived from a Durkheimian view of social structure, is that the integration of an individual organization member into an interorganizational network of activity is a good predictor of other work-related behavior, including, in the present case, extensive professional involvement with the community in which the interorganizational system is embedded. Although the individual organization member has been kept as the focus of interest, the analysis actually cuts across five distinct levels of observation, represented in Figure 1. Information about the professional and personal attributes of individual practitioners (Level I) was supplemented with information about

---

* Direct all communications to: Jon Miller; Department of Sociology; University of Southern California; Los Angeles CA 90007.

An earlier version of this paper was presented to the Session on Interorganizational Relations at the meeting of the International Sociological Association, Uppsala, Sweden, August, 1978. A colleague on the project, James Lincoln, shares the credit for generating and processing the data, and Margo Gordon and Larry Heck provided indispensable technical assistance. I am grateful to Herman Turk, Rolf Ziegler, Werner von der Ohe, and William Evan for their comments on earlier drafts. The paper was prepared under grant number 78NI-AX-0135 from the National Institute for Law Enforcement and Criminal Justice and grants 76JN-99-0014, 76JN-99-1004, and 77JN-99-0018 from the National Institute for Juvenile Justice and Delinquency Prevention, Office of Juvenile Justice and Delinquency Prevention, Law Enforcement Assistance Administration, United States Department of Justice. Points of view or opinions in this document are those of the author and do not necessarily represent the official position or policies of the United States Department of Justice.

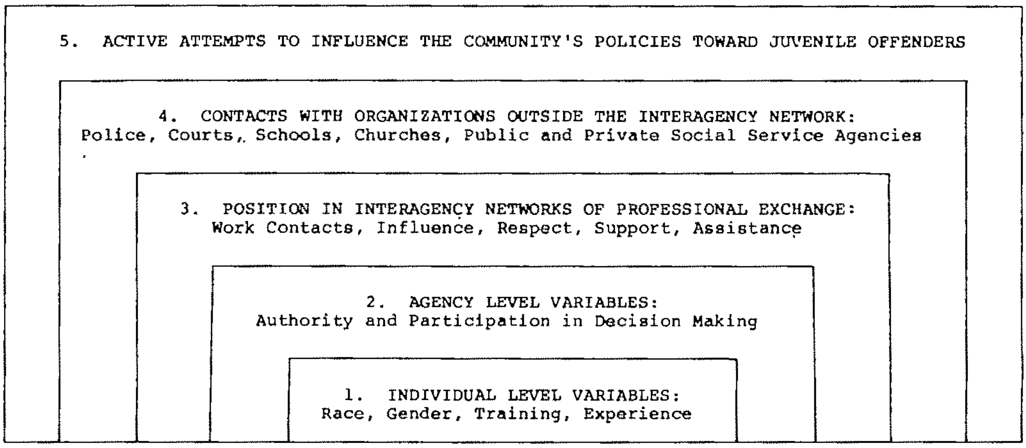

5. ACTIVE ATTEMPTS TO INFLUENCE THE COMMUNITY'S POLICIES TOWARD JUVENILE OFFENDERS
4. CONTACTS WITH ORGANIZATIONS OUTSIDE THE INTERAGENCY NETWORK:
Police, Courts, Schools, Churches, Public and Private Social Service Agencies
3. POSITION IN INTERAGENCY NETWORKS OF PROFESSIONAL EXCHANGE:
Work Contacts, Influence, Respect, Support, Assistance
2. AGENCY LEVEL VARIABLES:
Authority and Participation in Decision Making
1. INDIVIDUAL LEVEL VARIABLES:
Race, Gender, Training, Experience

Figure 1. Levels of Conceptualization Involved in the Investigation

their role in the agency by which they were employed (Level 2); their place in the interorganizational network of which their agency was a part (Level 3); their relations with specific organizations that were *not* included in this network (Level 4); and their active involvement, as professional practitioners, in attempts to influence the community decisions that were important for their clients (Level 5).

The respondents in the survey were the professional practitioners in six regional programs in a national project for the "deinstitutionalization" of "status offenders" (DSO). Because status offenders are problem youth whose offenses would not constitute legal offenses for adults, the objective of the project was to develop treatment alternatives that would not involve them in the official institutions of the juvenile justice system. To accomplish this, each DSO program (their origins will be described in more detail in the following section) was structured as a network of interacting, but formally independent, agencies. This strategy was based on the presumption that a member of one of the agencies in such a network would be able to draw on the other agencies in the network as a reservoir of helpful contacts, information and assistance, and that they would also be able to use their position in the network as a base from which to establish extensive professional ties to other key organizations outside the network itself, and as a vehicle for becoming actively involved in community decision making concerning the treatment of juveniles. In principle, then, a practitioner who was well connected within a DSO network would have an advantage in contributing to the development of a program with a firm community (as opposed to legal-institutional) base, an advantage that would be denied to an individual who was isolated or poorly placed within this network.

Together, the insistence by the funding source on multiagency networks as a service delivery mechanism and their emphasis on community-based services as a major program objective are an indication that the organization-to-organization and organization-to-community relationships that have long been of interest for organizational theorists have found their way into the central concerns of social service planners as well (on this point, see also Barth, 1963; Reid, 1964; Hasenfeld, 1972; Klein, 1976; Spergel, 1976; and Lincoln, 1979). In fact, in a rare instance of consistency between social practice and sociological theory, the rationale underlying the establishment of the DSO programs can be restated as a hypothesis: The more extensive the linkages that tie an individual into a functioning social network, the better situated the individual is to share in the benefits of that association and, therefore, to contribute to the objectives of the collective effort. Stated less abstractly and in the form in which it will be tested, the hypothesis is: The greater the involvement in a professional resource network, the greater the community involvement.

If we use "social integration" to refer to the extensiveness of the bonds tying individuals into a coherent social context, balanced, implicitly, against the fragmenting or individualistic forces that pull the individual away from the network, then the indebtedness of this basic argument to Durkheim (1952:208ff.) is apparent.[1] Taken literally, there is, of course, little resemblance between the contributions made to the "community-basedness" of a social program (which is our direct empirical concern) and the self-destructive behaviors that were Durkheim's immediate concern. However, the notion of social integration, to which we give empirical expression through the imagery of the social network, is sufficiently flexible as a theoretical construct to embrace a wide range of substantive interests.[2] Viewed in this light, much of the recent work on social networks—for example, works as diverse as Granovetter's (1974) study of personal networks in the search for employment, or Horwitz's (1977) work on the role of networks in the availability of mental health resources and, in the organizational literature, Blau's (1955) study of consultation patterns and structural effects (see also Rice and Mitchell, 1974)—can be seen as variations on a common theoretical theme. Before proceeding too far with this reasoning, however, there are countervailing arguments, to which we now turn, that suggest caution in applying this basic orientation to interorganizational problems.

As Turk (1973) has pointed out, relations among organizations are usually at least partially conflictive because the benefits of cooperation are counterbalanced by the necessity to compete for advantages and resources (see also Benson, 1975). In addition, Saranson et al. (1977) have argued forcefully, but less persuasively (given the thinness of their data), that interagency exchanges cemented primarily by money are counterproductive in the long run because the money nexus distracts attention and energy away from the proper performance of program activities.

In the case of the DSO project, these points are not without relevance. It is probable that some of the specific interagency linkages that were finally incorporated into the service delivery networks were entered into expediently by agency directors in order to qualify for the federal money. As a consequence, practitioners from different agencies were sometimes brought into interaction with each other without a clear-cut preexisting cooperative understanding and, in some instances, were required to participate in interagency activities and treatment practices that ran counter to their own professional philosophies. They then faced the problem of establishing the worth of their own approach to the problems of juveniles in the face of claims made by the other participating agencies in the network. In other words, the DSO programs were assembled in less than ideal circumstances. Agencies and individuals had much, potentially, to gain by participating in a network, but were asked to make sacrifices in return, not the least of which was the loss of autonomy—it was implicit in the agreement that they were to abide by the mandate imposed by a distant federal agency. Fry and Miller (1975) have pointed out that social experiments that are set into operation in such settings, even if they are intelligently staffed and generously funded, face built-in conflicts that can militate against success from the very beginning.

---

[1] Equally useful parallels could be drawn between contemporary social network analysis and other traditional theoretical formulations. Simmel (1955) is an obvious candidate for such an explanation.

[2] Drawing on Durkheim for insights into organizational activities is not entirely new. Blau (1977, chap. 8 and, especially, pp. 198ff.) for one, relies heavily on *The Division of Labor in Society* (Durkheim, 1933) in developing propositions about the division of labor and its correlations in work organizations. However, at the center of his discussion is a critique of the proposition that division of labor promotes solidarity, or social integration, through the mechanism of functional interdependence (social integration is defined approximately, as here, in terms of frequency of social contact). His argument is that functional interdependence need not imply interaction, particularly interaction between persons performing different (but complementary) functions in different organizations, who are usually also separated by social status differences. Whatever the validity of Blau's argument in some interorganizational contexts, it has little relevance in the present study, which is confined to a context in which the complementary functions of members in different agencies who were occupationally similar and more or less equal in status in fact did call for direct and indirect associations among them. In other words, it was precisely their functional interdependence in responding to the needs of clients that required their interaction. The question, then, is whether the effects of individuals' integration into a network of contacts at this interorganizational level could be traced out into their involvement with still other social contexts even further removed from the immediate workplace. Incidentally, Blau's assessment of the applicability of Durkheim to strictly *intra*-organizational problems has also been questioned (Lincoln and Miller, 1979).

On the other hand, Turk (1973) also reminds us that organizations, more than individuals, are able to balance the demands of conflict and cooperation by forming multiple linkages, sustained by different representatives for different kinds of interaction. In this way the value, on balance, of interorganizational ties may overshadow the circumstances in which they evolved. If so, then stable networks of exchange can continue to provide benefits even when a complete community of interest among the constituents does not exist. With this in mind, it is reasonable to take the hypothesis that we stated before as a point of reference. The mixed circumstances under which the DSO networks were established means that the hypothesis will have a fairly stringent test.

To assess the hypothesis properly, the effects of program participants' connections in these interagency networks must be compared with the effects of other variables that would also be expected to have an impact on their professional activities. Measures of training and experience are included in the analysis because they deal with the professional competence of program participants. Gender and race must also be accounted for because there is evidence that women and blacks work at a disadvantage in some organizational settings (Kluegel, 1978; Miller et al., 1975; Miller et al., 1980; Miller and Ransford, 1978), a fact that could influence their ability to form alliances both within the network and with important actors outside it. There is also evidence that official organizational rank may be a key to extensive interorganizational ties and linkages to the larger community (Blau, 1977:199; Perucci and Pilisuk, 1970). However, it has also been noted that most organization members of high and low status function as outside representatives, whether or not this is explicitly spelled out for them by the organization (Haas and Drabeck, 1973:220ff.; see also Evan, 1976). This was the case in the DSO programs; practitioners at all levels were expected to interact with agencies and institutions outside their own work settings. Therefore, to find a strong positive effect of status on community ties and community activism would indicate that this expectation was being violated. This possibility, alone, provided a reason to take this variable into account. Finally, in all of the programs, rank and file participation in decision making was the dominant and legitimate pattern.[3] Whether this also facilitated performance in community-based activities is a question that needs to be asked. Evidence to date concerning participation and performance has been quite mixed; this is apparent when we compare the critical review of Strauss and Rosenstein (1970) with a more recent and quite strongly argued study by Pennings (1976).

THE RESEARCH SETTING

The DSO project was put into operation in the spring of 1976 by an agency of the U.S. Department of Justice. Seven programs that submitted acceptable proposals were included in this survey,[4] which took place from six to nine months after the programs were funded and in operation. However, in one program a questionnaire that did not include any measures of network participation had to be used and, for this reason, a full test of the hypothesis was possible only in the remaining six. The two aspects of the programs of most interest here are their network structures and the mandate imposed upon them by the funding source that called for two different kinds of community-based activity. These will be discussed in turn.

---

[3] A large majority of both supervisors and supervisees in all programs reported a participatory decision-making arrangement that involved a great deal of discussion. Both groups also rated the quality of the supervisor/supervisee relationship favorably (means around seven on a nine-point scale). Fewer than 10% in either category expressed a desire for less participatory styles of decision making.

[4] Actually, eight programs shared the approximately $12 million allocated for the national DSO program. However, one of these was delayed in starting for several months and had to be excluded entirely from this survey. The remaining programs were widely dispersed, with two in the Northwest, one in the Far West, and one each in the Southwest, Midwest, Northeast and Southeast.

The Network Structure of the Programs

In each of the DSO programs, the grantee agency, which became the fiscal and administrative center, was required to establish and oversee an interagency service delivery "system," or network, as a way of bringing previously dispersed and autonomous service delivery resources in the community together into a coordinated joint enterprise. The agencies that were brought into these networks had to agree to function as part of such an effort in order to receive their share of the grant funds.

Among the funded projects, one relied almost exclusively on private agencies, two relied primarily on linkages among public agencies, and the others brought together a mixture of public and private agencies. Each program provided us with a roster of participating organizations so that the boundaries of the networks at the time of the survey could be accurately established. Within these boundaries, the separate agencies shared one common source of funding, a common set of official goals, a common "client" pool, and a common relationship to the organization occupying the recognized administrative center of the program. They also occupied complementary positions in an interagency division of labor and, therefore, were bound together to some extent by reciprocal obligations. For these reasons, the agencies in the networks were clearly set off from those that remained outside the networks.

In two of the programs, a network of agencies for dealing with juvenile offenders in general, not just status offenders, was already in place before the DSO funding began. The directors overseeing these established networks based their applications for DSO funding on the claim that status offenders could be dealt with as a clearly delineated category through their existing interagency mechanisms. In all the other programs, the networks were assembled largely de novo, specifically for the DSO project. This distinction will be important later in the interpretation of the findings.

The Mandate for Community Involvement

Because of the stress on treatment alternatives with a broad community support base, a matter of both theoretical and practical concern was the relationship between the network of agencies in a given program and the police, courts, schools, churches, and other public and private agencies that remained outside the DSO network, per se. In principle, by being part of the network, individual practitioners would have more knowledge of (and more points of access to) this larger circle of organizations. Their own direct contacts with these outside organizations would be multiplied by the shared pool of contacts that would then become accessible through interaction with other members of the network.

The second, and equally important, component of the mandate for community involvement was the expectation that the network participants would be active in attempts to influence the development and application of local treatment policies and in efforts to raise the level of support from the community as a whole for juvenile programs. In other words, it was expected that they would be active advocates (see Coates, 1974), engaged in efforts to change the climate of response in the community to the problems of status offenders.

METHODS AND MEASURES

The number of agencies involved in the DSO project ranged from seven in the smallest program to 25 in the largest, with a total of 88 agencies overall. Typically, the agencies were small and, because they had other obligations (for example, continuing to deal with juvenile offenders who were not categorized as status offenders), they sometimes assigned only a subset of their members to the DSO network. As a result, the number of subjects for the survey, 522, was not large. Of this number 350 (67%) responded.

A separate regression analysis was performed for each of the programs and the results are presented as a series of independent replications. This approach was selected because it preserves the distinctions among the programs so that the differing circumstances under which they were established can be highlighted.[5] No direct comparisons of standardized regression coefficients between programs are intended; rather, the approach is to assess the overall configuration of findings from one program to another. Analytically, the data cut across the five distinct levels indicated in Figure 1. The measurement of variables from each of these levels will now be described, beginning with the dependent variables, the two dimensions of community involvement (Levels 4 and 5).

Community Involvement: CONTACT and ACTIVISM

Six questions in the survey measured the respondents' contact, in their capacity as DSO practitioners, with law enforcement agencies, schools, churches, courts, and public and private social service agencies that remained outside the interagency DSO program. The correlations among the measures were all positive (see Appendix A), and the alpha reliability coefficient for an index based on their sum (referred to as CONTACT) was .74.[6] The format of the questions, which had seven response categories ranging from "never" to "almost constantly," is given by this example:

"In the course of your work for the DSO program, how often do you come into contact with (members of local police and/or sheriff's departments)?"

Another four items registered the respondents' professional activism in the larger community by asking how often in the course of their work for the DSO program they were involved in efforts to get more local economic support for programs for juveniles; efforts to get more community organizations involved in the problems of juveniles; attempts to influence local policies on the ways the problems of juveniles are handled; and attempts to get better local treatment resources for the problems of juveniles. These four items, which were also positively intercorrelated (Appendix A), were combined into an ACTIVISM index with a reliability coefficient of .88. (The means and standard deviations for these two community involvement indexes are given in Appendix B.)

Network Variables

The connectedness of interorganizational fields has been assessed in a variety of ways. In Turk's approach (Turk, 1970; 1973) interorganizational exchanges are inferred from the functional and spatial division of labor that is known to exist among a set of organizational actors. A different approach is represented by measures based on the existence of joint programs (Aiken and Hage, 1968; Hall et al., 1977) and a third method is emerging from sociometric network analysis, an approach that assesses interorganizational ties through the linkages that exist among the members of formally discrete organizational units (Anderson, 1967; Rogers, 1974; Knoke and Rogers, 1978).

The method used here is a variant of this third approach. Our concern was to locate individuals in the overall mesh of interpersonal connections that linked together the different agencies constituting a given DSO program. These linkages were established from reports of closest WORK CONTACTS and four other sociometric dimensions, including INFLUENCE (who determines how the work of the program is done); RESPECT (whose professional opinion is most highly regarded); informal SUPPORT (who is dependable in times of crisis); and professional ASSISTANCE (who is a good source of professional advice).[7] For these items, respondents were instructed to report on their relations with others throughout the interagency service delivery system rather than to confine their choices just to members of their own agency. Nominations that were, in fact, interagency as opposed to intraagency ranged from 36% to 60% on the work contacts criterion, with similar proportions on the other four sociometric items. In each of the programs, virtually every respondent was tied, through a varying number of intermediate links, to every other participant in the interagency network. For this reason, but also because the interagency network itself was the operating unit and, therefore, the focus of our attention, we have not separated intra- and interagency connections in the analysis.

---

[5] An alternative, which we elected not to pursue in this presentation, would involve a pooling of the data from the different programs, with dummy variables and interaction terms introduced to assess the effect of program site on the relationships of interest.

[6] This index was constructed to simplify and condense an otherwise unwieldy analysis. Separate regressions for each of the six items in the scale (not shown) produced results that did not depart substantially from the results reported for the index. In addition to recording the frequency of their contacts with organizations outside the network, respondents were also asked to rate those contacts in terms of the level of cooperation and the level of client benefit produced. In all the programs these evaluations were clearly favorable: means ranged from 6 to 8 on nine-point scales. Therefore, it is assumed throughout the analysis that extensive contact with outside agencies represented a positive contribution to program goals.

In processing the WORK CONTACTS data, a measure of individual Centrality was employed (Shaw, 1964; Harary et al., 1965) that indexes the number of links that must be crossed in order for a given individual to be reached by every other person in the network. It, thus, reflects both the direct (A-B) and indirect (A-B-C-D) paths, and their lengths, that tie a participant into a network; a highly central individual is one who is tied to others in the network by a large number of short distance connections.[8] The properties of the network and the positions of individuals in it are derived from the matrix of choices exercised by all the respondents. The structure of interaction and communication thus revealed is clearly a "social fact," to borrow again from Durkheim (1938: lvi; 12) and the measure of an individual's Centrality in the network is a relational property (Lazarsfeld and Menzel in Etzioni, 1961) for which the respondent, alone, should not be expected to be a completely reliable informant, since it is based on a collective judgment.

The official rosters of participating agencies prepared for us by the program administrators made it possible to specify the boundaries of the DSO networks with some certainty. The Centrality measures thus refer to individuals' locations within clearly defined and circumscribed multiagency fields. It is important to note that the items comprising the measure of community CONTACT (one of the two dependent variables described above) were deliberately phrased to deal with contacts developed with organizations outside this field and, therefore, represent a distinct sphere, or level, of activity. Thus, both Centrality and CONTACT tap interorganizational relationships sustained by individuals, but they are defined and measured independently and, therefore, can stand in the logical relation of independent to dependent variable (centrality in the network facilitates contact in the community) without becoming tautologically intertwined.

---

[7] The sociometric questions we used asked the respondents for a fixed number (three) of nominations, a method which, though it has important recent precedents (Laumann and Pappi, 1976; Burt, 1977), has also been criticized (Holland and Leinhardt, 1973). The rationale of the forced choice format is to confront respondents with a standard stimulus, and to minimize the reporting of ties that might be unimportant. A criticism is that this method is subject to error because of underreporting (or more rarely, overreporting) and, thus, may produce an inadequate picture of the density of the overall network. In the present case, however, no participant in any of the networks was excluded by the method we used (that is, neither chose nor was chosen), and we are confident that the basic structure of the networks was accurately captured. However, we agree that the open-ended format is preferable for most organizational and interorganizational applications.

[8] The Centrality data were generated by a program for DIGRAPH analysis generously furnished to us by Peter Marsden. Statistical properties of the index are also discussed by Shaw (1964). Another application of this measure to organizational data appears in Rice and Mitchell (1974). The Centrality index that we used is highly correlated with other, simpler, sociometric scores—in particular, the simple sum of choices received from others, but, because it includes information on indirect ties, it provides a more sensitive gauge of individuals' access to channels of communication and interaction.

Freeman (1977) has described an alternative measure of network centrality based on "betweenness," or the frequency with which an individual's position allows him or her to mediate the connections among other participants in the network. It should be very useful for isolating exchange "brokers" in a network, but this was not our objective. Rather, we were concerned with the extent to which individuals had access to, not control over, manifold others and for this purpose the Centrality index we employed is preferable.

The remaining four sociometric items were processed in much the same manner as the measure of WORK CONTACTS, but note that the "networks" derived from these items are not based on actual interaction. Rather, they comprise a collective representation of how INFLUENCE, RESPECT, MUTUAL SUPPORT, and ASSISTANCE were distributed among the participants. In fact, the responses based on these items largely replicated the WORK CONTACTS and, as a result, the five network measures are highly intercorrelated (Appendix A).[9] In the regression analysis, it was not possible to separate their effects on CONTACT and ACTIVISM with any confidence. For this reason, they were entered separately into the analysis, but a single Beta (a "sheaf coefficient") was computed, according to the method proposed by Heise (1972), to record their joint impact on the dependent variables. This method is preferable to combining the five measures into a single index and we believe it is also superior to simply choosing one measure and excluding the others from the analysis. However, inspection of preliminary results based on all three of these approaches showed that the substantive conclusions, in fact, would be quite similar. Only one set of results is reported.

Intraorganizational Variables

The organizations brought into the DSO networks were generally quite "flat" in their internal hierarchies; consequently, organizational STATUS refers to the dichotomy, supervisor/nonsupervisor. Ability to participate in the organizational decision-making process, PARTICIPATION, was measured by a dummy variable based on an item (taken in slightly altered form from George Miller [1967]) that records whether respondents were involved in participative, directive or laissez faire relationships with their supervisors.

Individual Resources and Attributes

At this level, four variables were entered into the analysis. GENDER and RACE were dummy coded. EXPERIENCE refers to the number of years the individual had worked at his or her occupation, and EDUCATION to the number of years of formal schooling reported. A measure of tenure was excluded because of the brief history of the DSO programs.

FINDINGS

The results fall into two distinct patterns, one supporting the hypothesis and the other questioning it. The prediction that network location has a positive impact on the level of practitioners' contact with non-DSO organizations and on their activism in the community at large received substantial support in the programs numbered one to four in Table 1. However, very different configurations appeared in Programs Five and Six, which were the two programs that had added the processing of DSO clients to the activities of ongoing service delivery networks. These disparate findings will be discussed in turn.[10]

Results Supporting the Hypothesis

In the first four programs, the net effect of integration into the interagency networks explained from about 11% to almost 60% of the variance in CONTACT

---

[9] Note also in Appendix A that, within each program, the five different network measures showed quite similar patterns of relationships to the two dependent variables, CONTACT and ACTIVISM. The implication is that there actually was a single network of interaction that served a number of different functions for the participants in these programs.

[10] The data for the seventh program, for which no network data were available, are also displayed in Table 1. Without the Centrality measures the explained variance for CONTACT and ACTIVISM (8% and 17%, respectively) was substantially smaller than that reported for the other six programs.

Table 1. Regression of Community Contact and Community Activism on Individual Resources, Organizational Position and Network Location, for Seven Interagency Programs Involved in the Deinstitutionalization of Status Offenders (DSO)

	Program One (N=76)		Program Two (N=37)		Program Three (N=36)		Program Four (N=20)		Program Five (N=57)		Program Six (N=35)		Program Seven (N=89)	
Individual Resources:	CONTACT	ACTIVISM	CONTACT	ACTIVISM	CONTACT	ACTIVISM	CONTACT	ACTIVISM	CONTACT	ACTIVISM	CONTACT	ACTIVISM	CONTACT	ACTIVISM
GENDER	.03	.08	−.10	.36*	−.09	−.02	.06	−.06	.02	.07	−.04	.29	.03	.10
RACE	−.10	−.05	−.22	−.39*	−.36	−.30	—[a]	—	−.06	−.21	.00	.30	.11	−.12
EXPERIENCE	.03	.09	−.26	−.19	.63*	.94	−.10	−.51	−.08	.05	−.55	−.42	.12	.01
EDUCATION	.20	−.01	−.17	.05	.20	.22	.13	.07	.17	.18	.36	.50*	−.02	.19*
Organizational Position:														
STATUS	−.10	−.20	−.10	−.07	.24	.07	−.42	.09	−.19	−.40*	−.08	−.08	−.15	−.27
PARTICIPATION	.02	.02	.21	.35*	.34	.50*	−.13	.19	.20	.11	−.06	.28	.13	.03
Network Centrality:														
Sheaf Coefficient[c]	.36*	.37*	.49	.68***	.87***	.63**	.83*	.28	−.45	.42	−.47	−.54	—[b]	—[b]
R^2 Without Network Variables:	.17	.17	.16	.31	.08	.27	.17	.24	.13	.32	.23	.38	.08	.17
R^2 With Network Variables:	.28	.28	.37	.64	.54	.56	.76	.28	.18	.34	.38	.56	—[b]	—[b]

Cell entries are standardized regression coefficients.

* $p<.10$, ** $p<.05$, *** $p<.01$.

[a] All respondents in this program were white.

[b] Network measures not available in this program.

[c] Sheaf coefficient computed from betas for five centrality measures: WORK CONTACTS, INFLUENCE, RESPECT, SUPPORT, and ASSISTANCE.

and sometimes smaller, but still appreciable, proportions of the variance in ACTIVISM. The impact of Network Centrality was generally greater than any other factor and more consistent than all of the individual- or organization-level variables, with the partial exception of RACE. (Overall, being white was a handicap for community involvement, judging from the consistently negative, if not pronounced, relationships between RACE and the two dependent variables.) By comparison, the effects of the remaining variables, GENDER, EXPERIENCE, EDUCATION, STATUS and PARTICIPATION, were scattered and inconsistent. The dominant impression, therefore, is that the practitioners' locations in the networks of professional exchange that knit these programs together were more consequential for their activities outside the network boundaries than were the ascribed attributes and technical resources they brought to their work or the position or autonomy they had attained within their own agency.

For purposes of interpretation, it is useful to note that (based on the overall configurations displayed by their networks) two different administrative strategies of interagency coordination were represented among these four programs. A detailed examination (not shown here) of the sociometric data revealed that, in Programs One and Two, the grantee agency responsible for administrative coordination frequently served as intermediary for the contacts that took place between one agency and another in the program. In general, the administrators adopted a fairly formalistic or bureaucratic approach to overall program coordination as a way of shaping the network to conform to program priorities. Programs Three and Four by comparison were more personalistic; members of the central administrative agency were by no means inaccessible, but practitioners in the separate agencies also had a great deal of direct, unmediated contact with each other.[11] In other words, while all four programs displayed coherent network structures, in Programs One and Two, Network Centrality was characteristic of practitioners who were strategically placed within a formally articulated system of exchanges, while in Programs Three and Four, Centrality describes a person well-integrated into a more loosely coordinated network of comparatively less formalized exchanges. This indicates that quite different strategies of interorganizational coordination can accompany similar outcomes; the critical factor for the practitioner was to be favorably located in the exchange apparatus, whatever the shape of that apparatus might have been. However, judging from the differences in explained variance in CONTACT, the advantage of network integration was more pronounced in the two less formalized programs. This accords well with the notion of professional autonomy, which is defined, in part, by the freedom to select one's working relationships with colleagues, and it suggests a direction that might be taken in exploring how professionals interact with various "audiences" and "publics" that are important to their professional standing (Haug and Sussman, 1969; Grimm and Kronus, 1973; Blankenship, 1973).[12]

---

[11] This descriptive information on the programs is presented in detail in Miller (1978) and will not be duplicated here. The descriptions of the networks as formalistic or personalistic were substantiated by visual plots based on a smallest space analysis of the sociometric data, and by field observations and interviews with key participants. The smallest space analysis plots for Programs One and Two revealed clear "radial" or "core-periphery" patterns; ties between individuals in different agencies were channeled through the administrative center. This indicates an active effort to shape the communication channels by those in positions of responsibility for the network as a whole. This was less in evidence in Programs Three and Four. These SSA plots are available upon request.

[12] Our predominant concern has been the advantage of network centrality for community involvement. However, a subsidiary analysis explored the effects of Centrality on an index based on participants' subjective evaluations of the effectiveness of the program and their own role within it (Miller, 1978). In the two "formalistic" programs, Centrality bore little relationship to this index; it accounted for 4% of the variance in Program One and 7% of the variance in Program Two. In the two "personalistic" programs the explained variance rose to 42% in Program Three and 27% in Program Four. For comparison, in Programs Five and Six the network construct accounted for, respectively, 10% and 20% of the variance in participants' subjective estimates of effectiveness.

Results Questioning the Hypothesis

The data for Programs Five and Six do not follow a similar pattern. Here, it was clearly not the well-integrated individuals who were accounting for community-based activities but, at the same time, in neither of these two programs was the average level of CONTACT or ACTIVISM low (see Appendix B). In Program Five, all of the predictor variables combined explained only 18% of the variance in community CONTACT. Of this total, the largest increment, about 5%, was accounted for by network location, but the effect was negative (Beta = −.45). For ACTIVISM the total explained variation was greater (34%), but the network increment to the R^2, while positive, was quite small (only about 2%). We have already pointed out that there was a network of interagency connections in both of these programs before the DSO funding began. It is also significant that in Program Five many of the agencies in the network were involved in other, non-DSO programs for juvenile offenders (to which we had no access) and were answerable to a variety of state, local and federal sources, many of which also stressed the desirability of community-based activities. As a result, they were tied into multiple networks in a way that was not characteristic of the other DSO programs. The likelihood that community-based activities were already well established in these agencies before the DSO mandate was added may have obviated any additional advantage to be gained from establishing a new set of linkages among the agencies officially engaged in the DSO program. This ex post facto interpretation is plausible, if speculative, but it stresses a fundamental difference between this program and the others that we were unable to document in sufficient detail. It is not intended to obscure the fact that the prediction we made for this program was essentially incorrect: Network Centrality was of quite limited usefulness in accounting for community involvement.

No such explanation would account for the data for Program Six, where the clearly *negative* impact of Centrality accounted for 15% of the variance in CONTACT and 18% of the variance in ACTIVISM. Those who were relatively isolated from the network of professional exchange, or who, perhaps, were able to avoid it, were represented more often in ties to the community than those who were integrated into the network.[13] It is interesting that these are the only results that are consistent with Granovetter's argument that weak ties to social networks facilitate outside linkages (Granovetter, 1973). In this program, as in Program Five, the federal DSO funds were also used to supplement the budget of an existing state-run juvenile service delivery system, but there was not the same multiplicity of mandates and funding sources. It is apparent that a viable interagency network did, in fact, exist in this program (51% of all sociometric work contacts were exchanged by the members of different agencies), and the level of its community-based activities was not low compared to the other programs. But the fact that less involvement in the interagency network meant more involvement in the community suggests a division of labor that was not anticipated, either by the funding source or by the hypothesis with which we began. The analysis in Table 1 indicates that the community-based activities had devolved somewhat disproportionately upon the better educated but less experienced personnel in the program, suggesting that being freed from extensive interagency ties may have been, for some of them, a prerequisite for taking on this important function. Conversely, as one anonymous reader has suggested (following the reasons of Granovetter), it may have been that the more experienced members had interpersonal ties within the program that were stronger, if not more numerous, and that this inhibited their outreach to the community.

Without additional data, it is not clear exactly why such a division of labor between those central to the network and those relatively isolated from it evolved, especially since this division of effort was inconsistent with the reasoning that persuaded the funding source to call for interagency efforts. Since Centrality did not promote community involvement, what its advantage in the interagency network actually was is also left unexplained, though the division of activities between more and less experienced personnel, suggested before, implies the operation of a separate set of goals for those who were closely tied into the interagency network. One possibility that has been suggested[14] is that this already-established network might have been able to delegate the new community liaison activities demanded by the DSO funding source to the periphery, thus allowing the core (central) members to continue to concentrate on directly client-related activities. By contrast, in the newly forming networks (Programs One through Four) all the members were expected to deal with both sets of tasks and in such a situation a strategic network location was a definite advantage in forging links to the community. This suggestion raises some interesting possibilities because it implies that, over time, organization-to-organization and organization-to-community ties in service delivery systems may be shifted from one category of members to another, with the result that an increasingly complicated division of labor becomes visible as networks age. This in turn prompts another question about how the uses made of an interorganizational network may be subject to manipulation by different interest groups represented in the system.

---

[13] This negative relationship removes any doubt that the connection between Centrality and CONTACT may be tautological.

DISCUSSION

Summary and Implications

The programs we surveyed involved agencies bound together by professional exchanges among their members and these interagency networks were, in turn, embedded in larger multiorganizational fields. The hypothesis we examined specified that the involvement of organization members in this larger interorganizational and community environment was directly affected by their integration into the interagency structures of professional exchange, over and above the advantages that might have been gained from their own individual resources and from their positions in the status hierarchies of the specific agencies that employed them. The hypothesis was confirmed in four of the six cases examined: integration into patterns of organized activity at the interagency level was predictive of involvement in collective action at two other, more encompassing levels. In a fifth case, the hypothesis was not confirmed, and in a sixth replication, the results were the reverse of what was expected. The reasonable conclusion is that integration into a comprehensive professional network can serve as the base for developing external community linkages but with the qualification that this is not the only mechanism through which such community involvement is realized. There are alternative means for achieving this objective that we were unable to illuminate beyond the suggestion that they are directly related to the age of the network, the multiplicity—and probably order of precedence—of program goals, and the developing internal division of labor among the constituent organizations and participants, all factors that are best examined from a longitudinal, possibly evolutionary, perspective.

The parallel we drew between the multiplicity of individuals' direct and indirect ties to interorganizational networks, on one hand, and the concept of social integration, on the other, reflects our reliance on a Durkheimian framework. In *Suicide* (Durkheim, 1952), social integration was discussed in a way that could be applied to several levels of analysis. In fact, it figured in the explanation of reactions to stress in two ways: tightly integrated collectivities have low suicide rates *and* isolated individuals are more likely to commit suicide. The concept thus had both a social-structural and a social-psychological usage. In *The Division of Labor in Society* (Durkheim, 1933), a closely allied concept, solidarity, played a comparable role in the characterization of entire societies. The common idea in these otherwise different treatments was the notion of interconnecting and overlapping bonds, of various sort, that link together the parts of social systems, whether the parts are individuals or collectivities. This is a notion that finds a direct analog in the concept of social network, which also bridges many levels of sociological investigation (Granovetter, 1973; Alba and Kadushin, 1976).

---

[14] By Rolf Ziegler, University of Munich, personal communication.

There is already a strong conceptual thread in the organizational literature formed by questions involving the nature of social ties; a thread that connects studies conducted at several different levels of analysis, from studies of interpersonal relations (Blau, 1962; Katz and Kahn, 1978:602ff.); through studies of work group cohesion (Seashore, 1954); intraorganizational structure (Rice and Mitchell, 1974; Rogers and Agarwala-Rogers, 1976); interorganizational connections (Levine, 1972; Rogers, 1974; Hall el al., 1977; Bick and Miller, 1978); and organization-community relations (Turk, 1973). Moreover, thinking in terms of networks provides a common conceptual ground not just among such studies in organizational analysis, but also between them and investigations in other substantive areas, ranging from the study of friendship groups (Granovetter, 1973), to the study of communities (Laumann and Pappi, 1976; Craven and Wellman, 1973), national elites (Moore, 1979) and international systems (Snyder and Kick, 1979). The specific interests and theoretical underpinnings of research conducted in these different areas have been quite varied, but in all of them the idea of integration into networks of social ties can be brought forward and, at one level of abstraction, they can be seen to be asking a similar question in different forms. Such a diffusion of common theoretical formulations and methods within and between specialties will mean less balkanization within organizational sociology and less pronounced segregation between it and the rest of the field.

Conclusion

In weighing these findings it is essential to keep in mind the environments in which the DSO programs were established. The agencies involved were generally small, democratic, reliant upon a highly specialized work force and chronically short of resources. They were called upon to create community-based solutions for the problems of troublesome young people (themselves, members of an ambiguous category) but, beyond the requirement that these solutions be implemented by "interagency systems," very few explicit guidelines as to how they should be structured were offered, and the assembling of the programs took place in a fashion that differed from one site to another. Thinking practically, this environmental turbulence and uncertainty could militate against program success and, thinking in terms of research and theory, it could militate against clear-cut findings because of the likelihood that, given the pressure of time and the lure of fast federal money, antagonists and competitors and philosophical opposites were sometimes drawn into the same networks. Therefore, conflict over resources and strategies could be exacerbated and the necessity of maintaining a network position simply to guarantee the flow of funds could become as much a liability as an asset for effective performance. The idea that location near the center of such an interagency network would function as a professional resource for practitioners in their attempts to forge cooperative, program-relevant ties to the larger community would seem to have a fairly stringent test in such unsettled circumstances, and we expect that the patterns we observed in four out of six programs will recur frequently in future replications.

APPENDIX A. CORRELATION MATRICES (Decimals Eliminated)

Program one	1	2	3	4	5	6	7	8	9	10	11	12	13
1 GENDER	—												
2 RACE	−22	—											
3 EXPER.	−02	−08	—										
4 EDUC.	02	21	05	—									
5 STATUS	−04	03	−17	−42	—								
6 PARTIC.	03	−12	11	−10	22	—							
7 WORK CON.	18	04	16	23	−35	−01	—						
8 INFLUENCE	26	03	12	16	−26	01	78	—					
9 RESPECT	27	−09	18	31	−32	02	65	66	—				
10 SUPPORT	27	−01	16	27	−34	−06	69	82	84	—			
11 ASSIST.	15	−06	08	24	−22	−05	48	56	68	67	—		
12 CONTACT	13	−10	11	33	−29	−04	22	21	40	39	37	—	
13 ACTIVISM	18	−11	19	19	−34	−01	34	29	46	41	35	56	—

Program two	1	2	3	4	5	6	7	8	9	10	11	12	13
1 GENDER	—												
2 RACE	−06	—											
3 EXPER.	−16	−19	—										
4 EDUC.	28	02	11	—									
3 STATUS	24	−20	−27	−06	—								
6 PARTIC.	00	−10	−01	−10	18	—							
7 WORK CON.	−02	−21	−06	12	00	−53	—						
8 INFLUENCE	−16	03	−03	19	−31	−31	51	—					
9 RESPECT	−16	12	04	24	−50	−32	40	69	—				
10 SUPPORT	−22	04	−04	21	−35	−41	54	90	78	—			
11 ASSIST.	16	08	−03	31	−33	−31	32	66	78	62	—		
12 CONTACT	−03	−18	−21	−16	−03	16	11	29	12	11	24	—	
13 ACTIVISM	18	−40	−16	15	03	18	29	45	19	38	15	42	—

Program three	1	2	3	4	5	6	7	8	9	10	11	12	13
1 GENDER	—												
2 RACE	23	—											
3 EXPER.	16	13	—										
4 EDUC.	40	39	−15	—									
5 STATUS	−22	−14	−45	−06	—								
6 PARTIC.	−24	−15	−44	10	13	—							
7 WORK CON.	−09	12	−15	05	−15	26	—						
8 INFLUENCE	−09	12	−11	04	−17	27	99	—					
9 RESPECT	00	14	−25	10	−06	16	84	82	—				
10 SUPPORT	−02	12	−17	15	−12	28	72	71	87	—			
11 ASSIST.	−01	13	−13	20	−09	35	73	70	86	98	—		
12 CONTACT	−13	−22	−07	−08	11	16	38	37	46	30	29	—	
13 ACTIVISM	07	−13	39	−01	−18	07	10	12	22	16	18	60	—

Program four	1	2	3	4	5	6	7	8	9	10	11	12	13
1 GENDER	—												
2 RACE*	—	—											
3 EXPER.	25	—	—										
4 EDUC.	44	—	11	—									
5 STATUS	27	—	−32	22	—								
6 PARTIC.	12	—	−18	16	23	—							
7 WORK CON.	−16	—	05	−02	−29	08	—						
8 INFLUENCE	−14	—	05	09	−22	11	93	—					
9 RESPECT	−06	—	16	−17	−56	−12	79	76	—				
10 SUPPORT	−19	—	−17	−19	−08	−18	65	66	67	—			
11 ASSIST.	04	—	−04	−03	−31	28	70	61	68	24	—		
12 CONTACT	−23	—	−12	−12	−24	−27	51	33	36	61	10	—	
13 ACTIVISM	−13	—	−45	−01	12	24	05	00	−07	00	09	60	—

* All respondents white

Program five	1	2	3	4	5	6	7	8	9	10	11	12	13
1 GENDER	—												
2 RACE	−15	—											
3 EXPER.	21	02	—										
4 EDUC.	15	−04	−06	—									
5 STATUS	−29	09	13	−50	—								
6 PARTIC.	−17	−18	−07	−07	17	—							
7 WORK CON.	−04	25	−25	06	−02	−03	—						
8 INFLUENCE	−02	27	−25	07	−02	−06	99	—					
9 RESPECT	−02	27	−25	08	−03	−05	97	99	—				
10 SUPPORT	−03	27	−26	06	01	−07	97	99	97	—			
11 ASSIST.	−04	28	−26	10	−03	−07	97	97	98	96	—		
12 CONTACT	−06	−11	−07	27	−22	15	−07	−07	−06	−06	−03	—	
13 ACTIVISM	23	−15	07	41	−47	03	04	05	05	02	06	57	—

Program six	1	2	3	4	5	6	7	8	9	10	11	12	13
1 GENDER	—												
2 RACE	−11	—											
3 EXPER.	11	26	—										
4 EDUC.	27	06	07	—									
5 STATUS	−07	−20	11	−55	—								
6 PARTIC.	−40	−01	−27	−02	−18	—							
7 WORK CON.	−05	21	02	42	−48	29	—						
8 INFLUENCE	00	20	04	36	−49	23	86	—					
9 RESPECT	−04	22	00	39	−44	22	86	81	—				
10 SUPPORT	−19	02	−69	00	−36	41	48	53	52	—			
11 ASSIST.	−27	04	−64	05	−29	41	56	54	57	94	—		
12 CONTACT	13	−18	−35	23	−15	−05	−22	−24	−25	01	−04	—	
13 ACTIVISM	35	12	−16	46	−33	08	−08	−06	−12	−03	−11	76	—

*Program Seven**

	1	2	3	4	5	6	7	8
1 GENDER	—							
2 RACE	07	—						
3 EXPER.	09	−05	—					
4 EDUC.	30	11	25	—				
5 STATUS	−14	−09	35	25	—			
6 PARTIC.	−16	−03	07	11	08	—		
7 CONTACT	00	08	18	08	19	14	—	
8 ACTIVISM	10	12	17	28	32	06	32	—

* Network measures not available.

APPENDIX B. MEANS AND STANDARD DEVIATIONS FOR COMMUNITY CONTACT AND COMMUNITY ACTIVISM IN SEVEN DSO PROGRAMS

	CONTACT[a]		ACTIVISM[b]	
	Mean	Std. Dev.	Mean	Std. Dev.
Program One (N=76)	15.8	5.8	11.9	6.3
Program Two (N=37)	16.7	6.1	10.2	5.9
Program Three (N=36)	17.1	5.8	9.5	4.1
Program Four (N=20)	13.8	6.8	9.7	4.8
Program Five (N=57)	15.5	6.1	13.0	6.8
Program Six (N=35)	15.6	7.6	10.0	6.5
Program Seven (N=89)	14.1	4.1	9.9	5.3

[a] A six-item index; possible range 6–42. Alpha=.74.

[b] A four-item index; possible range 4–28. Alpha=.88.

APPENDIX C: STATISTICAL SUMMARIES FOR MEASURES OF INDIVIDUAL RESOURCES, ORGANIZATIONAL POSITION AND NETWORK LOCATION IN SEVEN DSO PROGRAMS

	Program One (76)	Program Two (37)	Program Three (36)	Program Four (20)	Program Five (57)	Program Six (35)	Program Seven (89)
Percent White	72%	83%	95%	100%	61%	77%	82%
Percent Male	40%	28%	47%	40%	42%	31%	48%
Mean Education (s.d.)	16.1	16.9	17.3	17.3	16.1	16.3	17.3
	(2.2)	(1.6)	(1.7)	(2.1)	(2.9)	(2.2)	(2.0)
Mean Experience (s.d.)	4.1	6.8	6.5	7.2	5.5	5.4	6.0
	(4.4)	(5.3)	(4.7)	(6.4)	(2.9)	(5.4)	(6.0)
Percent Supervisory	41%	38%	25%	10%	35%	31%	35%
Percent Participatory	88%	59%	80%	77%	79%	80%	76%
Mean Centrality Scores* (s.d.)							
Work Contacts	105.7	52.4	49.8	25.9	92.9	56.5	—
	(6.0)	(15.2)	(6.1)	(8.8)	(18.9)	(10.5)	
Influence	112.8	54.4	61.5	25.1	87.8	55.0	—
	(5.0)	(9.9)	(4.2)	(7.7)	(17.4)	(7.5)	
Respect	133.0	58.5	58.5	25.6	89.5	55.7	—
	(5.5)	(9.5)	(4.1)	(10.4)	(17.9)	(6.5)	
Support	111.9	51.5	60.4	24.4	87.5	54.8	—
	(4.3)	(12.0)	(3.7)	(4.1)	(17.0)	(11.1)	
Assistance	124.5	52.5	57.6	22.2	92.2	52.5	—
	(5.1)	(4.7)	(4.0)	(6.9)	(19.1)	(11.1)	

* Not standardized for program size; not available for program seven.

REFERENCES

Alba, Richard D., and Charles Kadushin
1976 "The intersection of social circles: a new measure of proximity in networks." Sociological Methods and Research 5:77–102.

Aiken, Michael
1974 "Organizational environment, permeability and organizational structure: a study of forty-four local governments in Belgium." Paper presented at the meeting of the Eighth World Congress of Sociology, Toronto, Canada.

Aiken, Michael, and Jerald Hage
1968 "Organizational interdependence and intraorganizational structure." American Sociological Review 33:912–31.

Aldrich, Howard
1971 "Organizational boundaries and interorganizational conflict." Human Relations 24:279–93.

Aldrich, Howard, and Jeffrey Pfeffer
1975 "Environments of organizations." Pp. 79–104 in Annual Review of Sociology. Palo Alto: Annual Reviews, Inc.

Anderson, R. C.
1967 "A sociometric approach to the analysis of interorganizational relationships." Paper presented at the meeting of the American Sociological Association, San Francisco.

Barth, Ernest A. C.
1963 "The causes and consequences of interagency conflict." Sociological Inquiry 33:51–7.

Benson, J. Kenneth
1975 "The interorganizational network as a political economy." Administrative Science Quarterly 20:229–49.

Bick, Wolfgang, and Paul J. Müller
1978 "Stable patterns within a network of urban bureaucracies: domains or positions?" Paper presented at the annual meeting of the American Sociological Association, San Francisco.

Blankenship, Ralph
1973 "Organizational careers: an interactionist perspective." Sociological Quarterly 14:88–98.

Blau, Peter
1955 Dynamics of Bureaucracy. Chicago: University of Chicago Press.
1962 "Patterns of choice in interpersonal relations." American Sociological Review 27:41–55.
1977 Inequality and Heterogeneity: A Primitive Theory of Social Structure. New York: Free Press.

Burt, Ronald S.
1976 "Positions in networks." Social Forces 55:93–122.
1977 "Positions in multiple network systems, part two: stratification and prestige among elite decision makers in Altneustadt." Social Forces 56:551–75.

Coates, Robert B.
1974 "Community-based corrections: concept, impact, dangers." Paper presented at the Massachusetts Standards and Goals Conference, Boston.

Craven, Paul, and Barry Wellman
1973 "The network city." Sociological Inquiry 43:57–88.

Durkheim, Emile
1933 The Division of Labor in Society. New York: Macmillan.
1938 The Rules of Sociological Method. Glencoe: Free Press.
1952 Suicide. London: Routledge and Kegan Paul.

Etzioni, Amitai
1961 Complex Organizations: A Sociological Reader. New York: Holt, Rinehart and Winston.

Evan, William
1976 "The organizational set." Chapter 9 in Organizational Theory: Structures, Systems and Environments. New York: Wiley.

Freeman, Linton
1977 "A set of measures of centrality based on between-ness." Sociometry 43:35–41.

Fry, Lincoln J., and Jon Miller
1975 "Responding to skid row alcoholism: self-defeating arrangements in an innovative treatment program." Social Problems 22:675–88.

Granovetter, Mark S.
1973 "The strength of weak ties." American Journal of Sociology 78:1360–80.
1974 Getting a Job: A Study of Contacts and Careers. Cambridge: Harvard University Press.

Grimm, James, and Carol Kronus
1973 "Occupations and publics." Sociological Quarterly 14:68–87.

Haas, J. Eugene, and Thomas Drabek
1973 Complex Organizations: A Sociological Perspective. New York: Macmillan.

Hall, Richard H., John P. Clark, Peggy C. Giordano, Paul V. Johnson, and Martha Van Roekel
1977 "Patterns of interorganizational relationships." Administrative Science Quarterly 22:457–74.

Harary, Frank, Robert L. Norman, and Dorwin Cartwright
1965 Structural Models: An Introduction to the Theory of Directed Graphs. New York: Wiley.

Hasenfeld, Yeheskel
1972 "People processing organizations: an exchange approach." American Sociological Review 37:256–63.

Haug, Marie, and Marvin Sussman
1969 Professionals and the public." Sociological Inquiry 39:57–64.

Heise, David R.
1972 "Employing nominal variables, induced variables and block variables in path analysis." Sociological Methods and Research 1:147–73.

Holland, Paul W., and Samuel Leinhardt
1973 "The structural implications of measurement error in sociometry." Journal of Mathematical Sociology 3:85–111.
Horwitz, Allan
1977 Social networks and pathways to psychiatric treatment." Social Forces 56:86–105.
Katz, Daniel, and Robert Kahn
1978 "The Social Psychology of Organizations. 2nd ed. New York: Wiley.
Klein, Malcolm (ed.)
1976 The Juvenile Justice System. Beverly Hills: Sage.
Kluegel, James R.
1978 "The causes and costs of racial exclusion from job authority." American Sociological Review 43:285–301.
Knoke, David, and David L. Rogers
1978 "A blockmodel analysis of interorganizational networks." Paper presented at the annual meeting of the American Sociological Association, San Francisco.
Laumann, Edward O., and Franz U. Pappi
1976 Networks of Collective Action: A Perspective on Community Influence. New York: Academic Press.
Levine, Joel
1972 "The sphere of influence." American Sociological Review 37:14–27.
Lincoln, James, and Jon Miller
1979 "Work and friendship ties in organizations: a comparative analysis of relational networks." Administrative Science Quarterly 24:181–99.
Lincoln, Suzanne B.
1979 "Agency resource dependency and client recidivism: organizational factors in the diversion of juvenile offenders." Ph.D. dissertation, Dept. of Sociology, University of Southern California.
Miller, George
1967 "Professionals in bureaucracy: alienation among industrial scientists and engineers." American Sociological Review 32:755–67.
Miller, Jon
1978 The Organizational Properties of Seven Programs for the Deinstitutionalization of Status Offenders. Laboratory for Organizational Research, University of Southern California.
Miller, Jon, Sanford Labovitz, and Lincoln Fry
1975 "Inequities in the organizational experiences of women and men: resources, vested interests and discrimination." Social Forces 54:365–81.
Miller, Jon, and H. Edward Ransford
1978 "Inequality in the military: implications for organizations, occupational mobility and social stratification." Journal of Political and Military Sociology 6:65–74.
Miller, Jon, James Lincoln, and Jon Olson
1980 "Rationality and equity in professional networks: sex and race as factors in the stratification of interorganizational systems." Paper presented at the annual meeting of the Southwestern Social Science Association, Houston.
Moore, Gwen
1979 "The structure of a national elite network." American Sociological Review 44:673–91.
Pennings, Johannes
1976 "Dimensions of organizational influence and their effectiveness correlates." Administrative Science Quarterly 21:688–99.
Perrow, Charles
1979 Complex Organizations: A Critical Essay. 2nd ed. New York: Scott, Foresman.
Perucci, Robert, and Mark Pilisuk
1970 "Leaders and ruling elites: the interorganizational bases of community power." American Sociological Review 35:1040–57.
Reid, William
1964 Delinquency prevention and organizational relations." Social Science Review 38:418–528.
Rice, Linda E., and Terence R. Mitchell
1974 "Structural determinants of individual behavior in organizations." Administrative Science Quarterly 18:56–70.
Rogers, David L.
1974 "Sociometric analysis of interorganizational relations: association of theory and measurement." Rural Sociology 39:486–503.
Rogers, Everett M., and Rekha Agarwala-Rogers
1976 Communication in Organizations. New York: Free Press.
Saranson, Seymour, Charles Carol, Kenneth Maton, Saul Cohen, and Elizabeth Lorentz
1977 Human Services and Resource Networks. San Francisco: Jossey-Bass.
Seashore, Stanley
1954 Group Cohesiveness in the Industrial Work Group. Ann Arbor: Survey Research Center.
Seashore, Stanley, and Ephraim Yuchtman
1967 "Factorial analysis of organizational performance." Administrative Science Quarterly 12:377–95.
Shaw, Marvin E.
1964 "Communication networks." Pp. 111–49 in Leonard Berkowitz (ed.), Advances in Experimental Social Psychology. New York: Academic Press.
Simmel, Georg
1955 Conflict and the Web of Group Affiliation. Glencoe: Free Press.
Simpson, Richard L., and William H. Gulley
1962 "Goals, environmental pressures and organizational characteristics." American Sociological Review 27:344–51.
Snyder, David, and Edward L. Kick
1979 "Structural position in the world system and economic growth: a multiple-network analysis of transnational interactions." American Journal of Sociology 84:1096–126.
Spergel, Irving A.
1976 "Interactions between community structure, delinquency and social policy in the

inner city." Pp. 55–99 in Malcolm Klein (ed.), The Juvenile Justice System. Beverly Hills: Sage.

Stern, Robert N.
1979 "The development of an interorganizational control network: the case of intercollegiate athletics." Administrative Science Quarterly 24:242–67.

Strauss, George, and E. Rosenstein
1970 "Worker participation: a critical view." Industrial Relations 9:197–214.

Turk, Herman
1970 "Interorganizational networks in urban society: initial perspectives and comparative research." American Sociological Review 35:1–9.
1973 "Comparative urban structure from an interorganizational perspective." Administrative Science Quarterly 18:37–55.

Yuchtman, Ephraim, and Stanley Seashore
1967 "A system resource approach to organizational effectiveness." American Sociological Review 32:891–903.

RESEARCH NOTE

BEYOND MISSPECIFICATION IN SOCIOLOGICAL ANALYSES OF INCOME INEQUALITY *

ERICH WEEDE
University of Cologne, West Germany

American Sociological Review 1980, Vol. 45 (June:497–501)

Since the mid-70s, there has been some debate in the *ASR* about correlates of the size distribution of income in cross-national analysis. According to previous research, economic development seems to have little impact, whereas state strength or dependency, and democracy are more useful predictors of income inequality. Here it is argued that much sociological research rests on misspecification of the relationship between economic development and income inequality, and therefore needs reanalysis rather than uncritical acceptance. A partial reanalysis of previous studies, avoiding specification error, first demonstrates a moderately strong curvilinear relationship between economic development and inequality; second, it casts considerable doubt on state strength or little dependency as equalizers of income; third, it reconfirms a cross-sectional correlation between democracy and less inequality, even after proper controls for economic development.

Recently, there has been much interest in determinants (or cross-sectional correlates) of the size distribution of income. Economists and econometricians generally agree that the size distribution of income is a curvilinear function of the level of economic development, as has been suggested by Kuznets (1963; 1976) and empirically supported by Ahluwalia (1976), Chenery and Syrquin (1975), and Paukert (1973). Income inequality is fairly moderate at extremely low levels of economic development, reaches a maximum at intermediate levels of development, and finally decreases. The specification of the relationship used by Ahluwalia (1976) or by Chenery and Syrquin (1975) is a quadratic polynomial regression of inequality on economic development, after indicators of the latter have been logarithmically transformed, for example:

$$\text{Gini income distribution} = b_0 + b_1 (\ln \text{GNPC}) + b_2 (\ln \text{GNPC})^2 + E. \quad (1)$$

Research published in the *ASR* (Rubinson, 1976; Rubinson and Quinlan, 1977; Stack, 1978) argues that state strength variables—such as high government revenue to GDP or low trade to GDP ratios—or democracy are more powerful independent variables than is the level of economic development. However, such results rest on misspecification of the relationship between economic development and income inequality. While Rubinson (1976) or Rubinson and Quinlan (1977) regressed indicators of inequality on the logarithm of energy consumption per capita, Stack (1978) preferred gross domestic product per capita. None of them ever tried second order polynomial regression. The purposes of this research note are, first, to demonstrate the superiority of the economist's specification over other specifications, and second, a reconsideration of the effects of state strength and, to a much lesser degree, of democracy on the size distribution of income.

As this note is a critique of the work done by Rubinson (1976), Rubinson and Quinlan (1977), and Stack (1978), I work with the same sample of 47 nations as Rubinson (1976:657) does. Rubinson and Quinlan (1977), as well as Stack (1978),

---

* Address all communications to: Erich Weede; Forschungsinstitut für Soziologie; Universität zu Köln; Lindenburger Allee 15; 5 Köln 41; West Germany.

I would like to thank Bernd Scheuvens and Horst Tiefenbach for their assistance in producing this research note.

rely on subsamples. Moreover, I take quintile shares of personal income and Gini *circa* 1965 from Paukert (1973:114–5), again as was done in the work considered here. In accordance with Rubinson (1976) or Rubinson and Quinlan (1977) energy consumption per capita (ENPC) is chosen as indicator of economic development. My data source is Taylor and Hudson (1972:326–8).

As Table 1 demonstrates, the econometric or "proper" specification of the relationship between economic development and income shares or Gini is consistently superior to other specifications. Moreover, using ln ENPC only as a linear predictor of income inequality, as Rubinson (1976) and Rubinson and Quinlan (1977) did, is a particularly poor specification. This specification comes out worst from a fit point of view in 5 out of 6 equations. Neither adjusting percentages of variance explained for differences in degrees of freedom between equations nor substitution of GNPC for ENPC affects this conclusion (Weede and Tiefenbach, n.d.). Therefore, there is ample reason not to accept uncritically results concerning the beneficial impact of strong states produced by Rubinson (1976), but instead to test whether they survive respecification of the relationship between economic development and inequality.

From among his indicators of state strength, I chose only two, that is, government revenue as a percent of GDP for domestic state strength (or for state control of domestic economic activities), and trade as a percent of GNP for state strength vis-à-vis the international environment. Admittedly, this selection of independent variables has been influenced by the availability of data. Nevertheless, it is also defensible by other arguments. First, government revenue as a percentage of GDP is used in most regressions reported by Rubinson (1976) or by Rubinson and Quinlan (1977), while other independent variables play a much less central role. Second, imports and exports as a percentage of GDP had extremely similar effects on indicators of inequality in Rubinson's work (1976:654). Stack (1978:271), too, noted that both of them are related to inequality. Therefore, it seems plausible to combine imports and exports. The substitution of trade as a percentage of GNP for trade as a percentage of GDP is a matter of convenience which should not make much difference. While such a selective attitude towards independent variables used in the research considered here prohibits a recalculation of each and every detail, it should suffice to evaluate the merit of the central conclusions. Moreover, given 47 observations—sometimes less, due to missing values—it makes little sense to use too many independent variables.

There is one important detail where my design parts company with Rubinson's (1976) and with Rubinson and Quinlan's (1977). They time-matched independent variables and inequality data and therefore had to include the year as an independent variable. While such a strategy is not without merit, it is not the standard solution either. World Bank econometricians Ahluwalia (1976), and Chenery and Syrquin (1975) preferred to match the inequality data of *circa* 1965 with the independent variables for 1965, or the closest year available, in spite of the fact that the time-spread for inequality data is somewhat wider than for other variables. As I see the costs of doing it in Rubinson's way much more clearly than the benefits to be derived therefrom, I took *circa* 1965 data

Table 1. Percentages of Variance Explained in Income Shares or Gini by Economic Development under Different Specifications (N=47)

	Q_1 bottom 20%	Q_2	Q_3	Q_4	Q_5 top 20%	Gini
Energy Consumption per Capita (ENPC)	2.4	10.0	32.4	30.2	21.8	12.3
ln ENPC	9.3	2.9	21.9	20.6	10.4	3.9
ENPC, ENPC^2	7.0	10.1	35.6	35.9	22.8	12.3*
ln ENPC, $(\ln \text{ENPC})^2$	12.5	14.5	38.0	40.0	30.1	18.6

* Where the F-level was insufficient to warrant inclusion of ENPC^2 in this equation.

Table 2. State Strength as a Determinant of Inequality under Proper Specification (N=43)

	Q_1 bottom 20%	Q_2	Q_3	Q_4	Q_5 top 20%	Gini
ln Energy Consumption Per Capita Expressed in Kilograms of Coal	−1.27	−2.33	−4.25	−6.67	+14.30	+0.11
Equivalents (ln ENPC)	0.39	0.19	0.04	0.01	0.02	0.09
(ln ENPC)²	+0.058	+0.18	+0.41	+0.65	−1.27	−0.0094
	0.65	0.24	0.02	0.00	0.02	0.10
Beta Weight of b_1 (ln ENPC) + b_2 (ln ENPC)²	0.47	0.22	0.47	0.68	0.42	0.27
Government Revenue as	+0.035	+0.082	+0.056	−0.038	−0.13	−0.0022
a % of GDP	0.51	0.21	0.45	0.64	0.54	0.37
	+0.15	+0.30	+0.15	−0.09	−0.13	−0.21
Trade as a % of GNP	−0.019	−0.026	−0.026	−0.027	+0.099	+0.0011
	0.25	0.20	0.25	0.29	0.14	0.14
	−0.19	−0.21	−0.16	−0.14	+0.22	+0.24
Adjusted Percentage of Variance Explained	10	6	30	36	22	10
Percentage of Variance Explained	18	15	36	42	29	19

Where first cell entries are unstandardized coefficients, second cell entries are significance levels, and third cell entries are standardized coefficients (beta weights).

for government revenue as a percentage of GDP from Chenery and Syrquin (1975:188–91), and trade as a percentage of GNP from Taylor and Hudson (1972:372–7). My intention to do without time-matching also derives from Rubinson's report that time is insignificant when a time variable is included in the regressions after time-matching. Unfortunately, Chenery and Syrquin (1975) do not report government revenue for four nations which are part of my sample. I took this as an implicit but negative comment on availability, reliability, or comparability of those data and accepted the corresponding loss of four observations.[1]

Any deviation from Rubinson's (1976) design is a possible source of different results. Therefore, I first reproduced the major results of Rubinson, using his specifications with my somewhat different data set. As I could by and large do so, it makes sense to rule out differences in time-matching, data-sources, or number of cases as determinants of different solutions. Table 2 reports the impact of state strength and economic development on income inequality under proper specification.

If one compares the last rows of Tables 1 and 2, one finds that percentages of variance explained by economic development alone (in proper specification) are nearly as high as those explained by economic development *and* state strength. Because of the minor difference in the number of observations, economic development alone can do even better in two equations of Table 1 than economic development plus state strength do in Table 2. Therefore, state strength, as indicated by high government revenue/GDP and low trade/GNP, seems to have little impact. This suspicion is confirmed when we look at the significance levels[2] of regression coefficients. Rubinson's (1976) master indicator of state strength, that is, government revenue as a percentage of GDP, never comes even close to 10% significance. While domestic state strength makes little difference, state strength vis-à-vis the international environment has more impact. At least coefficients come close to significance at the 10% level in 2 out of 6 equations. Although even trade as a percentage of GNP performs quite poorly in Table 2, believers in dependence theories may point to foreign trade dependence increasing inequality by adding to the share of the top quintile and subtracting from the shares of all other income groups.

---

[1] The nations are: Argentina, El Salvador, Gabon, and the United States.

[2] In cross-national analysis, we almost never work with random samples. Therefore, no strong argument for use of significance levels can be made. Nevertheless, they are conventionally used for lack of a better alternative.

Economic development performs as it should for the third, fourth, and fifth quintiles, as well as for Gini. Coefficients for both the simple and the squared terms are significant at the 10% level, but are of opposing signs, as they should be. In the long run, economic development helps the third and fourth quintiles and hurts the most privileged quintile, thereby decreasing inequality. However, its impact on the two bottom quintiles is negligible. In polynomial regression, it makes little sense to report or interpret familiar standardized regression coefficients.[3] Nevertheless, one would like to compare the overall impact of economic development with that of domestic as well as international state strength. That is why the standardized coefficients of an additive function of ln ENPC and its square are provided, using the unstandardized coefficients reported in Table 2 as weights. These coefficients should be compared with the absolute values of the standardized coefficients for government revenue/GDP or trade/GNP. In the last four equations—the first two do not deserve interpretation for lack of significance—economic development always has a stronger impact than either of the state strength indicators. As far as the equations accounting for the third, fourth and fifth quintiles are concerned, the superior explanatory power of economic development over either of the state strength variables is as clear-cut as a critic of Rubinson's (1976) approach might hope for.

To summarize the results of the above reanalysis: First, Rubinson's claims concerning the beneficial impact of state control over the economic activities of its citizens are unwarranted and should be rejected, or at least no longer supported by reference to Rubinson's (1976) paper. Unfortunately, even authors (Bornschier and Ballmer-Cao, 1979:501) who avoid misspecification of the relationship between economic development and income inequality in their own work, approvingly quote Rubinson (1976) and thereby create the impression that misspecification does not matter, as if there were a cumulative body of literature supportive of some kind of dependency approach to income inequality.[4] Second, Rubinson's claims concerning the inegalitarian impact of dependence on external markets might deserve further testing because of only weak evidence in the expected direction, but the degree of support has been much exaggerated in Rubinson's paper. Third, while no cross-sectional regression analysis can ever prove that economic development leads to a more equal distribution of income in the long run—unfortunately, after a period of worsening of the income distribution—this analysis, in marked contrast to Rubinson's (1976:656), at least does not contradict "the widely held view that increasing economic . . . development will necessarily lead toward an equalization of incomes within states."[5]

For reasons of space, my discussion of the work of Rubinson and Quinlan (1977) or Stack (1978) must be extremely short. While they disagree with each other on a number of important points, they do agree on some association between democracy and income equality that is neither spurious nor produced by economic development. As neither Rubinson and Quinlan (1977) nor Stack (1978) properly controlled for economic development, since both studies misspecified the relationship between economic development and income inequality, their results cannot be admitted as positive evidence. A replication of these studies properly controlling for economic development, however, does not result in a major revision of their reported findings, as far as the association of democracy and income equality is concerned.

---

[3] Familiar standardized regression coefficients for ln ENPC and its square in Table 2 would run as high as 3.52. Such results are due to extreme multicollinearity (0.99) between ln ENPC and its square. However, none of the coefficients or significance levels concerning state strength nor variance explained is affected thereby. To demonstrate this, one may subtract the mean of ln ENPC from all such values and enter this new variable as well as its square in the polynomial regression. This brings multicollinearity down from +0.99 to −0.21 without affecting variance explained, as well as unstandardized or standardized regression coefficients of state strength variables. Therefore, the major conclusions of this paper are unaffected by this problem.

[4] One more such paper suffering from specification error and published in the *ASR* is Chase-Dunn (1975).

[5] In this context, "necessarily" is quite an unfortunate choice of words.

REFERENCES

Ahluwalia, Montek S.

1976 "Income distribution and development: some stylized facts." American Economic Review 66:128–35.

Bornschier, Volker and Thanh-Huyen Ballmer-Cao

1979 "Income inequality: a cross-national study of the relationships between MNC-penetration, dimensions of the power structure and income distribution." American Sociological Review 44:487–506.

Chase-Dunn, Christopher

1975 "The effects of international economic dependence on development and inequality." American Sociological Review 40:720–38.

Chenery, Hollis and Moises Syrquin

1975 Patterns of Development 1950–1970. London: Oxford University Press.

Kuznets, Simon

1963 "Quantitative aspects of the economic growth of nations. VIII: the distribution of income by size." Economic Development and Cultural Change 11:1–80.

1976 Modern Economic Growth: Rate, Structure and Spread. New Haven: Yale University Press.

Paukert, Felix

1973 "Income distribution at different levels of development: a survey of evidence." International Labour Review 108:97–125.

Rubinson, Richard

1976 "The world economy and the distribution of income within states: a cross-national study." American Sociological Review 41:638–59.

Rubinson, Richard and Dan Quinlan

1977 "Democracy and social inequality: a reanalysis." American Sociological Review 42:611–23.

Stack, Steven

1978 "Internal political organization and the world economy of income inequality." American Sociological Review 43:271–2.

Taylor, Charles and Michael C. Hudson

1972 World Handbook of Political and Social Indicators II. New Haven: Yale University Press.

Weede, Erich and Horst Tiefenbach

n.d. "Alternative approaches to the size distribution of income." Unpublished manuscript.

COMMENTS

DARWIN, SOCIOBIOLOGY AND SOCIOLOGY*

(COMMENT ON QUADAGNO, *ASR*, FEBRUARY, 1979)

Quadagno's thoughtful and scholarly article (1979:100–9) on paradigms in evolutionary theory reaches the conclusion that sociobiology is not applicable to human social behavior because of internal logical flaws, methodological inadequacies, and the complexity of human behavior. But Quadagno's understanding of sociobiology is fundamentally flawed and her conclusions should not be accepted. I will not discuss every problem I find in Quadagno's paper, but will confine myself to four issues: the relationship between sociobiology and Darwin's theory of evolution by natural selection, the charge that sociobiology is tautologically useless, the internal consistency of sociobiology with respect to the models for the evolution of altruism, and the applicability of sociobiology to humans.

Darwin and Sociobiology

First, Quadagno is, in fairness, to be complimented for her treatment of the relationship of Darwin's theory to earlier theories of evolutionary change. In particular, she sees that Darwin rejected the ideas of progress and direction present in other evolutionary formulations. What is startling, and to me incredible, is the charge that sociobiology has reintroduced these ideas. This reintroduction, Quadagno (1979:103) alleges, takes place with the introduction of the notion of reproductive maximization. This is said to change natural selection from "a minimum principle, becoming positive when any relative advantage in breeding is produced, to a maximizing one." This is not true.

The basic view of natural selection held by sociobiologists is simply a restatement of Darwin with the addition of Mendelian genetics. The thesis is simple and obvious: Genes produce organisms, in the sense that they carry the blueprints (from which the physical organisms will be constructed) and more or less explicit instructions—software, if you will—about how the organism is to act during its lifetime. Different sets of genes produce different models of the organism and, of course, each individual has particular life experiences that further influence its behavior. Some individuals act in such a way that they leave behind them, in succeeding generations, more copies of the genes that produced them than are left behind by other individuals. These genes become proportionally more common in the population, and evolutionary change takes place. This is simply Darwin's minimal principal, restated in genetic terms.

In restating Darwin's theory of evolution in terms of natural selection at the gene level, sociobiologists have argued that natural selection favors organisms that leave behind more genes than others, and that this selection results in organisms that act as if their goal were to maximize their reproductive success under the given ecological constraints (Greene et al., 1979). Quadagno has *understood* sociobiologists to be saying that natural selection is purposeful, when what they *are* saying is that natural selection produces organisms whose behavior is purposeful.

It is not natural selection that is operating to maximize inclusive fitness, it is the individuals produced by natural selection (see, especially, Dawkins, 1976). To suggest that natural selection operated purposefully would be teleological, involving a cause in the future. This is not being suggested; natural selection remains a passive, minimal process, ridden with blindness and chance. Not so the organisms that result from natural selection; they behave purposefully. No teleology is involved in suggesting that organisms behave purposefully. Mechanistic explanations of purposive behavior are possible, and the world about us is rife with apparently purposive automata ranging from thermostats to guided missiles (Rosenblueth et al., 1943).

Tautology

Quadagno (1979:104) charges that sociobiology contains an inevitable circularity, and is therefore useless as a theory.

> Fit behaviors are selected for, while nonfit behaviors are selected against. Thus, if a given behavior is exhibited by a species and typical of that species, it must be fit. There is an obvious tautology to this argument that was not present in the original concept of fitness.

---

* Address all correspondence to: Charles J. Morgan; 3835 46th Avenue NE, Seattle, WA 98105. I thank P. Greene, M. Linial, K. Valenzuela, and S. Macy for comments.

Occasionally somebody will slip into this tautology. But even tautological statements can sometimes be useful—as Russell (1971:309) pointed out, all of mathematics can be viewed as a tautology. More importantly, when the definitions are coupled with observations of particular situations it becomes possible to make specific predictions. Here is Wilson's (1975:67) discussion of the problem, taken from the page cited by Quadagno:

> Discussion of evolution by natural selection often seems initially circular: the fitter genotypes are those that leave more descendants . . . the genotypes that leave more descendants have greater Darwinian fitness.

But the circularity is broken when we observe that changes in gene frequency are determined by the relationship of the organism to its environment:

> Under natural conditions . . . variations in competence can stem from many causes: different abilities in direct competition with other genotypes; differential survival under the onslaught of parasites, predators, and changes in the physical environment; variable ability to penetrate new habitats; and so forth. (Wilson, 1975:67)

Fitness and adaptation are not simply defined after the fact—one looks at the material relationship of the organism to its surrounding environment.

Internal Consistency and Altruism

As Quadagno notes, the problem of apparently altruistic behavior is critical to sociobiological theory. How could a gene or genes spread if they cause the bearer, the organism they made, to make some sacrifice for the benefit of another organism? Would not the genes of the *recipient* be the ones more likely to be seen in the next generation? If it is true that behavior is subject to natural selection then there must be hidden payoffs for apparent altruism. The first problem is to develop models of what these hidden payoffs might be. The second problem is to use these models to make further predictions. The third problem is to test these predictions.

Two models that point out possible hidden payoffs for apparent altruism have been developed. The *kin selection* model points out that genes for apparent altruism can become more common if the altruism is extended to individuals whose probability of carrying these genes exceeds the relative cost to the altruist of extending the altruism, and the *reciprocal altruism* model shows that genes for apparent altruism can spread if the apparently altruistic act is likely to be repaid at a later date. Quadagno's treatment of the two models would be amusing if the issues were not so serious. She claims "the two theories combined provide a perfect example of the Fallacy of Affirming the Consequent" (p. 106) because "There are almost no acts of altruism that cannot be explained as adaptive" (p. 107); dismisses the theories as "contradictory," anyway; misquotes Wilson on the evidence for the existence of reciprocal altruism in nonhumans; and finishes with a methodological disquisition that warrants separate discussion.

Let us start with the charge, originally presented by the politically motivated Sociobiology Study Group of Science of the People and then echoed by Sahlins (1977), that the theories can explain anything. It is a matter of common experience that apparent altruism is sometimes shown toward both kin and nonkin. In the absence of the reciprocal altruism model, the critics of sociobiology could (and I suspect would) point to the occurrence of altruism directed toward nonkin and say, "See, that flies in the face of your theory." Given that sociobiologists have a plausible hypothesis to explain how there *might* be hidden payoffs in apparent altrusim toward nonkin, they say, "See, sociobiology can be used to explain anything." One does not test a theory by setting for it hypothetical challenges and then condemning the theory whether or not it is able to produce a plausible explanation of how the event *might* have taken place. One tests a theory, as indicated earlier, by deducing implications and then collecting relevant data under controlled conditions.

Testable predictions follow easily from the two models. For instance:

1. Other things being equal, more apparent altruism will be shown toward kin, genetically defined, than toward nonkin.
2. Other things being equal, especially *degree* of kinship, the more likely an individual is to reciprocate, then the more likely others are to extend apparent altruism to him/her.
3. Other things being equal, the more valuable a person's help is likely to be if it is extended, the more likely others are to extend apparent altruism toward them.

Again, the point here is that testable predictions *do* follow from the models.

The charge of internal contradiction would be devastating if it were true. But the models for the evolution of apparent altruism are not contradictory. They are complementary. I base this defense not on elaborate logical analysis, but simply on the definitions of the words. *Webster's Seventh Collegiate Dictionary,* the one I happen to have on my desk, contains these definitions: "Contradictory . . . a proposition so related to another that if either is true or false the other is false or true"; and "Complementary . . . serving to fill out or complete . . . mutually supplying each other's lack." The kin selection and reciprocal altruism models are clearly complementary, not contradictory.

Finally, Quadagno (p. 107) cites Wilson as her authority for the assertion that "there are no animal examples of reciprocal altruism." This is an egregious misstatement. On the cited page Wilson (1975:120) does say that animal behavior is "almost devoid" of reciprocal altruism, but Wilson speaks from the perspective of an entomologist. In the same paragraph he gives examples of reciprocity in *six* different *Mammalian genera.*[1] Quadagno uses her misquote of Wilson as a basis for arguing that human reciprocal altruism is entirely social. Even had she not been misquoting Wilson, her argument assumes that behavior can be assigned to genetic *or* social causes, and would not be entertained by a sociobiologist.

The "Method" of Sociobiology

Following her misrepresentation of Wilson, Quadagno (p. 107) writes:

> Since the examples of reciprocal altruism consistent with genetic theory only come from human behavior, it seems more logical to suppose that in actuality, there is no genetic basis at all; it is entirely social. Further, if it is true as Wilson notes that there are no animal examples of reciprocal altruism, then how can any of the methods of sociobiology be applied? The only possible way to test the genetic basis of the behavior would be to trace it phylogenetically. However, in contrast to other less complex human social behaviors that may be traceable, reciprocal altruism is not.

There are two serious misunderstandings in this paragraph. First, sociobiologists are not interested in whether a behavior is "social" or "genetic." They assume that all behaviors have a nonnegligible genetic component and go on to ask how natural selection would act on this behavior.[2,3] Second, even if sociobiologists were willing to classify some behaviors as "genetic" and others as "social," simply pointing to the behaviors in phylogenetic relatives would not be accepted as a demonstration of a common genetic basis. There is always the possibility of an analogous adaptation.[4]

Complexity

Finally, there is the issue of complexity. Humans have speech, true, and this does add another dimension to the analysis of behavior. But, even so, lions, langurs, baboons and chimpanzees are hardly "simple" organisms. If sociobiology can make sense of otherwise puzzling features of *their* social behavior, there is no reason to suppose, a priori, that *human* social behavior will be beyond its scope.

Charles J. Morgan
University of Washington
U.S. Environmental Protection Agency

REFERENCES

Dawkins, Richard
1976 The Selfish Gene. Oxford: Oxford University Press.

---

[1] Since the publication of Wilson's synthesis there has been more systematic work on nonhuman reciprocity. Most importantly, C. Packer (1977) showed that olive baboon males form reciprocal alliances that meet the criteria of reciprocal altruism. One male will solicit help from a second, and the alliance will then drive a third male from an estrus female. The helper-male fights with the third male while the solicitor copulates with the female. The roles are reversed in later episodes.

[2] The early ethologists, Konrad Lorenz (1961) in particular, did accept the idea that some behaviors could be classified as "genetic." They also argued for natural selection on the basis of "the good of the species." Neither position is accepted within sociobiology, although the data and observations of the ethologists are historically important, as was their extended fight with the behaviorists over the importance of genetic components of behavior.

[3] Most sociologists find unsettling the assumption that any behavior can be analyzed in genetic terms. Dawkins (1976:66) writes:

> For purposes of argument it will be necessary to speculate about genes "for" doing all sorts of improbable things. If I speak, for example, of a hypothetical gene "for saving companions from drowning," and you find such a concept incredible, . . . recall that we are not talking about the gene as the sole antecedent cause of all the complex muscular contractions, sensory integrations, and even conscious decisions, which are involved in saving somebody from drowning. We are saying nothing about the question of whether learning, experience, or environmental influences enter into the development of the behaviour. All you have to concede is that it is possible for a single gene, other things being equal and lots of other essential genes and environmental factors being present, to make a body more likely to save somebody from drowning than its allele would.

[4] The classic example of analogous adaptation is that bats and birds both fly. Analogies also occur between closely related species, so independent evolution is possible. The assumption of a common genetic basis simply on the grounds of relatedness is unsafe.

Greene, P. J., C. J. Morgan, and D. Barash
1979 "Sociobiology." In Scott and McNall (ed.), Theoretical Perspectives in Sociology. New York: St. Martin's Press.
Lorenz, K.
1961 "Phylogenetishe anpassung und adaptive modifikation des verhaltens." Z. Tierpsychol 18:139–87.
Packer, C.
1977 "Reciprocal altruism in *Papio anubis*." Nature 265:441–3.
Quadagno, Jill, S.
1979 "Paradigms in evolutionary theory." American Sociological Review 44:100–9.
Rosenblueth, A., N. Wiener, and J. Bigelow
1943 "Behavior, purpose teleology." Philosophy of Science 10:18–24.
Russell, B.
1971 "Is mathematics purely linguistic?" Pp. 295–307 in Douglas Lackey (ed.), Essays in Analysis by Bertrand Russell. New York: George Braziller.
Sahlins, Marshall
1977 The Use and Abuse of Biology: An Anthropological Critique of Sociobiology. Ann Arbor: University of Michigan Press.
Wilson, Edward O.
1975 Sociobiology: The New Synthesis. Cambridge, Mass.: Harvard University Press.

REPLY TO MORGAN*

The central aspect of my original paper (Quadagno, 1979) which has aroused the ire of sociobiologists is my concern for the applicability of sociobiological theories to the study of human social behavior. In his opening paragraph, Morgan (1980) states that this is one of the four issues he intends to discuss. Unfortunately, he never does so. Rather, he reserves the final portion of his comments for a critique of what I say about the method of sociobiology. First, let me respond to his methodological concerns. Then, I will reexamine the issue of human social behavior.

Morgan (1980) raises two objections to my summary of sociobiological method, both of which he attributes to a "misunderstanding" of how sociobiologists conduct their inquiries into human social behavior. First, he states that sociobiologists are not interested in measuring the genetic component of social behavior; rather, "they assume that all behaviors have a nonnegligible genetic component and go on to ask how natural selection would act on this behavior." Second, he appears to reject phylogenetic analysis as an acceptable sociobiological approach due to the problem of recognizing analogous adaptations (even though he uses it in a footnote to exemplify reciprocal altruism in baboons). While I would agree with Morgan that there are problems with both of these approaches, he seems to forget that they are advocated by Wilson (1975) in his final chapter on humans. Thus, in discussing cultural variation, Wilson (1975:550) notes:

> Although the genes have given away most of their sovereignty, they maintain a certain amount of influence in at least the behavioral qualities that underlie variations between cultures. Moderately high heritability has been documented in introversion-extroversion measures, personal tempo, psychomotor and sports activities, neuroticism, dominance, depression, and the tendency toward certain forms of mental illness such as schizophrenia. Even a small portion of this variance invested in population differences might predispose societies toward cultural differences. At the very least, *we should try to measure this amount*. (Emphasis, mine)

The second approach advocated by Wilson (1975:551) is phylogenetic analysis. He states, "By comparing man with other primate species, it might be possible to identify basic primate traits that lie beneath the surface and help to determine the configuration of man's higher social behavior." He then goes on to describe, in detail, just how this type of analysis might satisfactorily be performed on human social behavior, discussing both the disadvantages and the possible means of overcoming them.

In disregarding two of the methods suggested by Wilson, Morgan (1980) advocates the evaluative approach, defined as the attempt "to evaluate the adaptive significance of observed behavior patterns" (Barash, 1977:54). Morgan also states that theories are tested by deducing implications and then collecting data under controlled conditions. In order to illustrate just how evidence is used to determine the adaptive significance of observed behavior patterns in humans, I will analyze an extensive example from the sociobiological literature.

A good deal of sociobiological analysis concerns strategies that maximize reproduction (Daly and Wilson, 1978:264). One of Wilson's hypotheses is that dominance confers a reproductive advantage. He cites several examples from the animal literature which demonstrate that, in animals which establish a dominance hierarchy, dominant males sire more offspring (Wilson, 1975:288). He then extends the analysis to data from a study of the Yanomama Indians of Brazil which showed that politically dominant males fathered a disproportionate number of children, quoting the following conclusion from Neel (1970):

---

* Direct all communications to: Jill S. Quadagno; Department of Sociology; The University of Kansas; Lawrence, KA 66045.

I gratefully acknowledge the helpful comments of Charles Warriner and Michael Gaines.

> Our field impression is that the polygynous Indians, especially the headmen, tend to be more intelligent than the nonpolygynous. They also tend to have more surviving offspring. Polygyny in these tribes thus appears to provide an effective device for certain types of natural selection. Would that we had quantitative results to support that statement. (Quoted in Wilson, 1975:288)

In this culture, success was equated with political power (and, implicitly, intelligence) and was also associated with higher rates of reproduction. Implication? Political dominance and intelligence confer a positive adaptive advantage, being directly correlated with reproductive success—that is, with fitness.

These conclusions have been extrapolated by other sociobiologists to other measures of social success. Thus, Daly and Wilson (1978:289–90) state, "It is a common prejudice to suppose that the poor reproduce at an especially high rate but . . . several studies have shown a positive relationship between wealth and fertility." As evidence, they cite a study which demonstrated a definite correlation between number of living children and affluence. Implication? Wealth confers a positive adaptive advantage, being directly correlated with reproductive success.

The obvious difficulty with these sociobiological conclusions is that equally selective evidence could be mobilized to prove just the opposite. Thus, in Terman's study of gifted children with IQs of 135 or more, he found that they had somewhat fewer children by mid-life than the population at large (Terman and Oden, 1959). Similarly, a study of social class and family size in Peru found an inverse relationship between class and fertility—the poor had significantly more children (Stycos, 1968:148). Implication? Wealth and intelligence confer *no* adaptive advantage, being negatively related to reproductive success. Other research indicates that the very rich and the very poor have the largest families, with other income groups falling in between in terms of family size (Thomlinson, 1965:176). I am certain that a sociobiological explanation of the adaptive significance of this pattern, too, could be easily devised. Sociobiologists warn of "the danger in being too easily convinced by one's own cogent-sounding speculations, which nonetheless remain unverified" (Barash, 1977:54), but they seem unable to heed their own advice.

In specifying how to test a theory, Morgan (1980) emphasizes the phrase, "other things being equal," and stresses collecting data "under controlled conditions." What other things are equal? Where are the controlled conditions? In fact, human sociobiology is a highly speculative venture, in which selected evidence is used to prove that observed behaviors are examples of maximal reproductive strategies. For instance, Daly and Wilson (1978:290) reject the inclusion of industrialized countries for examining the relationship between fertility and social class, stating that the amount of reproductive variance in modern societies is too small. However, just a few pages later (p. 295), they cite Rainwater's survey of family design and attitudes in three American cities as an example of the persistence of pronatalism (an attitude equivalent to the desire to maximize one's reproductive success). Many more examples could easily be found. The method Morgan advocates simply is not used in the manner in which he asserts it is.

Now according to Morgan (1980), I do not understand the sociobiological view that natural selection produces organisms whose behavior is purposeful. Morgan is incorrect in his presentation of my view. In a reference to maximization, in which I quoted several sociobiologists in order to emphasize their preoccupation with this concept, all quotations referred to individual organisms (Quadagno, 1979:103). These quotations emphasized that natural selection produced organisms who were striving to maximize the number of successful offspring. Clearly, I am aware of what sociobiologists say. The conflict arises in regard to the interpretation of meaning, particularly the meaning of "purpose." Morgan engages in a familiar and peculiar bit of semantic manipulation by first declaring that "purpose" refers merely to a descriptive categorization or literary shorthand, as in the apparently purposive behavior of thermostats. If the analogy is to hold, then Morgan must accept the fact that "purposeful" behavior of organisms means only that organisms reproduce themselves the best they can (whether or not they are successful). Success cannot be used as a criterion for the anticipation of outcome (purpose) that guides the behavior of the organism. A thermostat does not anticipate outcomes. However, Morgan does not maintain the analogy, for he also uses purpose in a nonsymbolic, anticipatory sense—that is, by stating that organisms behave purposefully. The fact that Morgan vacillates between a literary and a literal interpretation elicits his second problem. How can natural selection remain a passive, minimal process when the orienting force of evolution is transferred to the organism, an organism which behaves purposefully to maximize its own inclusive fitness? The central erroneous hypothesis of sociobiology is not expunged by merely relocating purpose from nature per se to that part of nature called organism.

I also argued that sociobiologists tend to speak as if evolution produced maximal solutions which makes sociobiology irrefutable and tautological. Specifically (Quadagno, 1979:105), I stated, "... making the assumption that behaviors which presently exist represent the maximal adaptiveness of the organism makes it impossible to find behaviors which are not maximally adapted; by definition they cannot exist." The assumption of adaptiveness is central to sociobiological thought, and statements to this effect permeate sociobiological explanations. For example, Barash (1977:300) begins many paragraphs, "Assuming once again that human behavior has evolved to maximize individual fitness. . . ." If we make this assumption, then we can take any observed behavior pattern and produce a plausible explanation for its adaptive significance. Morgan's (1980) only response is to state that the circularity is broken by looking at the material relationship of the organism to its environment. How does one look at the material relationship of an organism to its environment when the "organism" is human and the "environment" is society? Further, why must we necessarily make the assumption of adaptiveness? Morgan's response is neither an argument nor an explanation. It is merely an assertion which allows him to avoid dealing with the very questionable assumptions that are built into sociobiological theories.

I have already discussed the issue of tautology in regard to reciprocal altruism in detail elsewhere (Quadagno, 1980). I see no value in repeating that argument here. In regard to Morgan's (1980) charge that I misquote Wilson in terms of the presence of reciprocal altruism in nonhumans, I would like to quote Wilson's complete statement, which is hesitant in tone, if not an outright denial:

> Human behavior abounds with reciprocal altruism consistent with genetic theory, but animal behavior seems to be almost devoid of it. Perhaps the reason is that in animals relationships are not sufficiently enduring, or memories of personal behavior reliable enough, to permit the highly personal contacts associated with the more human forms of reciprocal altruism (Wilson, 1975:120).

It is true, as Morgan (1980) asserts, that Wilson does subsequently list animal species where reciprocal altruism supposedly occurs, but it is merely a list of names. He does not document this list in any way or cite one single piece of research as confirmation.

Morgan (1980) repeats the basic tenets of evolutionary theory, including a stress on the importance of environmental influences, and then proceeds to ignore these basic elements as well as the ecological constraints upon them. Sociobiology does not proceed by testing predictions derived from theoretical models, as Morgan claims. Rather, it uses deductive logic to establish both evidence and conclusions, conclusions which are predetermined by the initial assumption of adaptiveness.

Jill S. Quadagno
The University of Kansas

REFERENCES

Barash, David
1977 Sociobiology and Behavior. New York: Elsevier.

Daly, Martin, and Margo Wilson
1978 Sex, Evolution and Behavior. North Scituate, Mass.: Duxbury.

Morgan, Charles J.
1980 "Darwin, Sociobiology, and Sociology" (Comment on Quadagno, 1979). American Sociological Review 45:502–4.

Neel, J. V.
1970 "Lessons from a 'primitive' people." Science 170:815–22.

Quadagno, Jill S.
1979 "Paradigms in evolutionary theory: the sociobiological model of natural selection." American Sociological Review 45:100–9.
1980 "Reply to Bolin and Bolin." American Sociological Review 45:159–62.

Stycos, J. Mayone
1968 Human Fertility in Latin America. Ithaca: Cornell University Press.

Terman, Lewis M., and Melita H. Oden
1959 The Gifted Group at Mid-Life. Palo Alto: Stanford University Press.

Thomlinson, Ralph
1965 Population Dynamics. New York: Random House.

Wilson, E. O.
1975 Sociobiology: The New Synthesis. Cambridge, Mass.: Belknap.

COMMENT ON WALTER R. GOVE AND MICHAEL HUGHES, "POSSIBLE CAUSES OF THE APPARENT SEX DIFFERENCES IN PHYSICAL HEALTH"

(*ASR*, FEBRUARY, 1979)*

Although males have higher mortality rates than females, health surveys often show that females have higher morbidity rates.[1] Females also have higher rates of restricted activity, doctor and dentist visits, and medicine use. Taking into account reproductive problems and health care for them, the sex differences are reduced but do not disappear. Sex differences in health are a longstanding and pervasive phenomenon; they have been reported for many years and in many countries. But, until recently, data to test hypotheses about why the differences occur were very limited.

---

* Direct all correspondence to: Lois M. Verbrugge; Institute for Social Research; The University of Michigan; P.O. Box 1248; Ann Arbor, MI 48106.

[1] "Morbidity" means physical illness and injury. The term "health behavior" includes all curative and preventive actions, such as short-term disability (re-

Using survey data for Chicago adults, Gove and Hughes (1979) examine sex differences in several health indicators. They believe that women do have poorer physical health than men, and they state their research aim: "We will present evidence to test the hypothesis that the apparent higher rates of morbidity [for women] are due to real differences in occurrence of physical disorder" (p. 131).

Gove and Hughes (1979) present results for two health indicators: (a) general health status, rated from excellent to poor, and (b) recent restricted activity because of "not feeling well." Both indicators show poorer health for women than for men. The authors posit that (a) poor mental health and high demands to help other family members lead to poor physical health, and that (b) women are disadvantaged for both mental health and nurturant role demands. If so, these factors account for part of the original sex difference in health. Gove and Hughes choose indicators for the two concepts and then use multiple regression procedures to look at how the sex difference changes when mental health, nurturant role demands, and several demographic characteristics are controlled. An external control variable (marital status/living arrangement) is used; equations are computed separately for four groups. For both dependent variables, Gove and Hughes report that the initial sex difference (usually statistically significant) is reduced and is not significant after controlling for the three types of predictors. They conclude that the results support the hypothesis that "the sex differences in physical health largely reflect real differences in physical health, and that this difference can be primarily attributed to women confronting more nurturant role demands and generally being in poorer mental health" (p. 143).

This comment considers how well Gove and Hughes (1979) test their hypothesis. It concludes that the dependent and independent variables are poorly operationalized and that the model tested is inadequate to explain sex morbidity differentials.

THE DEPENDENT VARIABLES

Reviewing health statistics, Gove and Hughes (1979) state that "the apparent higher rates of morbidity among women are based almost exclusively on higher rates of mild transitory disorders, and the task of determining the cause(s) of the apparent higher rate of morbidity among women must focus on these types of disorders" (p. 127).[2]

Three questions arise:

1. Accuracy. Is the summary of statistics accurate?
2. Coverage. Should research focus only on indicators with higher female rates?
3. Measurement. Are the health indicators used by Gove and Hughes good measures of "mild transitory disorders?"

The first two questions concern the rationale for choosing dependent variables. The third concerns measurement of those variables.

Accuracy

Gove and Hughes (1979:126) note that females have higher rates for many indicators of acute conditions, short-term disability, and health services use. They (pp. 126–7) find one exception: "There is, however, one rate that, while very small, is inconsistent with this pattern. Males are more likely to have more serious and incapacitating chronic conditions."

Actually, quite a few indicators show poorer male health: males have higher injury rates at all ages, higher partial work disability due to health, more severe mobility problems from chronic conditions, higher rates of institutionalization for poor health until elderly ages, and longer hospital stays at all ages. Males have higher prevalence rates for many "killer" (leading causes of death) conditions, both in health interview data and medical examination data. In early childhood, poorer health for males is especially common. Boys have higher rates than girls for most health indicators, whether they refer to acute or chronic problems.

---

stricted activity), long-term disability, health services use, medicine use, and preventive health habits. The term "health" is comprehensive, including both morbidity and health behavior.

[2] In health surveys, acute conditions are defined as transitory problems, whereas chronic conditions are long-term. (Operationally, acute conditions are problems with recent onset: for example, within the past three months. See Howie and Drury, 1978:51–53.) Gove and Hughes (1979) are apparently asking that research focus on acute conditions of mild severity.

As Gove and Hughes (1979) note, females tend to have higher rates for acute conditions and curative actions taken for them. But higher female rates are not limited to "mild transitory disorders." In health surveys, females report that they have more chronic conditions than men do. They have higher prevalence rates for many chronic conditions, although few of these are leading causes of death. Women are more likely to have complete work disability and house confinement due to chronic conditions.

Gove and Hughes's (1979) statements that (a) higher female morbidity is "based almost exclusively on higher rates of mild transitory disorders," and (b) higher male morbidity appears for "one rate," simply do not match the data. There is much more variation in the direction and size of sex differentials than those statements suggest.

Coverage

Gove and Hughes (1979) imply that research on sex differences in health should focus on items with higher female rates. There is no good substantive reason to do so. Explanations should be tested for a full range of health indicators, whether they show a male or female disadvantage. This is good scientific practice, and it is the best way to obtain strong support for a theory about why men and women differ in their health and health behavior.

(The same point can be made for large and small differentials. Sex differentials are the net result of many factors acting on men and women. These factors may "compete," some acting to boost male morbidity and others to boost female morbidity. A small sex differential can be the result of large competing factors. Thus, analysis of small differentials can be just as informative theoretically as analysis of large ones.)

Measurement

Given that the authors choose to study "mild transitory disorders," how well do their indicators fit that concept? Do they measure *physical health* (morbidity) and *mild transitory* problems?

Gove and Hughes (1979:134–5) present results for two indicators: general health status and "functional impairment." The first is a respondent's subjective evaluation of his/her overall health. The second refers to recent restricted activity ("whether or not during the past two weeks the respondents did not perform various activities because they were not feeling well").

General health status is a common health indicator in surveys. Its relationship to other indicators has been studied: it is strongly correlated with more-specific measures of physical health, and also strongly correlated with measures of psychological and social health (Tessler and Mechanic, 1978; Wan and Livieratos, 1978; Ware et al., 1978a; 1978b). It is, therefore, not a *distinct* indicator of physical health. In fact, it may be a better measure of general well-being than of physical health. Moreover, people's evaluation of their general health status is certainly influenced by the presence and severity of chronic conditions as well as of acute (transitory) ones.

"Functional impairment" is also far from the mark. Recent health problems may indeed be mild and transitory, but some are severe, and many are flare-ups of chronic conditions—or persistent symptoms from them. Moreover, the indicator used by Gove and Hughes (1979) concerns a health behavior, not the presence of illness or injury. The authors did construct an indicator for "the occurrence of any illness (that is, not feeling well)" in the past two weeks (pp. 134–5). This is closer to a measure of morbidity, although it is not limited to mild transitory problems. Gove and Hughes state that analyses were performed for the "occurrence" variable and that results are similar to those for "functional impairment," but they choose to show results for the latter.[3]

Summary

In summary, Gove and Hughes (1979) analyze several health indicators and show results for two of them. The indicators do not match at all well the concept of "mild transitory disorders." General health status is correlated with other measures of mental and social health. "Functional impairment" measures health behavior, not health status. Neither item is limited to mild transitory problems. This has important implications for the results, which I will soon discuss.

THE PREDICTOR VARIABLES

Gove and Hughes (1979:132) want to see if sex morbidity differentials are due to differences in the mental health and nurturant role demands of men and women. They expect that

---

[3] A note about terminology: normally researchers use the term "impairment" to mean a defect, usually static in nature, which permanently decreases a person's ability to perform some activities (see Howie and Drury, 1978:53). Gove and Hughes's (1979) label "functional impairment" is poorly suited to the survey items covered, and it is very misleading.

"poor mental health is causally linked to poor physical health and [because women have poorer mental health] this partially explains the higher rates of morbidity among women." They also expect "that certain types of role obligations interfere with one's ability to care properly for oneself and thereby produce minor forms of disability, and that such obligations occur more frequently among women and therefore partially account for their higher rate of morbidity."

There are three critical issues about the predictors:

1. Causation. Does poor mental health induce poor physical health? How? Do high nurturant demands reduce one's physical health? How? Do causal ties run the opposite direction, so that poor physical health leads to poor mental health and also decreases home responsibilities?
2. Measurement. How well do the selected items measure mental health and nurturant role demands?[4]
3. Conceptual Fit. Is an item a distinct measure for a theoretical concept, or is it related to other concepts too?

I shall focus on the third problem.

Conceptual Fit

Gove and Hughes's (1979) conceptual scheme contains three types of predictors: "artifacts," "role correspondence," and "real illness."[5,6] To support their hypothesis that sex morbidity differentials are "real," Gove and Hughes need predictors that measure risks of physical illness. Conceptually, these are "real illness" predictors.

The measures of mental health (also called psychiatric symptoms) and nurturant role demands do not fulfill those criteria very well. In fact, they may measure predictors of competing hypotheses, which claim that sex morbidity differentials are due to differences in health perceptions and attitudes: reports of psychiatric symptoms reflect "real" mental health status and *also* a person's perception of symptoms and willingness to report them. In the conceptual scheme, perception and willingness are "artifacts." Similarly, people with high demands to care for family members may become more sensitive to their own health. They may also feel that fulfillment of family roles depends on their good health, and this may encourage preventive and curative health actions. Sensitivity is an "artifact," and propensity to care for one's own health in order to fulfill family roles is "role correspondence."

Thus, even if good measures of mental health and nurturant role demands are used, they probably represent more than "real illness" risks. It is very likely they are related to health perceptions and attitudes which are contained in the "artifacts" and "role correspondence" concepts. This is especially plausible for Gove and Hughes's (1979:133) nurturant-role-demands items. The question on illness susceptibility is a good indicator for health perceptions (Becker, 1974): "If someone else at home is sick with the bug, do you often seem to catch it?" And the last three questions concern a person's ability to adopt the sick role: "When you are *really* sick, are you almost always able to get a good rest?"; "Even when you are really sick, are there a number of chores you just have to do?"; and "When you are really sick is there someone to help take care of you?" Using these items as if they were distinct measures of "real illness" invites trouble. The next section discusses the trouble.

---

[4] For example, there are more-direct indicators of nurturant role demands. Some simple and reasonable candidates are marital status and living arrangements. Gove and Hughes might have used these as predictors rather than as control groups.

[5] Based on several articles (Mechanic, 1976; Nathanson, 1975; 1977; Verbrugge, 1976a; 1976b), Gove and Hughes (1979:127–31) identify three main explanations for sex differences in health. These are "an artifact," "the correspondence between the woman's role and the sick role," and "real differences in illness."

"Artifacts" include symptom perception, willingness to talk about symptoms, propensity to use health services, and ways physicians treat patients. Medical sociologists usually call these factors *illness behavior* (cf. Mechanic, 1962; 1978). In the "artifacts" concept, Gove and Hughes (1979) include people's willingness to discuss symptoms with interviewers. This is a reaction to the interview situation, or *health reporting behavior*. In a conceptual scheme, it is probably good to keep these reactions to a special situation distinct from everyday illness behavior.

"Role correspondence" is viewed by medical sociologists as one aspect of *illness behavior*. It refers to how a person's roles encourage or discourage symptom perception, health services use, and other curative actions.

"Real differences in illness" refers to *genetic risks* of illness and *acquired risks* of illness and injury from people's social roles, life styles, stress, and responses to stress.

Gove and Hughes's (1979) scheme is awkward compared to the articles they cite. The categories italicized above are the principal ones used by Verbrugge (1976a; 1976b; 1979; forthcoming).

[6] A note on terminology: artifacts are factors that produce undesirable variation in data. Clinical epidemiologists might view illness behavior as an artifact. But medical sociologists do not; to them, illness behavior is a key theoretical interest. The label chosen by Gove and Hughes (1979) is misleading.

THE MODEL

Gove and Hughes's (1979) model is designed to include predictors about "real illness" risks and dependent variables about "mild transitory disorders." Predictors for "artifacts" and "role correspondence" are not included. Is this a good model through which to learn about sex differences in health?

It is risky to create a model which tests only the hypotheses one believes are true but deliberately excludes hypotheses believed to be untrue. What if predictors for the included hypotheses are correlated with predictors for the excluded ones? In other words, because of poor conceptual fit or measurement error, the included variables partly reflect concepts which are supposed to be absent. The consequences can be quite pernicious: results which apparently support the included hypotheses may actually support excluded hypotheses, a fact unperceived by researchers who see their favored hypotheses faring so well.

This is an especially critical issue for Gove and Hughes's (1979) analysis. The authors do acknowledge that the dependent variables are not "pure" measures of physical health. They (pp. 134–5) note that the variables may be influenced by symptom perceptions, propensities to take curative actions, ability to adopt the sick role, and the interview situation. This, they say, will work against their hypothesis: "Real illness" predictors will not account for all of the sex differential in such variables. If the included predictors do manage to account for *most* of it, that is strong support for the hypothesis. By this reasoning, Gove and Hughes conclude that the dependent variables actually permit a very good test of the "real illness" hypothesis.

This argument depends on an important assumption: that the predictors do indeed measure illness risks closely and distinctly. If they do not, the analysis strategy leads instantly to trouble. Earlier, I stated that indicators of mental health and nurturant role demands will often reflect "artifacts" and "role correspondence." If so, the model's predictors contain multiple concepts. The dependent variables in this case contain the same ones. What do correlations between the predictors and dependent variables then mean? Not necessarily what Gove and Hughes (1979) expect.

For example, people who are willing to report psychiatric symptoms are probably also willing to report physical health problems. If women are more-willing reporters than men, a sex morbidity differential can be accounted for by the predictor. But it does not support the "real illness" hypothesis. Instead, it supports the "artifacts" hypothesis. Another example: people with high nurturant role demands have health on their minds and may be as concerned about their own health as about others'. As a result (or as a correlate), they may view their general health status less favorably than other people do, and they may take more health actions for themselves. Gove and Hughes's (1979) five items are especially prone to such relationships. In their data, women express more concerns about sleep, susceptibility, and family support when ill than men do. Thus, the items can account for a sex morbidity differential while saying nothing about real illness of the sexes. Instead, the results support the "artifacts" and "role correspondence" hypotheses.

In summary, correlations between the predictors and dependent variables may support hypotheses about "artifacts" and "role correspondence." But in the context of Gove and Hughes's (1979) model, the correlations are accepted as evidence for the "real illness" hypothesis.

PROCEDURES

Multiple Classification Analysis (MCA) uses nominal-level predictors and interval-level dependent variables. "Functional impairment" is dichotomous (Gove and Hughes, 1979:135) and, therefore, meets the criterion for dependent variables. General health status is ordinal, with four categories. Gove and Hughes do not state how it is transformed into an interval-level variable for analysis. The presentation of results in Table 3 does not resolve the question. In fact, Table 3 is exceptionally difficult to understand. It does not follow MCA conventions: normally one should be able to read the average health status of males and females, controlling for certain predictors. If the health variable is dichotomous (for example, "excellent" vs. "other"), one then reads the percentages of males and females with "excellent" health. But in Table 3 the variables are reversed. One reads the percentage of people in the four health ratings who are male. (The second column showing percentage female is extraneous.) In short, we see sex by health status instead of health status by sex. Sex appears to be the dependent variable instead of the independent one. For people familiar with MCA, it is entirely unclear how Gove and Hughes produced the results in Table 3. Even without that familiarity with MCA, it is difficult to read the table and verify Gove and Hughes's (1979:140–2) discussion.

CONCLUSION

Gove and Hughes (1979:131) begin with an intriguing question: Why do females often have higher morbidity than males? They decide to test the general hypothesis that "the apparent higher rates of morbidity are due to real differences in occurrence of physical disorder." They decide *not* to test two other general hypotheses: Higher female morbidity is due to illness behavior and health reporting behavior ("artifacts") or to ability to adopt the sick role ("role correspondence"). To test their hypothesis, Gove and Hughes include predictors for mental health status and nurturant role demands, which they believe influence physical health. For dependent variables, they focus on mild transitory disorders, which often show higher rates for females.

But the predictor items chosen are not distinct measures of "real illness" risks: the mental health items may be affected by perception and reporting factors. The nurturant-role-demands items reflect perceptions and ability to adopt the sick role. Thus the mental health items say something about "artifacts," and the other items say something about both "artifacts" and "role correspondence." These factors are also present in the dependent variables. As a result, correlations between predictors and the dependent variables may say more about the two excluded hypotheses than the included one ("real illness").

Additional problems arise: the dependent variables are not good indicators of mild transitory problems. For the predictors, there are serious issues of causality and measurement.

For numerous reasons, then, the results do not test Gove and Hughes's (1979) favored hypothesis. What they *do* say about sex differences in health is hard to determine.

Could analysis of the Chicago data be more informative? Possibly. The data set apparently has few health variables, but it may have more variables which could serve as predictors for "artifacts," "role correspondence," and "real illness." By more careful assessment and choice of predictors, Gove and Hughes (1979) might have created models that test several hypotheses at the same time and that better separate the effects of artifacts, role correspondence, and real illness. Results could be compared for the morbidity variables vs. the health behavior variables. The critical point is to broaden the model and to understand the properties of predictor and dependent variables.

In general, it is better to test all principal hypotheses for a social behavior (or at least all that one's data permit), rather than only the hypotheses one likes. Then researchers can examine correlations among predictors, compute net effects, and assess the relative support for favored and disfavored hypotheses (if "sides" have been taken for them). Data often have limitations that preclude the testing of all the principal hypotheses. Researchers should then announce this and use extra caution in interpreting results.[7]

Finally, although Gove and Hughes (1979) imply that other researchers are content to speculate about reasons for sex differences in health and do not try to test them, the truth is quite different. Until recently, empirical work was hampered because health data sets lacked enough predictors to test a variety of hypotheses. There is now active research to collect appropriate data and also to conduct secondary analyses (see Davis, 1979; Marcus and Seeman, 1979; Verbrugge, 1979). In the next decade there will be plenty of pertinent research results. It will be especially interesting to see results for a variety of dependent variables which encompass the incidence and prevalence of health problems, many types of curative health actions, and preventive health actions. Whether they show a female or male excess, or a large or small sex differential, should not be a criterion for selection. With a wide range of dependent variables, a more general theory will emerge about how women and men perceive symptoms of illness and injury and how they respond to those symptoms.

Lois M. Verbrugge
University of Michigan

REFERENCES

Becker, M. H. (ed.)
1974 The Health Belief Model and Personal Health Behavior. Health Education Monographs, 2(4).
Davis, M. A.
1979 "The effects of sex, illness behavior, and disease on interview reports of symptoms." Paper presented at the American Sociological Association meeting, Boston.

---

[7] In this situation, researchers can also look for other surveys which have wider coverage of concepts and variables than their own data. By examining correlations among variables in other surveys, they will understand better the properties of their own items.

Gove, Walter R. and Michael Hughes.
1979 "Possible causes of the apparent sex differences in physical health: an empirical investigation." American Sociological Review 44:126–46.
Howie, L. J., and T. F. Drury.
1978 Current Estimates from the Health Interview Survey, United States, 1977. Vital and Health Statistics, Series 10, No. 126. Hyattsville, Md.: National Center for Health Statistics.
Marcus, A. C., and T. E. Seeman.
1979 "Male/female differences in health status: a report from the Los Angeles health survey." Unpublished paper. School of Public Health, University of California, Los Angeles.
Mechanic, D.
1962 "The concept of illness behavior." Journal of Chronic Diseases 15:180–94.
1976 "Sex, illness behavior, and the use of health services." Journal of Human Stress 2(4):29–40.
1978 Medical Sociology. 2nd ed. New York: Free Press.
Nathanson, C. A.
1975 "Illness and the feminine role: a theoretical review." Social Science and Medicine 9:57–62.
1977 "Sex, illness, and medical care: a review of data, theory, and method." Social Science and Medicine 11:13–25.
Tessler, R., and D. Mechanic
1978 "Psychological distress and perceived health status." Journal of Health and Social Behavior 19:254–62.
Verbrugge, L. M.
1976a "Sex differentials in morbidity and mortality in the United States." Social Biology 23:275–96.
1976b "Females and illness: recent trends in sex difference in the United States." Journal of Health and Social Behavior 17:387–403.
1979 "Female illness rates and illness behavior: testing hypotheses about sex differences in health." Women and Health 4(1):61–79.
Forthcoming "Sex differentials in health and mortality." Chapter in A. H. Stromberg (ed.), Women Health, and Medicine. Palo Alto: Mayfield Publishing.
Wan, T. T. H., and B. Livieratos
1978 "Interpreting a general index of subjective well-being." Health and Society 56(4): 531–56.
Ware, J. E. Jr., A. Davies-Avery, R. H. Brook, and S. A. Johnston
1978a Associations Among Psychological Well-Being and Other Health Status Constructs. Rand Paper P–6213. Santa Monica: The Rand Corporation.
Ware, J. E., Jr., A. Davies-Avery, and C. A. Donald
1978b The Conceptualization and Measurement of Health for Adults in the Health Insurance Study. Volume V, General Health Perceptions. Rand Report R–1987/5–HEW. Santa Monica: The Rand Corporation.

COMMENT ON GOVE AND HUGHES*

(*ASR* FEBRUARY, 1979)

There are numerous difficulties with the Gove and Hughes paper in the February, 1979, issue, but the key difficulty is that their analysis, like the earlier Gove and Tudor (1973) work on sex and mental health, creates confusion by using dependent measures that are not appropriate to the questions that are being raised and then asserts conclusions about measures (in this case, mild physical illness) that their data do not address.

It is the position of Gove and Hughes (1979) that higher rates of morbidity among women, as compared with men, result from a higher prevalence of "mild forms of physical illness," and that this results from poorer mental health and demands of a nurturant role. Given this hypothesis, one might have expected Gove and Hughes to investigate carefully sex differences in morbidity due to "mild forms of physical illness" and to seek instances in which their occurrence is independently validated to some extent. They do not do this.

Although Gove and Hughes (1979) purport to address the issue of mild physical differences, the choice of their dependent variables is inappropriate for this task. The measures used were a rating of "overall health" and an index of "whether or not during the past two weeks the respondents did not perform various activities because they were not feeling well" (p. 135). Although Gove and Hughes note that the later measure was "virtually interchangeable" with other indicators they had, all of the indicators they describe are gross indicators of response and behavior, not measures of symptoms or illness.

Gove and Hughes missed the point of the criticism made in an earlier study (Gove and Tudor, 1973) and make a similar error in the analysis of sex and physical illness. The point of the criticism is not that such entities as alcoholism or personality disorder need necessarily be defined as mental illnesses. The concept of mental illness is vague, and the definition of its constituents is arbitrary and largely determined by social definitions. The objection to Gove and Tudor's procedures is that they selectively aggregated entities into the definition of mental illness that are differentially related to sex but, in the aggregate, support the conclusion they wish to promote. The result is confusion and obfuscation, rather than improved understanding. It is fairly well established, for example, that there is no consistent sex difference in schizophrenia (Dohrenwend and Dohrenwend, 1969) but that the prevalence of depression is higher in women than in men (Weissman and Klerman, 1977). Because depression is more prevalent than schizophrenia, a combined index of the two will be correlated with sex; but noting such a correlation tells us nothing new and substitutes confusion for more refined information.

---

* Direct all communications to: David Mechanic; Graduate School of Social Work; Rutgers University; 536 George Street: New Brunswick. NJ 08903.

Gove and Hughes (1979) attempt to justify the illness response and behavior measures they use as proxies for physical illness. But overall health ratings are general indications of well-being, known to be correlated not only with physical illness of all types but also with psychological well-being, functional incapacity, and community participation (Ware et al., 1978; Tessler and Mechanic, 1978). In short, they are poor proxies for the concept the authors address. The fact that Gove and Hughes can demonstrate that sex differences on such indicators are reduced by controlling for psychological symptoms and other health-related reports (their scale of nurturant role demands) does not provide validation for their hypothesis that women have more mild physical illness. Perhaps women do; but the Gove and Hughes data do not really address this question. Their findings, as an examination of the literature would suggest (see the review by Ware et al., 1978), result from using a global dependent variable that measures health perceptions in their broadest sense.

Similar problems exist with the vague questions said to represent nurturant role demands (Gove and Hughes, 1979:133). It is plausible that differential role strain between men and women and between women with children and women without children could account for sex differences in illness reports. A fair examination of a role-demand hypothesis, however, would sample more widely from the possible range of role demands and life strains affecting both men and women and persons at home and those employed outside the home. It is difficult to understand why Gove and Hughes chose the items used in contrast to a more representative selection.

In presenting their argument, Gove and Hughes (1979) repeatedly misrepresent my review of hypotheses and data on sex differences in physical health (Mechanic, 1976). Rather than take space here, to correct these misattributions, I urge interested readers to review the original paper (which is also available elsewhere [Mechanic, 1978]) and to come to their own judgments. A major conclusion of that paper was that there is large variability in the responses from men and women, depending on the illness conditions or symptoms involved, and that we would learn more by focusing on these variabilities in contrast to global hypotheses that attempt to explain the data in the aggregate. Gove and Hughes would have done well to have heeded this advice.

David Mechanic
Rutgers–The State University
of New Jersey

REFERENCES

Dohrenwend, Bruce P., and Barbara Snell Dohrenwend
1969 Social Status and Psychological Disorder: A Causal Inquiry. New York: Wiley-Interscience.

Gove, Walter, and Jeanette Tudor
1973 "Adult sex roles and mental illness." American Journal of Sociology 78:812–35.

Gove, Walter R., and Michael Hughes
1979 "Possible causes of the apparent sex differences in physical health: an empirical investigation. American Sociological Review 44:126–46.

Mechanic, David
1976 "Sex, illness, illness behavior, and the use of health services." Journal of Human Stress 2:29–40.
1978 "Sex, illness, illness behavior, and the use of health services." Social Science and Medicine 12:207–14.

Tessler, Richard, and David Mechanic
1978 "Psychological distress and perceived health status." Journal of Health and Social Behavior 19:254–62.

Ware, John E., Jr., Allyson Davies-Avery, and Cathy A. Donald
1978 Conceptualization and Measurement of Health for Adults in the Health Insurance Study: Vol. V, General Health Perceptions. Santa Monica: Rand Corporation, Report R-1987/5-HEW.

Weissman, Myrna M., and Gerald L. Klerman
1977 "Sex differences and the epidemiology of depression." Archives of General Psychiatry 34:98–111.

SEX DIFFERENCES IN PHYSICAL HEALTH AND HOW MEDICAL SOCIOLOGISTS VIEW ILLNESS*

(A REPLY TO MECHANIC AND VERBRUGGE)

In an important article, "The oversocialized conception of man in modern sociology,"

---

* Direct all communications to: Walter R. Gove; Department of Sociology; Vanderbilt University; Nashville, Tenn. 37235.

Support for this paper was provided by NICD Grant No. 5-RO1-HD06911-02. We would like to thank Antonina Gove and Lisa Heinrich for the comments on an earlier draft.

Dennis Wrong (1961) describes how sociologists, by viewing the nature of men and women as almost entirely a result of socialization, have a very incomplete image of human beings. Wrong is quite aware that sociologists give lip service to the inherent psychological and biological characteristics of men and women, but argues that in their research and writing sociologists virtually ignore these aspects of human nature. To a large extent there is a similar bias in the work of some medical sociologists—including Mechanic and Verbrugge, who appear to have an oversocialized conception of illness and, in absence of any compelling contrary evidence, assume that the apparent sex differences in illness must be due to social and/or psychological processes. In reading this literature, one is struck by the extent to which actual illness, scientific diagnostic procedures, and forms of effective treatment are ignored. As Mechanic (1978:210), citing Mechanic (1968) states, "It is widely recognized that the definition of illness is arbitrary and based on social and cultural understandings." In an early paper, which presents a concept that is fundamental to his subsequent work, Mechanic (1962) discusses the "concept of illness behavior" which included factors affecting "symptom perception, the willingness to talk about symptoms, the propensity to use health services and the way physicians treat patients" (Verbrugge, 1980; fn. 5). We are aware of these processes and, in fact (Gove and Hughes, 1979:144), so asserted: "There is, of course, considerable evidence that such effects [processes] do exist." The issue is whether men and women have personalities which produce fundamentally different amounts and types of illness behavior such that the apparent higher morbidity rates among women are properly viewed as an artifact (that is, as a consequence of their different forms of illness behavior).

BACKGROUND

A premise that appears to be fundamental to the work of Verbrugge and Mechanic is that women are much more likely to manifest illness behavior and that, for this reason, they appear to have higher overall rates of morbidity due to physical illness. For example, Verbrugge (1976:399) states, "We suspect that males are physically sicker from both acute and chronic conditions, but that social and psychological factors enhance female reports to such an extent that a female excess appears overall." Similarly Mechanic (1978) notes that women have higher rates of utilization of medical facilities and also that women "report many more subjective symptoms than men, particularly those symptoms that may be indicative of either physical or psychological distress." However (p. 213), he concludes "that it is difficult to support the argument that women have more illness than men." Dohrenwend and Dohrenwend (1977:192) state, "Treated rates can be expected to give a picture of excess pathology among women if only because women are more likely to avail themselves of help for personal problems, from professionals in general and from medical and psychiatric professionals in particular." They go on to indicate that the higher rates of women in community surveys of mental health are also a consequence of women being more willing to engage in illness behavior than men. As noted in our paper (Gove and Hughes, 1979), there are a number of other scholars who present similar arguments, and it is our perception that, through process of mutual citation, it is made to appear that this characteristic of women is firmly established. However, there is no clear empirical evidence that this is the case. We would note that we take this statement to be a very serious charge, but neither Mechanic nor Verbrugge comments on it. For persons focusing on illness behavior, the two most commonly cited studies are those by Phillips and Segal (1969) and Mechanic (1964). Phillips and Segal (1969) show that women reported more symptoms of distress than men and argued that this was a consequence of it being culturally more appropriate for women to admit such symptoms. However, they presented no evidence for this assertion, and three subsequent studies (Gove and Geerken, 1977; Gove et al., 1976; Clancy and Gove, 1974), using the techniques subsequently developed by Phillips (e.g., Phillips and Clancy, 1970; 1972), showed that response bias, that is, illness behavior, is not a reason why women admit to more symptoms. Mechanic (1964) looked at boys and girls on the 4th and 8th grades and found that boys, particularly in the 8th grade, were less likely to say they were "afraid to get hurt," and more likely "to ignore pain" and "not tell others that they did not feel well." These data are suggestive, but, in interpreting them, other factors should be taken into consideration. First, boys at those ages "have higher rates than girls for most health indicators" (Verbrugge, 1980). Second, a 16-year follow-up of these subjects by Mechanic (1979) found that males and females were equally sensitive to—and willing to report—subjective symptoms of distress. This longitudinal study by Mechanic, in our view, raises very serious questions regarding his own position, which holds that there are marked sex differences in this type of illness behavior. Furthermore, in an analysis of the 1975 National Ambulatory Medical Care Survey, Verbrugge (1978:1) finds that (a) "there is little evidence that men and women differ in their perception, interpretation, and description of physical symptoms" and (b) "the evidence for sex bias in physicians' diagnosis is scant."

HYPOTHESIS

As noted by Verbrugge, we (Gove and Hughes, 1979) were concerned with three possible explanations of the apparent higher rates of morbidity among women. First, it was our substantive hypothesis that the rates of higher morbidity among women are real and can be attributed to: (a) the role obligations of many women and (b) the tendency for women to be in poorer mental health. If we were correct, then, when we controlled for mental health and women's nurturant role obligations, the differences between the sexes would disappear. If we were not correct—and the differences were due to illness behavior or the greater ability of women to assume the sick role—then the health differences between men and women would have remained the same, even after controls. This procedure would not have been appropriate if either: (1) our mental health scale could be shown to be subject to reporting bias by sex; or (2) our measure of role obligations actually measured the extent to which persons were free to assume the sick role (and persons with few obligations were more likely to report poor health and functional impairment). As will be indicated here, there is no evidence for either of these potential problems.

Our null hypothesis, of course, is that controlling for role obligations and mental health would have no effect. The alternative two hypotheses revolve around illness behavior. They suggest that women tend to be in situations where they can readily adopt the sick role and/or that women were more willing to describe their physical ailments to others. If either of these hypotheses were true, then controlling for mental health and role obligations would not decrease the apparent poorer physical health of women (because it would, apparently, have been due to illness behavior). Thus, Verbrugge and Mechanic's corresponding *null* hypotheses are that, with our controls, the apparent differences between the sexes would essentially disappear. We thus had a design which would indicate whether the sex differences were real or a consequence of illness behavior and, contrary to what Verbrugge says, our analysis does provide a test of her hypothesis.

MEASURES

We (Gove and Hughes, 1979) had two dependent measures: respondents' evaluations of his or her physical health—that is, (1) their responses to the question, Was his or her physical health excellent, good, fair or poor? and (2) the respondents' functional "impairments" as indicated by the respondents' saying that during the previous two weeks they had not performed activities they *normally* did because they were not feeling well. Both Mechanic and Verbrugge are highly critical of these two measures. Ironically, Tessler and Mechanic (1978) provide perhaps the best reason for using questions of this nature in survey data. They (with supporting citations) indicate (1978:254–5) that respondents, as they do not have a medical perspective, do not deal with specific symptoms of a disease but, instead, "view their health status in a more global fashion . . . [reacting] . . . to their overall sense of well-being and the extent to which the symptoms they experience disrupt their ability to function or interfere in some fashion with their daily activities." Elsewhere, Mechanic (1978:210) indicates that "self reported medical histories are highly suspect," due to differential reporting by sex; that there are "serious problems with using reported symptoms"; and that statistics on treatment are highly reactive to sex differences in illness behavior. In short, if Mechanic is correct, respondents are better at evaluating their overall health and functional status than they are at dealing with specific aspects of various illness.

Mechanic (1980) and Verbrugge (1980) are also critical of our measures of physical health because they are correlated with a respondent's affective state. The grounds for such criticisms puzzles us. In terms of overall health and a large number of specific diseases, it is well established that one's affective state has a substantial effect on one's physical health. Our (1979) paper presents 12 citations bearing on this fact; there are numerous others. Let us here cite one; it has been repeatedly demonstrated that in placebo therapy the body produces endorphins which are chemicals with a similar structure to that of the opiate-derived painkillers and they can be very effective (e.g., see Levine et al., 1978). Given present knowledge, if our global measures of physical health were not related to a person's affective state, that would be, ipso facto, evidence that our measures are virtually worthless. We would emphasize that the fact that the physical and mental health measures are correlated does not invalidate our study, for not only should they be related but they must also account for the sex differences in physical health.[1] Furthermore, Verbrugge appears to feel that we should not have looked at functional impairment[2] and both Mechanic and Verbrugge indicate that questions dealing with the respondents' evaluations of their overall health are inappropriate because these measures are related to illness behavior. As was indicated in our paper, from our point of view this is one of the positive attributes of those measures because they weighted the analysis against our substantive premise and in favor of our null hypothesis.

With regard to our intervening variables, we can be brief. The psychiatric symptom scale is presented in the paper, and we would simply reemphasize that it contains no psychophysiological items and there is no response bias by sex (Gove and Geerken, 1977). For ease in reading the remainder of the paper, we list the nurturant role variables: (1) Do you think you get enough sleep? (2) If someone else at home is sick with a bug, do you often seem to catch it? (3) When you are *really* sick, are you almost always able to get a good rest? (4) Even if you are *really* sick, are there a number of chores that you just have to do? and (5) When you are *really* sick, is there someone to help take care of you?

MECHANIC, SPECIFIC COMMENT

(1) We, as would Mechanic, urge you to read his paper (Mechanic, 1978). It is true that we did not focus on his major point, which appears to be that you need a specific theory for every disease (Verbrugge, in *her* comment on our article, seems to concur). Instead, we focused on a much more general question—namely, Do women, in comparison to men, have a strong general tendency to engage in illness behavior? Obviously, the importance of this tendency will vary, by disease; but the answer to this general question is necessary if one is to take Mechanic's position. Mechanic should also pay attention to studies that do appear to heed his advice in that they use the type of data and analysis he indicates are appropriate. In particular, Wolinsky (1978) has conducted the type of study that Mechanic (1978:211) specifically calls for. Wolinsky (1978:394) finds that "most of the explained variants are the results of illness—morbidity characteristics. This casts doubt on the importance of predisposing and enabling characteristics in explaining health care utilization." He also finds that the "indirect effects of predisposing and enabling characteristics are negligible." We would also note that, after controlling for indices of actual physical health, in all the analyses not only was the relationship with sex statistically insignificant but also, in addition, in 11 out of 24 analyses, *men* had higher rates of utilization.

(2) If Mechanic had read the article by Gove (1978) which appeared in the same issue as Mechanic's (1978), he would have seen an update on the community studies presented initially in Gove and Tudor (1973) and realized that his comment on schizophrenia serves as a smoke screen (intended or not). Only 2 (Hagnell, 1966:99–103 and Primrose, 1962:18–24) of 34 studies presented data on schizophrenia, and their rates are so low (the largest is .4%) as to have no effect on the general conclusion, because the rates in these community surveys typically range between 20% and 30% with the differences between the sexes being substantial. It is true that in those two studies (both conducted in atypical communities) there were more male than female schizophrenics, but even with combining the two studies there are too few cases to draw any conclusion (in combination, there were six male and two female

---

[1] For example, as is shown (Gove and Hughes, 1979:139) in Table 2, psychiatric symptoms uniquely account for over 50% of the variance in the functional impairment of those who live alone but, as is shown (p. 142) in Table 4, psychiatric symptoms have no effect on the sex difference in functional impairment (because these men and women are virtually identical in their mental health). We would note that the study by Wan and Livieratos (1978) and the Rand studies (e.g., Ware et al., 1978) of the General Well-Being Scale appear to be misinterpreted by Verbrugge and Mechanic. The General Well-Being Scale was designed by Harold Dupuy to measure the respondent's *psychological well-being* [personal communication, emphasis mine] and these studies demonstrate that this is what they, in fact, do measure, with the scale's relationship to physical health being modest. Verbrugge and Mechanic also appear to be unaware of the fact that Gove et al. (1976) show that, with an earlier version of the General Well-Being Scale, controlling for three forms of response bias (that is, "illness behavior") has no effect on the rates of men and women.

[2] Verbrugge (1980) is highly critical of our use of the term "impairment" because she feels it should indicate a permanent decrease in a person's ability to perform. We would simply note that our use is consistent with the *Webster's Unabridged Dictionary* and with how the term is used in psychiatry.

schizophrenics). Furthermore, in the United States, in 1967, women had a higher first-admission hospitalization rate for schizophrenia (sex ratio = 1.16) and were also more likely to be treated for schizophrenia in outpatient psychiatric clinics (sex ratio = 1.10) (Gove and Tudor, 1971).

VERBRUGGE, SPECIFIC COMMENTS

(1) Accuracy. Verbrugge (1980) translates our study into her framework and then criticizes us on the basis of her mistranslated formulation. In our view, high injury rates, partial work disability, mobility problems, early institutionalization for poor health, and longer hospitalization all fit under the single category of "serious and incapacitating conditions." We quote Verbrugge's listing of mild chronic conditions where women have higher rates (p. 127). We also both quote parts and summarize other parts of her discussion of chronicity, causes of death, and the occurrence of presumed sex reversals (that is, she misrepresents our presentation).

(2) Coverage. We (Gove and Hughes, 1979) wrote one paper that seriously looked at the possibility that the apparent excess female morbidity rate may be higher. It was the first paper to do so but used a data set not collected specifically for that purpose and we would welcome replications.

(3) The term "mild transitory disorders." Verbrugge demonstrated that males had higher mortality rates (except from diabetes) with those illnesses where there is a high mortality rate. As Verbrugge has also noted, acute conditions are more common than chronic. In short, if women do have higher actual rates of morbidity, it has to be primarily (but not solely) among disorders which are mild and transitory.

(4) Predictors. Verbrugge (in her 1980: fn. 4), suggests that we look at marital status and living arrangements as predictors rather than as control groups. First, we have no control groups in the formal sense of the term. In categorizing by marital status and living arrangement we have men and women in comparable situations. Thus, for the married, we are able to show that both the nurturant role obligations of the wife/mother and her poor mental health are related to her poorer physical health. In contrast, when we look at persons who live alone, we see that the poorer physical health among women can be attributed to demographic variables on the general health question, whereas mental health and role obligations have no effect on sex differences in physical health.

(5) Causation. Verbrugge (1980) suggests that poor physical health and poor mental health lead to a decrease in home responsibilities. This is virtually incomprehensible as a critique of our study because, in the data we present, poor physical health and poor mental health are associated with a very *high* level of obligations in the home. Not having obligations (that is, having a role compatible with the opportunity to be ill) is related to reporting *less* ill health, *less* functional impairment, and *fewer* psychiatric symptoms. In short, the relationship suggested by Verbrugge runs directly counter to the actual direction.

(6) Conceptual fit. (a) Refer, please, to Verbrugge (1980: fn. 6): to medical sociologists, illness behavior and health reporting behavior are properly viewed as processes to be studied. However, we are concerned with the roles and mental health of men and women as possible determinants of sex differences in physical illness. In the (Gove and Hughes, 1979) present study, a characteristic which systematically distorts the actual relationship between gender and physical illness means that the relationship we are looking at is not real but an artifact of an exogenous variable. Thus, in the present study, the term "artifact" is appropriate.

(b) Now, as to role obligations and role correspondence: role correspondence deals with those factors which sensitize one to health issues and encourage one to take good care of oneself. Our role variables involve social obligations which prevent one from taking care of oneself. This is especially true of the last three items which are our most powerful role variables.

TABLE 3: MULTIPLE CLASSIFICATION ANALYSIS AND AN APOLOGY

Verbrugge notes that our (Gove and Hughes, 1979) Table 3 does not follow MCA conventions and finds it very difficult to read. She is correct that the style of presentation is unusual but, if one is familiar with MCA, it is very easy to read and our style of presentation is more informative than the normal presentation with a multicategory dependent variable, where the only unadjusted and adjusted means are presented in combination with the Eta's and Beta's. Neither of these measures allows one to look at the presence or absence of monotonicity or to decide whether the variance observed with adjustment is due to a marked shift in one category or a modest general shift across a range of categories. MCA always has the same denominator, regardless of whether one is dealing with Eta's or Beta's. In fact, the only way a Beta differs from an Eta is that the value of the dependent variable has been adjusted for the effects of all control variables—with the specification that the marginal values remain the same. In normal regression, it is often difficult to compare the strength of variable because of the issue of multicollinearity. In partial correlations, the variation that is collinear is dropped from the analysis and, thus, it is difficult to know if a partial correlation is accounting for much variance or not. With Beta's (or, for that matter, b's), collinear variance is split among the different variables with the division of collinear variance being determined entirely by the variance uniquely explained by each variable. Thus, with two very highly intercorrelated variables, if one of the variables has virtually no unique variance, and another a slight amount, the latter variable will get virtually all collinear variance and have a large b and Beta—while the other variable may have a b and Beta that barely differ from zero. With the Eta's and (MCA) Beta's there is no such problem. Looking at an Eta^2 and a set of Beta^2's (as was shown in Tables 3 and 4), one can first state in precise terms the percentage of variance uniquely explained by an independent variable and, then, compare this with the percentage of variance that is uniquely explained after controls. Alternatively, one can look at the Beta^2 associated with a set of independent variables and tell precisely what percentage of variance is explained by each predictor after the effects of all the others have been controlled for (adjusted).

Verbrugge (1980) is correct that, in order to present the percentages, we reversed the independent and dependent variables. She expresses difficulty in reading Table 3 (modified), but this is probably only because the style of presentation is unusual. For example, take the upper part of the left column where one has a simple 2 × 4 table which shows how married men and women are distributed in terms of each category of health. The next-down 2 × 4 table shows exactly the same relationship after adjusting for the effects of the demographic variables. Down one more step, one finds what those relationships are after one has adjusted for the demographic and role variables. With each step, if comparing the Eta^2 and the Beta^2's one can tell exactly how much effect, in terms of percentage variance explained, is associated with each new set of controls. As an example of information gained in this style of presentation, look at the bottom of the last two columns. The two relationships that started out monotonic are no longer monotonic, but there is no way we could tell this from the means of the health variables or from the Eta's or Beta's, as no pattern of relationship is assumed with these measures. To get the correct Eta^2 and Beta^2 for each of these analyses, all one has to do is rerun the analysis with health as the dependent variable and sex as the independent variable. However, as Table 3 has the numbers of both men and women for each health category, one could, by hand, calculate the Eta's and Beta's for both sex and health for each 2 × 4 combination.

Now comes the apology. Table 3, which—as revised—is presented here, has the correct Eta^2's and Beta^2's and mean health scores. The original Table 3 had the Eta^2's and Beta^2's, with sex as the independent variable. In creating this original table it was necessary to have two identical sets of runs, one with sex as the dependent variable and one with health as the dependent variable (see preceding paragraph). In our paper (Gove and Hughes, 1979) we inadvertently reported the Eta's and Beta's from the wrong run. Table 2, we would note, correctly had health as the dependent variable. A comparison of the original Table 3 with the present Table 3 will show no substantive differences for the married, those living alone, and those formerly married living with others. In the case of the never married living with others the final Beta^2 is now smaller, while the relationship is now just barely statistically significant (this is due to the fewer degrees of freedom in the new table and the more predictor categories in the original table). Because of the relatively large Beta^2 we had treated this as an ambiguous category open to a variety of interpretations and as one that should not be taken as supporting our hypothesis. Thus, there was nothing in our discussion or conclusion that was affected by this error.

MODELS

The models being tested are discussed (Gove and Hughes, 1979) in the first section, and a number of the problems are dealt with there. From reading the paper, Verbrugge somehow concludes the data fit the "artifacts" and "role correspondence hypothesis." She concludes (Verbrugge, 1980) that it fits the artifact hypothesis because "people who are willing to report psychiatric symptoms are probably also willing to report health problems." If she had carefully read the paper she would have realized that Gove and Geerken (1977) have shown that, with the psychiatric symptom scale used, there is *no* bias in the scores of men and women (that is, there is no difference in illness behavior). This lack of bias has been replicated twice using different scales (Gove et al., 1976; Clancy and Gove, 1974). Furthermore, if women have a general tendency to report psychiatric symptoms and/or poor

Table 3.* Sex Differences in the Respondents' Evaluations of their General Physical Health: An Analysis of the Consequences of Controlling for the Effects of Demographic Variables, Nurturant Role Characteristics and Mental Health among Different Categories of Respondents

		Married		Living Alone		Formerly Married Living with Others		Never Married Living with Others	
Controls	Health	Male	Female	Male	Female	Male	Female	Male	Female
None:	Excellent	43.6	56.4	49.3	50.7	21.2	78.8	56.7	43.3
	Good	32.5	67.5	36.5	63.5	12.0	88.0	43.9	56.1
	Fair	31.4	68.6	29.5	70.5	7.4	92.6	27.0	73.0
	Poor	28.0	72.0	25.8	74.2	13.0	87.0	16.7	83.3
	$\overline{X}$	3.16	3.00	3.18	2.86	2.97	2.71	3.36	3.04
		Eta^2=.010^b		Eta^2=.028^c		Eta^2=.011^{ns}		Eta^2=.046^c	
Demographic:	Excellent	43.4	56.6	43.9	56.1	19.2	80.8	53.3	46.7
	Good	32.7	67.3	37.7	62.3	13.0	87.0	44.5	55.5
	Fair	31.3	68.7	34.1	65.9	8.5	91.5	31.4	68.6
	Poor	27.5	72.5	31.9	68.1	7.8	92.2	32.3	67.7
	$\overline{X}$	3.15	3.01	3.06	2.93	3.01	2.70	3.31	3.08
		Beta^2=.008^b		Beta^2=.005^{ns}		Beta^2=.014^a		Beta^2=.024^b	
Dem. and Role:	Excellent	40.1	59.9	43.3	56.7	17.4	82.6	51.2	48.8
	Good	33.1	66.9	37.8	62.2	13.0	87.0	45.6	54.4
	Fair	33.9	66.1	34.9	65.1	9.3	90.7	32.6	67.4
	Poor	39.2	60.8	31.6	68.4	9.3	90.7	36.8	63.2
	$\overline{X}$	3.10	3.03	3.05	2.94	2.93	2.71	3.28	3.10
		Beta^2=.002^{ns}		Beta^2=.003^{ns}		Beta^2=.007^{ns}		Beta^2=.014^a	
Dem., MH:	Excellent	40.5	59.5	44.1	55.9	17.2	82.8	53.1	46.9
	Good	32.8	67.2	37.4	62.6	13.1	86.9	44.5	55.5
	Fair	34.2	65.8	34.2	65.8	8.7	91.3	31.4	68.6
	Poor	37.7	62.3	31.8	68.2	11.1	88.9	35.3	64.7
	$\overline{X}$	3.10	3.03	3.05	2.94	2.91	2.72	3.29	3.09
		Beta^2=.002^{ns}		Beta^2=.003^{ns}		Beta^2=.006^{ns}		Beta^2=.016^a	
Dem., R., MH:	Excellent	38.9	61.1	43.2	56.8	15.9	84.1	51.8	48.2
	Good	33.2	66.8	37.6	62.4	13.0	87.0	45.5	54.5
	Fair	35.0	65.0	35.2	64.8	9.6	90.4	31.5	68.5
	Poor	43.9	56.1	32.0	68.0	11.3	88.7	36.8	63.2
	$\overline{X}$	3.07	3.05	3.04	2.95	2.86	2.72	3.28	3.10
		Beta^2=.000^{ns}		Beta^2=.002^{ns}		Beta^2=.003^{ns}		Beta^2=.013^a	
% Each Sex:		35.6	64.4	38.6	61.4	12.5	87.5	45.6	54.4
Number of Respondents:	Excellent	296	30.5%	150	34.5%	52	18.0%	97	36.9%
	Good	458	47.2%	159	36.6%	133	46.0%	123	46.8%
	Fair	191	19.7%	95	21.8%	81	28.0%	37	14.1%
	Poor	25	2.6%	31	7.1%	23	8.0%	6	2.3%
		970		435		289		263	

[a] <.05.
[b] <.01.
[c] <.001.
* This is a revised version of our (Gove and Hughes, 1979:140) original Table 3.

physical health, it should be fairly consistent in different living arrangements or different marital statuses. But in the present study, men and women who live alone show no such effect. Furthermore, Brocki (1979) has shown, with the same psychiatric symptom scale, that never married and widowed men articulate more symptoms than do their female counterparts. (Also see Gove, 1972; 1978; 1979).

As indicated above, the role variables reflect an *inability* to adopt a sick role. If Verbrugge had looked at Gove et al. (1979), which appeared in the same issue (as Gove and Hughes, 1979) of *ASR*, she would have found that the role items were related to household crowding, particularly to an overload of social demands.

In summary, the role variables reflect obligations to respond to the needs and demands of others, regardless of one's physical condition. It may be true that this experience sensitizes one to the need for rest and general health care, but the point is that it prevents one from taking good care of oneself.

CONCLUSION

Having systematically criticized the general orientation and specific comments of Mechanic (1980) and Verbrugge (1980) it is clear that we can find nothing in them that in any way affects our (Gove and Hughes, 1979) conclusion. We hope their comments and our rejoinder will clarify for others the issues involved. No single study provides a definitive answer, particularly when it is using a body of data gathered for another purpose, and we hope others will expand upon our measures of nurturant role obligations and attempt a replication. At the end of her comment Verbrugge notes that there are now three studies in progress that will help resolve the questions addressed in our paper and in her comment. We would only note that these studies were not in progress when our paper was written and that one of these studies, namely the one by Verbrugge, has no measures to test the hypothesis we proposed in our study even though she had a copy of that paper long before her study was initiated.

Walter R. Gove
Vanderbilt University
Michael Hughes
Virginia Polytechnic Institute and State University

REFERENCES

Brocki, Severine
1979 "Sex role, marital roles and mental well-being." Ph.D. dissertation. Department of Sociology and Anthropology, Vanderbilt University, Nashville.

Clancy, Kevin, and Walter Gove
1974 "Sex differences in respondents' reports of psychiatric symptoms: an analysis of response bias." American Journal of Sociology 78:205–44.

Dohrenwend, Bruce and Barbara Dohrenwend
1977 "Reply to Gove and Tudor's comment on 'sex differences in psychiatric disorders.'" American Journal of Sociology 82:1336–45.

Gove, Walter R.
1972 "Sex roles, marital roles and mental illness." Social Forces 51:34–44.
1978 "Sex differences in mental illness among adult men and women: an examination of four questions raised regarding whether or not women actually have higher rates." Social Science and Medicine 12:187–205.
1979 "Sex, marital status and psychiatric treatment." Social Forces 58:89–93.

Gove, Walter R., and Michael Geerken
1977 "Response bias in surveys of mental health: an empirical investigation." American Journal of Sociology 82:1289–317.

Gove, Walter R., and Michael Hughes
1979 "Possible causes of the apparent sex differences in physical health: an empirical investigation." American Sociological Review 44:126–46.

Gove, Walter R., Michael Hughes, and Omer Galle
1979 "Overcrowding in the home: an empirical investigation." American Sociological Review 44:59–80.

Gove, Walter, R., James McCorkel, Terry Fain, and Michael Hughes
1976 "Response bias in community surveys of mental health: Systematic bias or random noise?" Social Science and Medicine 10:497–502.

Gove, Walter R., and Jeanette Tudor
1971 "Adult sex roles and mental illness." Paper presented at the annual meeting of the American Sociological Association. Denver, August.
1973 "Adult sex roles and mental illness." American Journal of Sociology 77:812–35.

Hagnell, Olle
1966 A Prospective Study of the Incidence of Mental Disorder. Sweden: Berlingske Boktickeriet.

Levine, Jon, Newton Gordon, and Howard Fields
1978 "The mechanism of placebo analgesia." The Lancet Vol. II, No. 8091:654–7.

Mechanic, David
1962 "The concept of illness behavior." Journal of Chronic Diseases 15:180–94.
1964 "The influences of mothers on their children's health attitudes and behavior." Pediatrics 35:444–53.
1968 Medical Sociology: A Selective View. New York: Free Press.
1978 "Sex, illness, illness behavior, and the use of health services." Social Science and Medicine 12:207–14.
1979 "Development of psychological distress among young adults." Archives of General Psychiatry 36:1333–42.
1980 "Comment on Gove and Hughes." American Sociological Review 45:513–4.

Phillips, Derek, and Kevin Clancy
1970 "Response bias in field studies of mental illness." American Sociological Review 35:503–15.
1972 "Some effects of 'social desirability' in survey studies." American Journal of Sociology 77:921–40.

Phillips, Derek, and Bernard Segal
1969 "Sexual status and psychiatric symptoms." American Sociological Review 34:58–72.

Primrose, E. J. R.
1962 Psychological Illness. Springfield, Illinois: Thomas.

Tessler, Richard, and David Mechanic
1978 "Psychological distress and perceived health status." Journal of Health and Social Behavior 19:254–62.

Verbrugge, Lois
1976 "Sex differentials in morbidity and mortality in the United States." Social Biology 23:275–96.
1978 "Sex differences in complaints and diagnosis." Presented at the annual meeting of the American Public Health Association. Los Angeles, October.
1980 "Comment on Walter R. Gove and Michael Hughes 'Possible causes of the apparent sex differences in physical health.'" American Sociological Review 45:507–12.
Wan, T. T. H., and B. Livieratos
1978 "Interpreting a general index of subjective well-being" Health and Society 56: 531–56.
Ware, John E., Jr., Allyson Davies-Avery, Robert Brook, and J. Shawn Johnston
1978 "Associations among psychological well-being and other health statuses constraints." Vol. V, General Health Perceptions. Santa Monica: Rand Corporation, Report P-6213.
Wolinsky, Fredric
1978 "Assessing the effects of predisposing, enabling and illness-morbidity characteristics on health service utilization." Journal of Health and Social Behavior 19:384–96.
Wrong, Dennis
1961 "The oversocialized conception of man in modern sociology." American Sociological Review 26:183–93.

IDEOLOGY, SPECIFICITY, AND THE CODING OF LEGAL DOCUMENTS: WHAT DO MEASURES OF CONSTITUTIONAL CONTENT MEASURE?*

(COMMENT ON BOLI-BENNETT AND MEYER, *ASR*. DECEMBER, 1978)

In "Ideology of Childhood and the State," Boli-Bennett and Meyer (1978:797) attempt to "trace the emergence of differentiated and state-managed childhood at the institutional level over the last century," by drawing on a kind of evidence seldom used by sociologists: the content of national constitutions, viewed as "fundamental rules . . ." that ". . . embody very general ideological definitions of the nature of persons and society." They conclude that the emergence of a state-managed childhood ideology is highly correlated with increasing state authority and is best accounted for as a diffusion phenomenon, moving from core countries to peripheral ones (p. 810).

We applaud Boli-Bennett and Meyer's attempt to answer important questions about social change by deriving quantitative indicators from the content analysis of legal documents. We must argue, unfortunately, that the validity of their indicators is suspect, because they failed to satisfactorily resolve major problems in deciding which legal documents to code, in developing their coding rules, and in interpreting their results. The content analysis-based indicators may describe national ideologies about childhood and state authority in the way in which Boli-Bennett and Meyer claim that they do. But they probably do not. Even though Boli-Bennett and Meyer's hypotheses may prove correct (and some of those on the diffusion of legal reforms are consistent with the findings of others—for example, Collier and Messick, 1975), the data presented do not necessarily provide much evidence in their favor.

Selecting documents to code. Boli-Bennett and Meyer's (1978:798) basic hypothesis concerns "the institutional rules defining childhood . . . ." In order to analyze institutional rules, they examine the content of constitutions. Their dependent variable, the "childhood index," is a measure of the "elaboration of constitutional rules defining the nation-state as responsible for . . . children" (p. 810). In addition, two independent variables viewed as measures of general state authority—indices of state authority and of citizen rights—are also measures of the content of constitutions.

But many important institutional rules are contained in legislation, administrative regulations, and court decisions, as well as in constitutions. Boli-Bennett and Meyer (1978:800) state, for example, "Constitutions do not fully capture the extent of state involvement in childhood . . . much state involvement in childhood occurs through legislation."

Boli-Bennett and Meyer's (1978:800) major reason for excluding legislation (and other forms of state rule-making) from their analysis is apparently their belief that ". . . legislation reflects many factors besides the ideological process discussed here." They appear to be assuming—their discussion is only a footnote—that constitutions are "purer" manifestations of ideology than ordinary legislation is. They provide no evidence for this assumption that all states consistently regard constitutions as the appropriate document in which to specify their commitment to the status of childhood.

The assumption—and consequently the validity of their measures—can readily be ques-

---

* Direct all communications to: Ronnie Steinberg Ratner; Center for Women in Government; State University of New York at Albany; Albany, NY 12222.

We would like to thank Richard Wilson for helpful suggestions.

tioned. States may vary in their tendencies to institutionalize rules affecting childhood in constitutions. For example, Boli-Bennett and Meyer (1978:801) point out that the United States Constitution "contains no provisions distinguishing children as a separate category of persons with unique rights or obligations, or subject to particular state action." This lack does not reflect the American ideology of childhood in any meaningful sense, however, because ideological commitments are typically made in American state constitutions and legislation (Ogburn, 1912; Ratner, forthcoming). Israel has no constitution at all, because of intense disagreements over what should be regulated through inclusion in a constitution rather than in ordinary legislation.

In addition, nations may change over time in their tendencies to institutionalize, in constitutions rather than elsewhere, the rules affecting childhood. Lowi (1971) has provided evidence that the boundaries between different forms of state rule-making can change over time, showing that federal authority in the United States is increasingly expressed in administrative regulations rather than in legislation.

Thus, we believe that constitutions, like ordinary legislation, will reflect many factors, including politicians' ideas about what should be in an ordinary law, in administrative regulations, or in court decisions, rather than in a constitution. By examining only constitutions and ignoring other legal documents, Boli-Bennett and Meyer have developed a childhood index which measures rules affecting childhood, and cross-national differences in the inclination to put rules into constitutions, and over-time differences in the same inclination—each to some unknown degree. We cannot, therefore, know the extent to which Boli-Bennett and Meyer are measuring ideologies of childhood rather than patterns of constitution-writing.

In order to construct more valid indices, we suggest that, for each country studied, the legal documents (constitution, legislation, etc.) which contain a definition of childhood and "references to state action in relation to children" (Boli-Bennett and Meyer, 1978:800) be identified. Coding would then be carried out on all types of legal documents containing rules for state action. This alternative procedure would ensure that the focus would be on the substantive problem. (Moreover, it would provide an additional piece of information: by coding the type of legal document, one could also determine which legal documents encompass rules about childhood in different countries and at different times. In this way, that which Boli-Bennett and Meyer take to be a constant—the boundary between legislation and constitution—could be treated as a variable.) To be sure, locating all the relevant documents from a 100-year period would be laborious, and, as Boli-Bennett and Meyer (p. 800) state, the analysis would be "complex." The workload could be reduced, however, by studying not all independent nations, but a sample; the nations could be stratified by two plausible explanatory variables used by Boli-Bennett and Meyer: (1) date of independence, and (2) centrality in the world system. Improvements in the childhood index and other indicators would, we believe, more than compensate for the reduction in the number of countries studied (see Ratner, forthcoming; Kagan et al., 1977, for discussions of this issue).

Coding rules. As noted, three of Boli-Bennett and Meyer's crucial variables—the childhood index and two measures of state authority— are based on the content analysis of constitutions. All three indicators of content are largely measures of the amount of detail in those constitutional provisions dealing with a given issue. For example, one of the components of the childhood index is an indicator of the "levels of education specified": a constitution is given a score of 0 on this indicator if education is not mentioned; a 1 if it is mentioned but if no more than one level is specified; and a 2 if two or more levels of education are specified (Boli-Bennett and Meyer, 1978:803). Similarly, the index of constitutionally defined state authority "simply sums the number of institutional areas under state jurisdiction and the number of administrative mechanisms available to the state to manage society" (p. 807).

What do these measures of content measure? There are at least two possibilities. First, they may measure ideologies of childhood and of state authority, as Boli-Bennett and Meyer claim. The degree of detail with which people deal with a subject probably indicates their degree of concern and the breadth of their aims.

Alternatively, the measures may gauge the inclination of constitution writers to go into detail. Boli-Bennett and Meyer show that the mean score on the childhood index has increased fairly steadily during the last 100 years. This result is consistent with the hypothesis that constitutions have become more specific during that time—possibly because provisions that formerly would have been written into ordinary legislation are now being written into constitutions. On the basis of the evidence presented, we cannot tell if Boli-Bennett and Meyer are measuring ideologies of childhood or an inclination to go into detail.

How does this ambiguity about what they are measuring affect our interpretation of their findings? We can conclude that their findings are consistent with one of their implicit hypotheses—that certain aspects of constitutions diffuse around the world. The problem is that we cannot tell, from the data, exactly what it is that is diffusing—ideologies of childhood, or the propensity of constitution-writers to go into detail, or government regulation.

This problem is especially apparent when we try to interpret the correlations between the childhood index and other variables presented in Table 3. The strong correlations are those between the childhood index and the indices of general state authority already mentioned. Boli-Bennett and Meyer (1978:800) claim that this shows that "childhood as political ideology reflects . . . the degree of authority over society assumed by the state" and (p. 801) that "the rules defining childhood . . . should be little affected by variations in the level of organizational complexity of national societies." In fact, these results are consistent with either ideologies of childhood or specificity of constitutions having the hypothesized relationships with other variables. For example, it is especially noteworthy that the highest correlations are those between indices of different dimensions of the content of constitutions—the childhood index and indices of state authority. This is exactly what we would expect to find if the different indices were, in fact, all imperfect measures of a single underlying variable—the specificity of constitutional provisions.

How could we resolve this ambiguity? One way would be to develop additional independent measures of state involvement in childhood, and to compare the new indicators to those of Boli-Bennett and Meyer. For example, Boli-Bennett and Meyer (1978:797; and similar statements on 810–1) suppose ". . . that, in the long run, the establishment of the ideology of modern childhood throughout the world lends strong support to the organizational implementation of dependent but protected childhood status." If this is the case—that is, *if* Boli-Bennett and Meyer's indices measure ideology, and *if*, as they suppose, ideologies will be implemented—then we would expect significant (lagged) relationships between their indices and variables gauging the implementation of relevant programs. (Implementation of ideologies of childhood could be measured, for instance, by levels of educational attainment, proportion of children working in manufacturing, and so forth.) Alternatively, if the indices measure specificity—and not ideologies of childhood or state authority—we would not find such relationships. Such a lack of relationship between the childhood index and an implementation index (coupled with strong relationships between the different dimensions of the content of constitutions) would lend great weight to the constitutional specificity hypothesis.

Thus, we could determine whether Boli-Bennett and Meyer's indices are valid indicators of the rules concerning childhood likely to be implemented. Our guess is that the relationship between indices of ideology found in constitutions and implementation will be weak; consider the civil liberties guarantees in the Soviet constitution, or the lack of rules about childhood in the United States Constitution. The relationships will probably be stronger, however, when other legal documents are taken into account.

Interpreting results. Boli-Bennett and Meyer (1978:798–9) assert that "general rules differentiating childhood and incorporating it as a distinct status in the structure of the state arise as part of the 'modern' (bourgeois and technical) ideology defining the nature of life and action. Two central elements of this perspective are . . . *the nature of individualism* and the *expansion of the state.*" They conclude (p. 810) that "the dominance of the ideology of differentiated and state-managed childhood reflects the rise of both individualism and the rationalized nation-state."

Boli-Bennett and Meyer appear to link the rise of childhood as a distinct status—particularly as dealt with by the educational system—with an increase in individualism. Their coding scheme appears to operationalize an odd notion of individualism, however. Consider their indicator of "state control of education," part of the childhood index. A constitution is assigned a score of 2, the highest possible score, if "the state has monopoly power over education: nonstate schools are prohibited" (Boli-Bennett and Meyer, 1978:802). The 1954 constitution of the People's Republic of China, an explicitly collectivist society, is assigned a score of 2 on this dimension (p. 802). If this indicator were a measure of individualism, we might expect a higher score to be given to constitutions in which education was required, and in which the individual was offered alternative forms of education from which to choose. But that is not the case.

Table 4 (Boli-Bennett and Meyer, 1978:809) presents the mean scores of the childhood index of different types of countries. "Communist-party dominant" countries have a mean score of 9.4, while "European Protestant" countries (including the United States) have a mean of 4.2. It is perfectly all right to give technical meanings to everyday words, but it defies common sense to suggest that an index reflects individualism stemming from the rise of bourgeois ideology when Communist countries get high scores and European and American Protestant countries get low scores. The childhood index is probably better seen as reflecting state acknowledgment of stages of the life cycle and social control over children.

One last point. Boli-Bennett and Meyer set themselves a difficult task—studying social, political, and ideological change by methodically content analyzing sample constitutions. Problems might have arisen no matter what they did. But they appear to have exacerbated their problems in two major ways: first, by relegating most of the discussion of a critical issue—the meaning of the legislation-constitution distinction—to a footnote; and, second, by claiming that "few researchers have yet tried to tackle legislation on a systematic comparative basis . . ." (Boli-Bennett and Meyer, 1978:800). We suggest they refer to Kochran (1973), Stetson and Wright (1975), Berk et al. (1977), Burstein (1978), and Ratner (1977; forthcoming). All deal extensively with the content analysis of legislation, and the latter three in particular include bibliographies of the vast political science literature on the subject.

We think that Boli-Bennett and Meyer's hypotheses are important and their aims worth pursuing. Because their indicators are not well thought through, however, their data do not support their conclusions. At this stage in the development of this type of research, it is especially important to draw on previous work so that we can sidestep the pitfalls surrounding the construction of sound measures of legal rules.

Ronnie Steinberg Ratner
SUNY, Albany
Paul Burstein
Yale University

REFERENCES

Berk, Richard A., Harold Brackman, and Selma Lesser
1977 A Measure of Justice: An Empirical Study of Changes in the California Penal Code, 1955–1971. New York: Academic Press.

Boli-Bennett, John, and John W. Meyer
1978 "The ideology of childhood and the state: rules distinguishing children in national constitutions, 1870–1970." American Sociological Review 43:797–812.

Burstein, Paul
1978 "A new method for measuring legislative content and change." Sociological Methods and Research 6:337–64.

Collier, David, and Richard C. Messick
1975 "Prerequisites versus diffusion: testing alternative explanations of social security adoption." American Poltiical Science Review 69:1299–1315.

Kagan, Robert, Bliss Cartwright, Lawrence Friedman, and Stanton Wheeler
1977 "The business of state supreme courts." Stanford Law Review 30:121–56.

Kochran, Thomas A.
1973 "Correlation of states' public employment bargaining laws." Industrial Relations 12:322–37.

Lowi, Theodore
1971 The End of Liberalism. New York: Norton.

Ogburn, William F.
1912 Progress and Uniformity in Child Labor Legislation: A Study in Statistical Measurement. New York: Columbia University Press.

Ratner, Ronnie Steinberg
1977 "To them that hath shall be given: a quantitative analysis of the relationship between unionization and social reform legislation." Paper presented at the annual meeting of the Eastern Sociological Society, New York City.
Forthcoming The Rise of Wage and Hours Standards Laws in the United States, 1900–1973; A Social Indicators Approach: A Modest Magna Charta. New Brunswick, N.J.: Rutgers University Press.

Stetson, Dorothy M., and Gerald C. Wright
1975 "The effects of laws on divorce in American states." Journal of Marriage and the Family 37:537–47.

CONSTITUTIONS AS IDEOLOGY*

(REPLY TO RATNER-BURSTEIN)

In its evolution over the past three centuries, world society has constructed a global level of social reality in which highly expanded state authority is legitimated. Part of this process has involved an expansion of the linkages between states and individual citizens. The individual has come to be defined as possessing more and more rights and duties, in relation to collective society, as well as the personal qualities and capacities necessary to exercise these rights and fulfill these duties. This new, complex definition of the individual is increasingly organized around citizenship: the individual is a member of and agent for the state (Bendix, 1964; Swanson, 1971). There has been a progression from earlier definitions of the individual identified in relation to a religious or natural universe to the more recent rationalized definition of the individual as a citizen of the nation-state.

---

* Direct all communications to: John W. Meyer, Department of Sociology; Stanford University; Stanford, CA 94305.

World society has increasingly taken on a life of its own, transcending even the level of social organization of the dominant national societies, and it enjoys rather strong legitimacy, in part because of the formal equality among all nation-states in the present world. Its culture flows, as ideology, into the constitutive structures of national societies, in large part independently of the variations among local societies and economies.

We studied this process of the flow of ideology as it involves the development of state authority over childhood, a central part of the life of the individual. We showed that state authority over childhood is increasingly incorporated in national constitutions in a way that clearly reflects the influence of world society rather than of variations in national development. Indeed, the phenomenon occurs most comprehensively in weak peripheral nations, perhaps because they are the most dependent on world society and the least constrained by their own organizational histories.

The comments of Ratner and Burstein do not seem relevant to the general issue we analyzed, for the following reasons:

(1) Constitutions, which are quite general ideological depictions of state authority, are indeed most appropriate for our purposes. Our critics complain that these documents may be subject to prevailing world ideologies, but that was precisely our point: They are. Of course it would be useful to study other ideological documents (we are presently involved in doing so), but neither legislation nor administrative regulations are as suitable as constitutions for evaluating our theoretical perspective. Such other documents obviously reflect social organization processes other than ideology, though they have ideological aspects too.

(2) We did not argue that constitutional rules in particular countries are likely to be "implemented" but, rather, that prevailing world ideologies are likely to be incorporated both ideologically and organizationally. For instance, the expansion of educational enrollments is a world process largely independent of national variations (Meyer et al., 1977). But ideological adherence and organizational implementation do not necessarily occur at the same time or at the same rate: peripheral states that are most likely to adopt constitutional ideology as a mobilizing strategy are often slowest to adapt organizationally. Their structural weakness in world society underlies both processes.

The important point is that we studied constitutions in their own right as ideology, not under the naive assumption that ideological adherence in constitutions has automatic and immediate effects on concrete organizational structures.

(3) Constitutional rules are not functional alternatives to legislation or administration. There is a complex two-way relationship between ideological conformity to world society and state organizational efforts to give concrete form to such ideologies. For instance, American national legislation in the area of childhood has been greatly limited by the absence of constitutional authority for such legislation. More generally, our index of national constitutional authority over childhood (the Childhood Index) is correlated .30 (74 cases) with the Ramirez and Rubinson (1979) measure of central national controls over primary education, which is derived from quite different sources. These two distinct aspects of state structure therefore complement, rather than substitute for, each other.

(4) Constitutions indeed get more specific over time as they legitimate expanded state authority over more domains, including childhood. This reflects world society standards that go far beyond the whims of writing style, for actual state structures have enormously expanded their domains and controls over the same period (Meyer et al., 1975). Constitutional specificity or "wordiness" is a minor issue: constitutional length (in characters) in 1970 is correlated only .05 with the Childhood Index (139 cases, data from Boli-Bennett, 1976). For 1950, the figure is .23 (76 cases).

(5) We attempted no direct measure of national "individualism" and employed the notion only as an explanatory theoretical element which accounts for one aspect of the evolving world political culture.

Further, individualism (as we used the term) is not in opposition to collectivism. The modern world society dialectically elaborates both collective authority as vested in the state and the importance of the role of individuals and individual action, also increasingly vested in the state. Thus, socialist states expand the ideologies of both collective authority and the individual, and many new postsocialist states having great constitutional authority endow individual actors with extraordinarily expanded citizenship rights and duties. This phenomenon has been discussed at least since the work of Marshall (1948).

(6) We noted that there is little comparative research on our problem. Our critics cite a literature, and specific studies, that are not very relevant, consisting as they do of case studies of specific types of legislation, and comparisons among American states. There are few cross-national studies, and practically

no studies of organizational ideologies at the national and transnational levels.

John Boli-Bennett
John W. Meyer
Stanford University

REFERENCES

Bendix, Reinhard
1964 Nation-Building and Citizenship. New York: Wiley.

Boli-Bennett, John
1976 The Expansion of Nation-States, 1870–1970. Ph.D. dissertation, Department of Sociology, Stanford University.

Marshall, T. H.
1948 Citizenship and Social Class. New York: Doubleday.

Meyer, John W., John Boli-Bennett, and Christopher Chase-Dunn
1975 "Convergence and divergence in development." Annual Review of Sociology 1:223–46.

Meyer, John W., Francisco O. Ramirez, Richard Rubinson, and John Boli-Bennett
1977 "The world educational revolution, 1950–1970." Sociology of Education 50:242–58.

Ramirez, Francisco O., and Richard Rubinson
1979 "Creating members: the political incorporation and expansion of public education." In J. Meyer and M. Hannan (eds.), National Development and the World System. Chicago: University of Chicago Press.

Swanson, Guy E.
1971 "An organizational analysis of collectivities." American Sociological Review 36:607–24.

ITEMS

■ BERT USEEM (The Boston Anti-Busing Movement) is a Postdoctoral Fellow at the Center for Research on Social Organization, University of Michigan. He recently completed a study of worker participation in the Bolivian revolution (forthcoming, as *Politics and Society*). His current research involves the nuclear-power controversy.

■ KENNETH A. BOLLEN (Comparative Measurement of Political Democracy) is an Associate Senior Research Scientist in the Societal Analysis Department of General Motors Research Laboratories. Currently, he is working on several projects in the area of international development and statistical problems of using ratio variables.

■ JAMES R. LINCOLN (Organizational Properties from Aggregate Data) is Associate Professor in the Department of Sociology, Indiana University, Bloomington. His current research interests concern interorganizational networks. He is engaged in a study of client flows and other relations between community agencies delivering services to youthful "status" offenders. He has also begun a project addressed to corporate interlocks and other ties relating large United States corporations. GERALD ZEITZ is Assistant Professor in the Department of Sociology, State University of New York (SUNY) at Stony Brook. He is currently researching two areas: (1) structural and individual determinants of morale in industrial organizations; and (2) development of nuclear-power industry in the United States. The intention is to determine to what extent interorganizational theory can be used to understand this development.

■ HERBERT BLUMER (Social Behaviorism and Symbolic Interactionism) is Professor Emeritus, Department of Sociology, University of California at Berkeley. Currently, he is working on "Analysis of the 'Social Situation'," "Limits of Structural Analysis," and "The Career of Social Movements."

■ CLARK MC PHAIL (*Ex Cathedra* Blumer or *Ex Libris* Mead?) is Associate Professor, Department of Sociology, University of Illinois at Urbana-Champaign. Presently, his research concentrates on the many aspects of "Crowd Behavior." CYNTHIA REXROAT is Assistant Professor, Department of Sociology, University of Florida. Her current research involves a study of racial differences in timing of the first birth and in wives' labor-force participation.

■ D. A. ANDREWS (Structural Intervention and Differential Association) is Associate Professor, Department of Psychology, Carleton University, Ottawa, Canada. Three topics occupy his current research: (1) psychosocial and organizational correlates and predictors of problem behaviors; (2) program evaluation as a means of investigating, experimentally, the causal and practical significance of and the links between social and behavioral theory; and (3) the selection and training of citizen volunteers for intensive vs. nonintensive probation supervision and their effects on the process and outcome of adult probation.

■ RONALD R. RINDFUSS (Education and Fertility . . . Roles Women Occupy) is Assistant Professor, Department of Sociology, University of North Carolina at Chapel Hill. His book, *Postwar Fertility Trends and Differentials in the United States*, written with James A. Sweet, was published in 1977 (New York: Academic Press). He continues work on various sociological aspects of the timing of fertility in the United States, as well as in Southeast Asia, and is preparing a monograph, *Age, Time and Fertility*, which summarizes the U.S. work. LARRY BUMPASS is Professor, Department of Sociology, and the Center for Demography and Ecology, University of Wisconsin-Madison. He is researching factors affecting the timing of fertility in the United States, Korea, and the Philippines. He is also studying children's experience with divorce. CRAIG ST. JOHN is a Ph.D. candidate, Department of Sociology, University of North Carolina. His research centers on patterns of residential segregation (concentration) in large urban centers, emerging, over time, as the metropolis expands in scale.

■ RONALD C. KESSLER (Social Class and Psychological Distress) is Assistant Professor, Department of Sociology, University of Michigan. With David Greenberg he is completing a monograph on the methodology of panel analysis. His other work focuses on the structural determinants of psychological well-being and mental illness. He is also working on an analysis of the stress-buffering effects of social support-systems. PAUL D. CLEARY is Assistant Research Professor, Graduate School of Social Work, Rutgers University. His present research concerns psychiatric epidemiology and factors related to the use of prescribed psychotropic drugs.

■ JON MILLER (Access to Interorganizational Networks) is Associate Professor, Department of Sociology and Acting Director, Laboratory for Organizational Research, University of Southern California. His current research concentrates on the effects of race, sex, and age (and their interactions) on the stratification of organizations and interorganizational systems; and on attempts to link treatment performance of social service delivery systems to their positions in community-based networks—an extension of the work published in this issue and being done in collaboration with James R. Lincoln, Indiana University.

■ ERICH WEEDE (Sociological Analyses of Income Inequality) is Associate Professor, Forschungsinstitut für Soziologie, Universität zu Köln. His current research is on Income Inequality and Economic Growth. Recent books: *Weltpolitik und Kriegsursachen im 20.* (Jahrhundert, München 1975); and, together with Dietmar Schössler, *West German Elite Views of National Security and Foreign Policy Issues*. Königstein/ Ts. 1978.

■ DR. CHARLES J. MORGAN (Comment on Quadagno) is the statistician and research design analyst for Region 10 of the Environmental Protection Agency and teaches occasionally in the Department of Psychology, University of Washington. His book, co-authored with Penelope J. Greene, *The Logic of Social Statistics*, is forthcoming from Wiley. His research currently involves (1) environmental quality; (2) computer simulation of natural selection and altruism, with special reference to the social definition of kinship in human societies; and (3) behavioral ecology of Eskimo societies.

■ LOIS M. VERBRUGGE (Comment on Gove and Hughes), is Assistant Professor of Biostatistics and Faculty Associate (Institute for Social Research), University of Michigan. Her chapter, "Sex Differentials in Health and Mortality," in A. H. Stromberg (ed.), *Women, Health, and Medicine*, is being published (1980) in Palo Alto, Calif: Mayfield. She is continuing her research in sex differences in health and mortality . . . particularly recent trends in sex differentials in the United States, and differences in the ways physicians diagnose and treat men and women patients.

■ DAVID MECHANIC (Comment on Gove and Hughes) is Professor of Social Work and Sociology in the Graduate School of Social Work, Rutgers University. His books, *Future Issues in Health Care* and *Mental Health and Social Policy* (2nd.ed.) are forthcoming. He is also researching the determinants of health behavior and psychological impairment.

■ RONNIE STEINBERG RATNER (Comment on Boli-Bennett and Meyer) is Research Director and Visiting Assistant Professor in the Center for Women in Government, SUNY at Albany. Her book *The Growth of Wage and Hour Standards Laws, 1900–1973: A Modest Magna Charta* is forthcoming from Rutgers University Press. She also edited *Equal Employment Policy for Women . . ., 1979,* from Temple University Press. Her work now is on comparative study of equal employment opportunity policy: impact of civil service procedures on occupational location; and the wage disparity, by sex, of public employees. PAUL BURSTEIN is Assistant Professor at Yale University, until June, 1980. His *Politics and Labor Markets: Causes and Consequences of the Passage of Equal Employment Opportunity Legislation* is forthcoming. His research is currently being done on the causes and consequences of the passage of equal employment opportunity legislation; and on social stratification and democratic political change.

BRANDEIS UNIVERSITY

THE FLORENCE HELLER GRADUATE SCHOOL

Applications are invited from individuals interested in full-time faculty appointments for 1980-81 and later, in:

- SOCIAL PLANNING and POLICY ANALYSIS
- SOCIOLOGY, with emphasis on the social context of policy issues
- LAW and SOCIAL POLICY
- ORGANIZATIONAL BEHAVIOR, emphasizing social policy implications

Candidates will be expected to teach core courses and offer additional courses in their specialty areas. They also will be expected to engage in research relevant to their substantive areas. Demonstrated capacity to generate outside sources of support is required. Applicants should have a Ph.D. or its equivalent, be familiar with current social policy issues, and have an interest in participating in a multidisciplinary approach to social policy analysis and social welfare. Rank is open.

The Heller School offers a Ph.D. in Social Welfare and a Master's Degree in Human Services Management. Graduates of the School have assumed high level positions as researchers, planners, policy analysts, managers, and academicians.

Please send applications to Ms. Bibeau, The Heller School, Brandeis University, 415 South Street, Waltham, MA 02254.

An Affirmative Action / Equal Opportunity Employer

THE AMERICAN SOCIOLOGIST

(James L. McCartney, *Editor*)

(Quarterly—ISSN 0003-1232)

The American Sociologist contains major articles analyzing sociology as a profession and as a discipline. Included are papers on such professional concerns as the use of sociological knowledge and skills in academic and non-academic settings; the conditions of work and maintenance of professional standards; the ethical, practical, and intellectual issues related to research; practical problems affecting sociologists as professionals (e.g., taxes, publication, copyright); ideological issues related to the development of new perspectives.

Recent issues have included articles on:

- *Field Work and the Protection of Human Subjects* DUSTER, MATZA & WELLMAN
- *Filthy Pictures or the Case of the Fraudulent Social Scientist: Unmasking the Phony Expert,* ST. GEORGE & McNAMARA
- *Representations of Merton's Theory of Anomie* HILBERT & WRIGHT
- *Universality of Science and the Question of National Sociologies,* HILLER

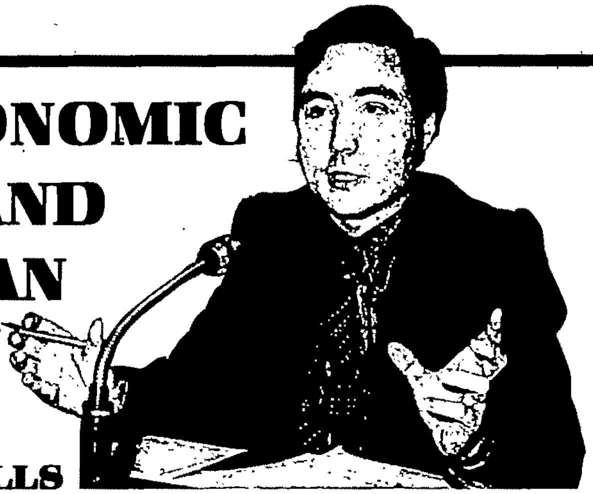
ASA members, $8

Non-members, $12

Single issues, $4

Institutions, $16

Order from:* *American Sociological Association
1722 N Street, N.W.
Washington, D.C. 20036



THE ECONOMIC CRISIS AND AMERICAN SOCIETY

MANUEL CASTELLS

A Marxist critique of American society and economy which pinpoints the new forms of the capitalist mode of production and the contradictory nature of its class relations as the root of our problems. Taking a hard look at the recent crisis afflicting Western economies, Manuel Castells suggests that the very structures that fostered economic growth since 1945 are the same structures that are now undermining these economies. Instead of dogmatic solutions, he proposes a profound transformation of the logic of investment and modification of the power relationships between social classes as eventual ways leading towards a more humane society. Cloth, $18.50. Paper, $5.95

THE MAKING OF A STORMTROOPER

PETER H. MERKL

Singling out members of the most actively militant wing of the Nazi movement, Peter Merkl explores the historical, sociological, and personal factors that motivated their attitudes and behavior. The principal resource for his inquiry is Theodore Abel's collection of 581 autobiographical statements of early Nazis. What emerges is an integrated picture of the motives of the stormtroopers and the circumstances under which they fostered the transition from a fringe movement to a mass movement and hence to the success of Adolf Hitler in taking over the country. Illus. $16.50

PRINCETON UNIVERSITY PRESS
PRINCETON, NEW JERSEY 08540

A Disturbing, Completely Documented Study
of the Invasion of Privacy in America

THE
POLITICS
OF
PRIVACY

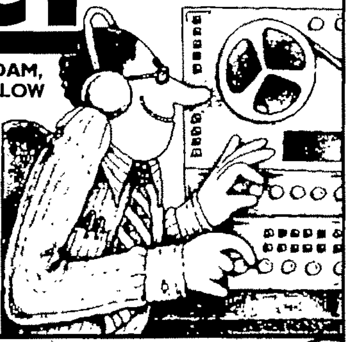
BY JAMES RULE, DOUGLAS McADAM,
LINDA STEARNS AND DAVID UGLOW

The right to privacy in America continues to diminish. The computers of banks, insurance companies, credit rating services, corporations and government agencies hold vital data on the private lives of virtually everyone.

THE POLITICS OF PRIVACY is a fully documented status report on this "Big Brother" phenomenon of modern life and the impact it may have on our society. It is disturbing, frightening, yet absolutely essential reading.

Mentor ME 1829/$2.50

New American Library Education NAL 1633 Broadway
New York, N.Y. 10019



"This book is special

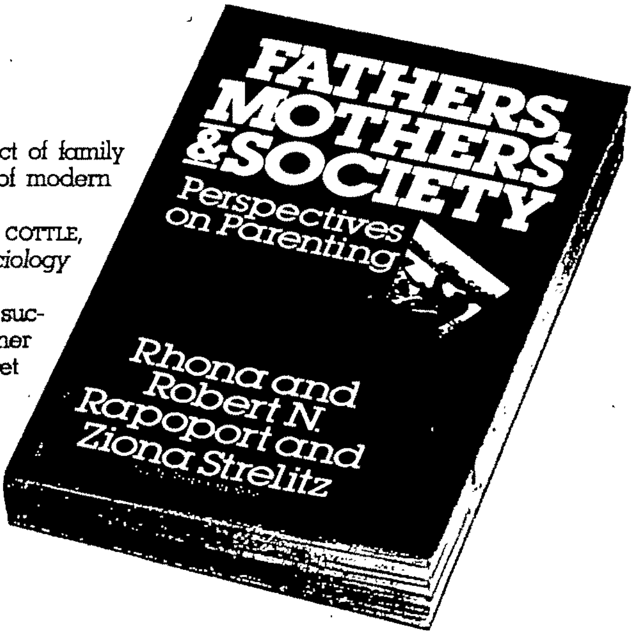
because it traces the effect of family life, the changing nature of modern families on *parents*."

—THOMAS J. COTTLE,
American Journal of Sociology

"An important book which succeeds well beyond any other treatment of the subject yet produced."

—ROSALYN WEINMAN SCHRAM,
Journal of Marriage and the Family

$5.95, now in Vintage paperback
VINTAGE BOOKS
A division of Random House


FATHERS, MOTHERS & SOCIETY
Perspectives on Parenting
Rhona and Robert N. Rapoport and Ziona Strelitz

ROUTLEDGE & KEGAN PAUL

9 Park Street, Boston, Mass. 02108

The Sociology and Politics of Development A Theoretical Study
BAIDYA NATH VARMA City College, City University of New York

In answering the crucial question of how social change should be guided in the developing countries, Professor Varma examines some of the models that have been used in the past. He provides extensive summaries of the views on modernization of theorists in various disciplines, including sociology, politics, economics and anthropology, and focuses on the relevancy of these theories as they relate to decisions affecting the economy, the polity, the educational system, and the bureaucracy in a developing society.
International Library of Sociology $25.00

The Sociology of Belief Fallacy and Foundation
KEITH DIXON Simon Fraser University, British Columbia

Keith Dixon contends that traditional and contemporary formulations of the sociology of knowledge involve a series of fallacies, and argues that the sociology of knowledge and belief needs to give due weight not only to the social and historical location of thought, but to the independently operative logic of various human discourses. **Monographs in Social Theory** $20.00, paper $10.00

Marx and Mead Contributions to a Sociology of Knowledge
TOM W. GOFF Mount Allison University, New Brunswick

It has often been suggested that a resolution of issues generated by the sociological study of ideas might be reached through a synthesis of specific insights to be found in the works of Karl Marx and George Herbert Mead. This study originated in an investigation of this hypothesis, particularly as it bears on the central issues of sociological relativism. **Monographs in Social Theory** $18.50

The Articulation of Modes of Production
Essays from 'Economy and Society'
EDITED BY HAROLD WOLPE University of Essex

This collection of essays is primarily concerned with the concept of articulation of modes of production and with the analysis of a number of different social formations utilizing this concept. The emphasis is on the relationship between capitalist and other modes of production. **Original paperback** $18.50

Lesbians, Women and Society
E.M. ETTORRE Institute of Psychiatry, London

In presenting an analysis of lesbianism as this phenomenon has developed from an 'individual deviance' to a social movement or political practice, this book establishes the links between this specific group of women and women in general, and between lesbians' struggles and the wider fight for women's rights.
$18.95, paper $7.95

CAMBRIDGE

Computers and Social Change

Murray Laver

Laver addresses himself to the often unintended effects of the computer age on education, employment, and leisure, reviewing the social and human consequences of the increased availability of information to governmental and commercial organizations. *Cambridge Computer Science Texts 10*
Hardcover $19.95 Paper $6.95

Spectacles and Predicaments

Essays in Social Theory

Ernest Gellner

Essays deal with the basic images through which we interpret humanity, with the basic nature of knowledge, and with substantive socio-political questions. $34.00

Rational Action

Studies in the Philosophy of the Social Sciences

Ross Harrison, Editor

Martin Hollis, Bernard Williams, Richard Tuck, John Maguire, Ross Harrison and others deal with the concept of rationality and the interrelations among rationality, belief, and desire in the explanation of human action. $19.95

The Tragedy of Enlightenment

An Essay on the Frankfurt School

Paul Connerton

Connerton conveys an overall sense of the continuities and discontinuities in the concerns of representative members of the School, showing how the experience of Fascism shaped their interpretation of modern society as a whole and setting their work in the context of German intellectual history. *Cambridge Studies in the History and Theory of Politics*
Hardcover $24.95 Paper $7.95

Naturalism and Social Science

A Post-Empiricist Philosophy of Social Science

David Thomas

This application of recent developments in the philosophy of science to an understanding of social science includes detailed case studies of such authors as Althusser, Durkheim, Kuhn, and Winch, and areas of work including ethnomethodology, the sociology of magic and religion, and Marxist views of consciousness and revolution. *Themes in the Social Sciences*
Hardcover $32.50 Paper $11.50

The Economics of Individual and Population Aging

Robert L. Clark and Joseph J. Spengler Hardcover $22.50 Paper $7.95

Sociolinguistics

R. A. Hudson Hardcover $34.50 Paper $10.95

Measurement in the Social Sciences

The Link Between Theory and Data

Richard A. Zeller and Edward G. Carmines

In this theoretical and methodological introduction to the measurement techniques used in social science research, Zeller and Carmines thoroughly explain the assumptions, limitations, and interpretations of these techniques as they discuss the ways in which measurement principles can be effectively applied in research. Hardcover $26.95 Paper $7.95

Who Should Know What?

Social Science, Privacy and Ethics

J. A. Barnes

Barnes looks at changes in the conduct of social research since the early nineteenth century, discussing the ethical and political obligations of social scientists in collecting data and publishing their findings. His incisive study reveals the precariousness of the survival of a truly democratic social science. Hardcover $17.50 Paper $4.95

Comrades and Christians

Religion and Political Struggle in Communist Italy

David I. Kertzer

Drawing from his own participant-observation research conducted in a working-class quarter of Bologna, Kertzer effectively blends case studies, individual portraits, and sociopolitical analysis in his examination of the ways in which the people actually experience their divided loyalties, torn between a tradition of anti-clericalism and a Church that decries Communism as incompatible with Catholicism. Hardcover $24.95 Paper $6.95

The Western Isles Today

Judith Ennew

Dr. Ennew traces social changes in the Western Isles of Scotland from the beginning of the nineteenth century to the present. She finds that agricultural practices, political activity, and family life are all affected by the conflict between Gaelic, Puritan, and English norms, and argues that these are not related to the staple sociological divisions between tradition and modernity or state and community. *Changing Cultures* Hardcover $21.50 Paper $5.95

The New International Division of Labour

Folker Fröbel, Jürgen Heinrichs, and Otto Kreye. Translated by Pete Burgess

Studies in Modern Capitalism/Etudes sur le Capitalisme Moderne. A co-publication of *Cambridge University Press* and *Les Editions de la Maison des Sciences de l'Homme* $54.50

Cambridge University Press

32 East 57th Street, New York, N.Y. 10022

The Third World Reassessed

Elbaki Hermassi

Recent developments in the Third World have led to a polarization in scholarship, calling into question theories associated with Development. Hermassi discusses the directions Third World societies are evolving; how the Social Sciences should be applied to societies for which they were not originally designed; and how the Western Social Sciences can be de-parochialized by informing them with the experience of the Third World.
$15.00

Asian and African Systems of Slavery

Edited by James L. Watson

In this comparative analysis, the contributors emphasize indigenous systems of exploitation and their interconnections with the worldwide traffic that developed during the eighteenth and nineteenth centuries. Relatively little has been published on Asian forms of slavery, which are here contrasted with African modes.
$20.00

The Changing Academic Market

General Trends and a Berkeley Case Study

Neil J. Smelser and Robin Content

In recent years there has been a dramatic shift in the demand for academic services. Smelser and Content examine the new dynamics of the academic marketplace and, in particular, the processes of recruitment. To underscore their general analysis, they describe the way the sociology department at the University of California, Berkeley, carried out its search for three junior faculty in 1975-76.
$14.00

The Implementation Perspective

A Guide for Managing Social Service Delivery Programs

Walter Williams

Implementation is a fundamental management problem at all levels of government. Williams presents a new decision-making rationale as a basic guide to program management where the central focus of policy must be the point of service delivery.
$12.50 cloth, $4.50 paper

People Studying People

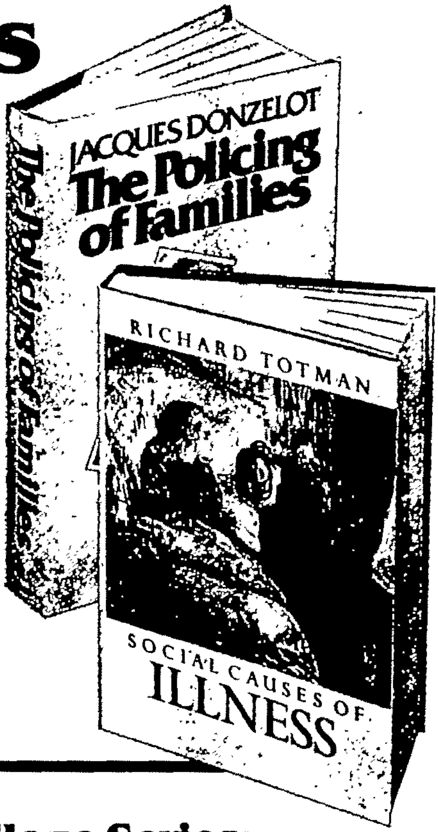
The Human Element in Fieldwork

Robert A. Georges and Michael Owen Jones

Information about the human element in field-based studies is usually subordinated to discussions of fieldwork "methods." The authors develop the thesis that an appreciation of fieldwork as a unique mode of inquiry depends upon an understanding of the role the human element plays in it.
$12.95 cloth, $3.45 paper

University of California Press
Berkeley 94720

Distinguished new books



THE POLICING OF FAMILIES
by JACQUES DONZELOT
"A superb, absolutely indispensable book, which cuts through old debates about the family and helps to put the whole subject on a new footing." —CHRISTOPHER LASCH. "Mr. Donzelot's history is frightening . . . and a stunning indictment."—RICHARD SENNETT, *New York Times Book Review*. Translated by Robert Hurley. $10, cloth; $4.95, paper

SOCIAL CAUSES OF ILLNESS
by RICHARD TOTMAN
"An interesting exploration and review." — *Kirkus Reviews*. "Provocative . . . Totman writes in the iconoclastic tradition of Ivan Illich." —*Publishers Weekly*. $10

The Pantheon Village Series:

An exciting series of quality paperbacks that introduces readers to everyday life in villages around the world, past and present. These volumes "represent literate social science at its best." —*N. Y. Times Book Review*

AKENFIELD
Portrait of an English Village
by RONALD BLYTHE. $3.95, paper

AMOSKEAG
Life and Work in an American Factory City
by TAMARA K. HAREVEN and RANDOLPH LANGENBACH.
$5.95, paper, illustrated

SHINOHATA
A Portrait of a Japanese Village
by RONALD P. DORE.
$4.95, paper

PANTHEON 201 E. 50th St., N.Y. 10022

CHARLES C THOMAS • PUBLISHER

New! **SPIRITUAL WELL-BEING OF THE ELDERLY** *edited by* **James A. Thorson,** *Univ. of Nebraska, Omaha, and* **Thomas C. Cook, Jr.,** *National Interfaith Coalition on Aging, Athens, Georgia. (33 Contributors)* Contributors from such disciplines as psychology, social work, philosophy, theology, and nursing discuss the importance of spiritual well-being to the quality of life of the elderly. Practical applications are related to the research findings presented. *'80, 260 pp., 1 table, $22.50*

New! **THE BOTANY AND CHEMISTRY OF HALLUCINOGENS (2nd Ed.)** *by* **Richard Evans Schultes,** *Harvard Univ., Cambridge, Massachusetts, and* **Albert Hofmann,** *Basel, Switzerland. Forewords by* **I. Newton Kugelmass** *and* **Heinrich Klüver.** This updated edition continues to explore the botanical distribution and chemistry of plants of known, possible, and dubious hallucinogenic potential. Ethnobotanical, historical, pharmacological, and psychological aspects are also covered. *'80, 464 pp., 162 il., 10 tables, $28.75*

SOCIETY'S VICTIM—THE POLICEMAN: An Analysis of Job Stress in Policing (2nd Ptg.) *by* **William H. Kroes,** *Institute for Stress Management, Los Angeles.* This book brings the serious problem of police stress into the public's awareness and offers suggestions for reducing such stress. Chapters cover specific police stressors, the effects of stress, and stress reduction. *'77, 144 pp., 14 il., 4 tables, $12.50*

OLDER PERSONS AND RETIREMENT COMMUNITIES: Case Studies in Social Gerontology *by* **Jerry Jacobs,** *Syracuse Univ., Syracuse, New York.* This comparative ethnographic study of three different kinds of retirement settings explains why patterns of social interaction developed as they did within each setting. Suggestions for improving ineffective interaction are delineated. *'75, 144 pp., cloth-$13.50, paper-$8.75*

New! **THE RIGHTS OF THE MENTALLY RETARDED/DEVELOPMENTALLY DISABLED TO TREATMENT AND EDUCATION** *by* **Issam B. Amary,** *Marshall State School and Hospital, Marshall, Missouri.* This text explains and encourages recognition of rights of the mentally retarded/developmentally disabled to marriage, family life, and social interaction; to due process of law, dignity, respect, and freedom; to personal possessions and proper clothing; and to high quality treatment, comprehensive education, and vocational rehabilitation. Pertinent professional and special services are delineated. *'80, 216 pp., 6 il., $17.50*

WOMEN WHO DRINK: Alcoholic Experience and Psychotherapy *edited by* **Vasanti Burtle,** *Los Angeles. Foreword by* **Phyllis K. Snyder, Jan DuPlain** *and* **George Staub.** *(14 Contributors)* Contributors from a variety of disciplines explain in what ways the female alcoholic's experience differs from that of the male and offer gender-specific therapy suggestions. Chapters cover drinking patterns of women alcoholics, training needs of alcoholism caregivers, women and sex, skid row women, alcoholism among Third World women, family counseling issues, and related subjects. *'79, 304 pp., 3 il., 9 tables, $21.00*

CHANGING SEXUAL VALUES AND THE FAMILY *edited by* **G. Pirooz Sholevar,** *Hahnemann Medical College and Hospital, Philadelphia. (22 Contributors) '77, 192 pp., 5 il., $15.25*

LANGUAGE PATTERNS OF POVERTY CHILDREN *by* **Nicholas J. Anastasiow,** *Indiana Univ., Bloomington, and* **Michael L. Hanes,** *Univ. of Florida, Gainesville.* Theoretical concepts and educational prescriptions are included in this psycholinguistic study of the cultural/dialectal language patterns of poverty children. *'76, 176 pp., 1 il., 4 tables, $15.25*

Orders with remittance sent, on approval, postpaid

301-327 East Lawrence Avenue • Springfield • Illinois • 62717

Why do you behave the way you do?

Social Traps

John G. Cross and
Melvin J. Guyer

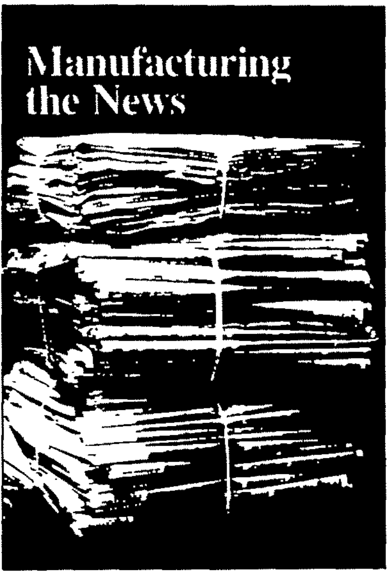
Most of us like to think of ourselves as rational people. Yet we continually do irrational things. We know of the health hazards involved in substance abuse, yet we continue to smoke and drink and overeat. We know the difference between superstition and real danger, yet we refuse to walk under ladders and will not cross the path of a black cat. Just as a mousetrap entices its prey with attractive bait and the promise of reward, so are people in social situations drawn into behavioral traps. And just as the trap destroys the mouse, so does the individual caught in the trap find himself threatened with the consequences of his actions. *Social Traps* is an analysis of our irrational moments, drawn from game theory, economics, and psychological learning theory, offering us a mirror by which to measure our often self-destructive behavior.

paper **$6.50**

The University of Michigan Press

P.O. Box 1104 Ann Arbor, Michigan 48106

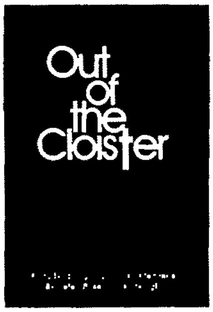
New



Manufacturing the News
By Mark Fishman

News organizations cover the world mainly through "beats," which tend to route reporters through governmental agencies and corporate bureaucracies in their search for news. It is part of the function of such bureaucracies to transform complex happenings into procedurally defined cases, and the information they produce for newsworkers represents their own bureaucratic reality. According to Fishman's research, journalists tend to view this reality as hard fact, while accounts from other sources are treated as unconfirmed reports. Under this system, Fishman concludes, news stories will continue to support the interests of the status quo independently of the attitudes and intentions of reporters, editors, and news sources.
224 pages, $11.95

Also available



Out of the Cloister
A STUDY OF ORGANIZATIONAL DILEMMAS
By Helen Rose Fuchs Ebaugh
"Dr. Ebaugh is exceptionally well qualified for this endeavor. Not only did she spend ten years as a nun, but she is a gifted, imaginative sociologist. . . . Her research is one of the first organizational surveys of female religious life in the country." —*Sociology*
177 pages, $9.95

Citizens for Decency
ANTIPORNOGRAPHY CRUSADES
AS STATUS DEFENSE
By Louis A. Zurcher, Jr. and R. George Kirkpatrick
"[*Citizens for Decency*] is a document that will tell our descendants the history of the bizarre rivalry of our community attitudes regarding the decency of printing and photographing details of human sexuality in our age. . . . of particular pertinence for advanced courses in sociology and, among psychologists, in social psychology and sexology." —*Contemporary Psychology*
428 pages, $7.95, paper

TEXAS

University of Texas Press
POST OFFICE BOX 7819 AUSTIN, TEXAS 78712

HUMAN NEEDS: A CONTRIBUTION TO THE CURRENT DEBATE

Edited by Katrin Lederer, *Science Center Berlin*, in cooperation with Johan Galtung, *United Nations University*, and David Antal, *Science Center Berlin*

Internationally recognized experts evaluate the theoretical, methodological, and practical implications of the concept of human needs. $25.00

INDICATOR SYSTEMS FOR POLITICAL, ECONOMIC, AND SOCIAL ANALYSIS

Edited by Charles Lewis Taylor, *Virginia Polytechnic Institute and State University*

These papers from a conference at the International Institute for Comparative Social Research, Science Center Berlin, review indicator construction and systemization and examine the development and applications of quantitative indicators

ca. $22.50

INDIVIDUAL ENERGY CONSERVATION BEHAVIORS

Paul Beck, Samuel I. Doctors, and Paul Y. Hammond, *University of Pittsburgh*

The results of Project Monitor, a study to determine attitudes, situations, behaviors, and perceptions that influence (or fail to influence) energy conservation.

Available July 1980 ca.$22.50

ENERGY AND HOUSING

Consumer and Builder Perspectives

Edited by Raymond J. Burby and Mary Ellen Marsden, *University of North Carolina*

Analyses of the issues involved in building energy efficient homes from the perspective of the consumer and the builder who are the key decision agents in the process of increasing energy efficiency in the home.

$25.00

Available from the publisher
Telephone: (617) 876-5100

FEAR OF SCIENCE — TRUST IN SCIENCE

Conditions for Change in the Climate of Opinion

Edited by Andrei S. Markovits, *Wesleyan University*, and Karl W. Deutsch, *Harvard University*

These contributions from international scholars, researchers, and politicians examine the relationships between science, scientists, technology, and the public in the past, during the present, and toward the future.

ca. $20.00

DECENTRALIZATION

Toward a Rational Theory

Manfred Kochen, *University of Michigan*, and Karl W. Deutsch, *Harvard University*

This book describes a theory of systematic planning for the future of large service organizations incorporating basic concepts and mathematical models for decentralization and stressing common aspects of different service networks. $27.50

THE CRIMINAL JUSTICE AND MENTAL HEALTH SYSTEMS

Conflict and Collusion

Kent S. Miller, *Florida State University*

This study of the amalgamation of the criminal justice and mental health systems suggests that these events do not reflect a move away from the therapeutic state and that any emerging relationship between the two systems is a benefit to both.

ca. $17.50

Oelgeschlager,
Gunn & Hain, Inc.
Publishers
1278 Massachusetts Ave.
Harvard Square.
Cambridge, MA 02138

OXFORD

THE JEW IN THE MODERN WORLD
A Documentary History

Edited by PAUL R. MENDES-FLOHR, Hebrew University, and JEHUDA REINHARZ, University of Michigan. "An excellent, exhaustive, and many-sided collection." — Jacob Katz. "A rich compilation of sources which illuminate the problems of the modern Jew over the last 200 years.... An impressive work of scholarship." — Gerson D. Cohen. This sourcebook of over 200 primary documents — annotated and placed in their historical context — illuminates social, political, and cultural aspects of Jewish history from the 18th century through the early 1950s. 544 pp., $25.00

THE MIDDLE EAST
A Political and Economic Survey
New Edition

PETER MANSFIELD. Called "a standard reference for students of the Middle East from its first publication in 1950" (**Choice**, on the fourth edition in 1973), this extensive survey covers the geography, natural resources, history, and politics of the Middle East before the Crusades and then concentrates on the current problems of pan-Arabism, Palestine, and Iran and the significance of the Middle East in East West relations. It includes an account of Arab political movements and a detailed analysis of the role of oil in the world economy and international diplomacy.
592 pp., $29.95

A CRISIS OF IDENTITY
Israel and Zionism

DAN V. SEGRE, Haifa University. Tracing the history behind Israel's present political and cultural crises from the nineteenth century to today, Segre challenges the traditional view that judges Israel exclusively in relation to the West. He shows how Israel's position between Western and non-Western poles inhibits its spiritual and cultural independence and says that emancipation through imitation of the West is little more than self-colonization.
192 pp., $14.95

CLASS
Image and Reality in Britain, France, and America Since 1930

ARTHUR MARWICK, The Open University, London. Are class distinctions diminishing or disappearing? In this provocative, wide-ranging historical overview Marwick compares American, French, and British society since the Depression and concludes that in all three, class is still an important factor. Showing how class has changed as a result of economic and technological developments and social and political upheavals, Marwick reveals that since 1930, America has become much more like Europe. 350 pp., $14.95

Prices subject to change.

SINCE 1478

Oxford University Press
200 Madison Avenue • New York, N.Y. 10016

JOURNAL OF
HEALTH & SOCIAL BEHAVIOR

(Howard B. Kaplan, Editor)

(Quarterly—ISSN 0022-1465)

JHSB publishes reports of empirical studies, theoretical analyses, and synthesizing reviews that employ a sociological perspective to clarify aspects of social life bearing on human health and illness, both physical and mental. Its scope includes studies of the organizations, institutions, and occupations devoted to health services, as well as studies of the behavior of actual and potential recipients of these services.

Recent issues contain reports on:

Social Class and the Use of Professional Help for Personal Problems: 1957 and 1976, RICHARD A. KULKA, JOSEPH VEROFF & ELIZABETH DOUVAN

Health Occupations in Israel: Comparative Patterns of Change During Socialization, JUDITH T. SHUVAL AND ISRAEL ADLER

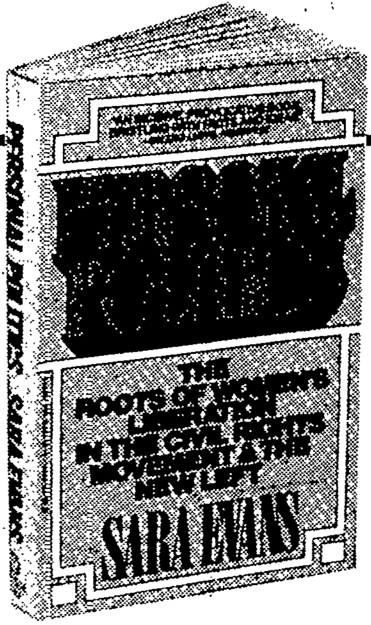
Occupational Stress and Health Among Factory Workers, JAMES S. HOUSE, ANTHONY J. McMICHAEL, JAMES A. WELLS, BERTON H. KAPLAN, & LAWRENCE R. LANDERMAN

The Functional Importance of Sex Roles and Sex-Related Behavior in Societal Reactions to Residual Deviants, WILLIAM A. RUSHING

Stress, Social Status, and Psychological Distress, RONALD C. KESSLER

ASA members, $8 **Non-members, $12** **Institutions, $16**
Single issues, $4

Published by THE AMERICAN SOCIOLOGICAL ASSOCIATION
1722 N Street, N.W. • Washington, D.C. 20036


PERSONAL POLITICS
THE ROOTS OF WOMEN'S LIBERATION IN THE CIVIL RIGHTS MOVEMENT & THE NEW LEFT
SARA EVANS

"An incisive, provocative book, bristling with facts and ideas."

—VALERIE LLOYD, *Newsweek*

"The book's place among the histories of American women should be secure."

—ELINOR LANGER,
N.Y. Times Book Review

"An insider's view of SNCC, voter registration, and SDS's community organizing efforts in Northern ghettos, for which women deserve most of the credit."

—*Library Journal*

$3.95, now in paperback
VINTAGE BOOKS
A division of Random House

JOURNALS OF THE
American Sociological
Association
1722 N Street, N.W.
Washington, D.C. 20036

American Sociological Review
Rita J. Simon, ***Editor***
(Bi-monthly—ISSN 0003-1224)
Articles of major concern to social scientists. New trends and developments in theory and research.
Members, $10; Non-members, $15; Institutions, $30; Non-member students, $10; Single issues, $4

Contemporary Sociology: A Journal of Reviews
Norval D. Glenn, ***Editor***
(Bi-monthly—ISSN 0094-3061)
A journal of reviews, criticism and debate designed to give new thrust and style to reviewing and intellectual exchange within the field.
Members, $10; Non-members, $15; Institutions, $20; Non-member students, $10; Single issues, $4

Journal of Health & Social Behavior
Howard B. Kaplan, ***Editor***
(Quarterly—ISSN 0022-1465)
Distinctive for a sociological approach to the definition and analysis of problems bearing on human health and illness.
Members, $8; Non-members, $12; Institutions, $16; Single issues, $4

Sociology of Education: A Journal of Research in Socialization & Social Structure
Alan C. Kerckhoff, ***Editor***
(Quarterly—ISSN 0038-0407)
A forum for social scientists concerned with research on education and human social development.
Members, $8; Non-members, $12; Institutions, $16; Single issues, $4

Social Psychology Quarterly
George W. Bohrnstedt, ***Editor***
(Quarterly—ISSN 0190-2725)
Genuinely interdisciplinary in the presentation of works by both sociologists and psychologists.
Members, $8; Non-members, $12; Institutions, $16; Single issues, $4

The American Sociologist
James L. McCartney, ***Editor***
(Quarterly—ISSN 0003-1232)
Contains major articles analyzing sociology as a profession and as a discipline.
Members, $8; Non-members, $12; Institutions, $16; Single issues, $4

ASA-SPONSORED PUBLICATIONS

Sociological Methodology

(Samuel Leinhardt, *Editor*)

This annual volume, sponsored by the American Sociological Association, deals with methodological problems in sociology, including data-gathering and analysis, model construction, testing and verification, and the theory of methods. Both quantitative and qualitative approaches are represented.

Recent chapters have included . . . "*Multiway Contingency Analysis with a Scaled Response or Factor*" . . . "*Clustering on the Main Diagonal in Mobility Matrices*" . . . "*The Assessment of 'No Opinion' in Attitude Surveys*" . . . "*Exploratory Data Analysis: An Introduction to Selected Methods*" . . .

Volumes 1969 thru 1975	**$13.50 each**
Volumes 1976 thru 1979	**$15.00 each**
Volume 1980	**$27.50 each**

Order from: **Jossey-Bass, Inc.**
433 California Street
San Francisco, CA 94104

SOCIAL PSYCHOLOGY QUARTERLY

(formerly SOCIOMETRY)

A Journal of Research and Theory in Social Psychology

(George W. Bohrnstedt, Editor)

Genuinely interdisciplinary in the presentation of works by both sociologists and psychologists (Quarterly—ISSN 0190-2725)

Recent issues have dealt with:

Alternative Interpretations (Freese & Rokeach)
Motivational Relevance of Educational Plans (Alexander & Cook)
Why a Rejection? (Wiley, Crittenden & Birg)
Bayesian Model for Performance Expectations (Foschi & Foschi)
Explanations of Black Self-Esteem (Taylor & Walsh)
Varieties of Interpersonal Attraction (Segal)

Special rate to ASA members, $8.00

Non-members, $12 **Institutions, $16** **Single issues, $4**

THE AMERICAN SOCIOLOGICAL ASSOCIATION
1722 N Street, N.W. • Washington, D.C. 20036

Announcing Two Volumes in the
STUDIES IN SOCIAL DISCONTINUITY Series

THE MODERN WORLD-SYSTEM II

Mercantilism and The Consolidation of The European World-Economy, 1600–1750

By IMMANUEL WALLERSTEIN

Critical Praise for The Modern World-System I —"I can say without hesitation that it is the finest book of analytic history that I have read in the last ten years. That Europe had formed a world-economy around herself historians knew, but only in general. What they had never thought about with the keenness and intelligence which characterizes Wallerstein's thought is that this entity provides a new framework for the subject of European history, that it is compelling, a new explanation, a new classification, indeed a revolutionary one, of received knowledge and current thought. . . ." —*FERNAND BRAUDEL*

This second volume of the *Modern World-System* continues the story of the historical development of the capitalist world-economy. Whereas Volume I treated the genesis of the system in the "long" and inflationary sixteenth century, this volume treats the consequences of the economic stagnation of the seventeenth century.

CONTENTS: INTRODUCTION: Crisis of the Seventeenth Century? The B-Phase: Dutch Hegemony in the World-Economy. Struggle in the Core—Phase I: 1651–1689. Peripheries in an Era of Slow Growth. Semipheripheries at the Crossroads. Struggle in the Core—Phase II: 1689–1763. Bibliography. Index.

HARDCOVER EDITION:
1980, 384 pp., $22.00 ISBN: 0-12-785923-3

PAPERBACK TEXT EDITION:
*1980, 384 pp., $9.50 ISBN: 0-12-785924-1(t)**

To obtain examination copies of THE MODERN WORLD-SYSTEM II (Paperback Text Edition) please write to the Sales Department, Academic Press, Inc. 111 Fifth Ave., New York, N.Y. 10003. Requests must include your position, course of instruction, and enrollment.

STUDIES OF THE MODERN WORLD-SYSTEM

Edited by ALBERT BERGESEN

CHAPTER HEADINGS: *A. Bergesen*, From Utilitarianism to Globology: The Shift from the Individual to the World as a Whole as the Primordial Unit of Analysis. *I. Wallerstein*, Imperialism and Development. *R. Wuthnow*, The World-Economy and the Institutionalization of Science in Seventeenth-Century Europe. *R. Wuthnow*, World Order and Religious Movements. *J. Boli-Bennett*, Global Integration and the Universal Increase of State Dominance, 1910-1970. *J. W. Meyer*, The World Polity and the Authority of the Nation-State. *G. M. Thomas and J. W. Meyer*, Regime Changes and State Power in an Intensifying World-State-System. *M. Hout*, Trade Dependence and Fertility in Hispanic America 1900-1975. *C. Chase-Dunn*, The Development of Core Capitalism in the Antebellum United States: Tariff Politics and Class Struggle in an Upwardly Mobile Semiperiphery. *A. Bergesen and R. Schoenberg*, Long Waves of Colonial Expansion and Contraction, 1415-1969. Subject Index.

References appear at the end of each chapter.

1980, 304 pp., $25.00 ISBN: 0-12-090550-7

MEDIATION AND SOCIETY

Conflict Management in Lebanon

By CATHIE J. WITTY

A volume in the STUDIES ON LAW AND SOCIAL CONTROL Series

Mediation and Society is a theoretical and empirical inquiry into the nature of mediation and its relation to people, communities, and social systems. This comparative legal analysis is based on two comparative case studies—one Lebanese and the other American. It develops and illustrates a general theory of mediation and also addresses concrete issues of social and legal planning.

1980, 176 pp., $14.50 ISBN: 0-12-760850-8

Continuation Orders authorize us to ship and bill each volume in a series, or "Advances" type publication automatically, immediately upon publication. This order will remain in effect until cancelled. Specify the volume number or title with which your order is to begin.

ACADEMIC PRESS, INC.

A Subsidiary of Harcourt Brace Jovanovich, Publishers
111 FIFTH AVENUE, NEW YORK, N.Y. 10003
24-28 OVAL ROAD, LONDON NW1 7DX

Announcing

the first contemporary work to examine crime
punishment and social harms
from a libertarian perspective

—CRIMINOLOGY FROM AN ANARCHIST PERSPECTIVE—

THE STRUGGLE TO BE HUMAN: CRIME, CRIMINOLOGY AND ANARCHISM

By

LARRY TIFFT
Central Michigan University

DENNIS SULLIVAN
College of St. Rose

The Struggle To Be Human is a foundational work which attempts to restore to human consciousness a more fundamental understanding of the conditions of social life, maintaining as it does that human survival and continuity are not insured by violence or power or institutions derived from the state, or its certifying professions, but through human competencies and usefullness derived from cooperation and mutual aid.

The Struggle To Be Human emerges amidst the recurring failure of both academic and marxist sociologies to provide insight into ways in which we can live together harmoniously. It links the issues of social harm and punishment to scarcity and the basic human struggle to be free, and provides an incisive critique of the institutions of law, the state and certifying administrative systems. Also, it provides some historical insights into how both academic and marxian analyses, by their insistence upon the centrality of the State, continue to trade away the possibility of present human fulfillment and community for a managed, power-based existence.

now available; 224 pp. $14.00 hardbound (ISBN 0 904564 31 2)

order from
CIENFUEGOS PRESS
Box 105, Mt. Pleasant, Michigan 48858

FIRM FOUNDATIONS.

Outstanding new sociology texts that build students' knowledge.

JAMES WILLIAM COLEMAN
& DONALD R. CRESSEY

SOCIAL PROBLEMS

583 pages. Instructor's Manual.
ISBN 0-06-041326-3.

WILLIAM F. KENKEL

SOCIETY IN ACTION

Introduction to Sociology, *Second Edition*

600 pages. Instructor's Manual. Study Guide. Test Bank. ISBN 0-06-384486-9.

CHAD GORDON
& GAYLE JOHNSON

READINGS IN HUMAN SEXUALITY

Contemporary Perspectives, *Second Edition*

400 pages. Paper. ISBN 0-06-042399-4.

HOWARD J. SHERMAN
& JAMES WOOD

SOCIOLOGY

Traditional and Radical Perspectives

413 pages. Instructor's Manual.
ISBN 0-06-046107-1.

DELBERT C. MILLER
& WILLIAM H. FORM

INDUSTRIAL SOCIOLOGY

Work in Organizational Life, *Third Edition*

834 pages. ISBN 0-06-044471-1.

WILLIAM R. ROSENGREN

THE SOCIOLOGY OF MEDICINE

Diversity, Conflict, and Change

446 pages. ISBN 0-06-045582-9.

EDWIN M. SCHUR

INTERPRETING DEVIANCE

A Sociological Introduction

539 pages. ISBN 0-06-045811-9.

To request examination copies, write to Suite 5D, Harper & Row, 10 East 53d Street, New York, N.Y. 10022. Please include course title, enrollment, and present text.

Harper & Row
New York • San Francisco

DEC 198

American Sociological Review

Dependence, Inequality, and Growth in Less Developed Countries 531
Peter B. Evans and Michael Timberlake

Capitalist Class Impact on Lending Behavior of Banks 553
Richard E. Ratcliff

Sibship Size and Birth-Order Effects on Intelligence 571
Lala Carr Steelman and James A. Mercy

Race and Sex Differences in Career Dynamics 583
Rachel A. Rosenfeld

Class and Income Inequality in Korea 610
Hagen Koo and Doo-Seung Hong

Structure of Attitude Systems in the General Public 627
Charles M. Judd and Michael A. Milburn

Rejoinder to Judd and Milburn 644
Philip E. Converse

How Jews Avoid Alcohol Problems 647
Barry Glassner and Bruce Berg

Parametric Techniques for Contingency Table Analysis 664
Michael Swafford

Research Note
Sex and Involvement in Deviance/Crime Literature 691
Douglas A. Smith and Christy A. Visher

Comments 702
Robert M. Hauser and Andrew M. Greeley

LCUTTA UNIVERSITY

Volume 45 August 1980 Number 4

Notice to Contributors

A processing fee of $10 is required for each paper submitted, except for comments; such fees will be waived for student members of ASA. A check or money order, made payable to the American Sociological Association, should accompany each submission. The fee must be paid in order for the review process to begin.

The length of an *ASR* paper is generally ten (10) printed pages or thirty (30) typed pages. However, the *ASR* is happy to accept manuscripts up to fifty (50) typed pages in length for review. We also are interested in receiving manuscripts for a Research and Notes Section.

To permit anonymity in the review of manuscripts, keep identifying material out of the manuscript. Attach a cover page giving authorship, institutional affiliation and acknowledgments, and provide only the title as identification on the manuscript and abstract.

ASA Multiple Submissions Policy: Submission of a manuscript to a professional journal clearly implies commitment to publish in that journal. The competition for journal space requires a great deal of time and effort on the part of editorial readers whose only compensation for this service is the opportunity to read papers prior to publication and the gratification associated with discharge of professional obligation. For these reasons, *the American Sociological Association regards submission of a manuscript to a professional journal while that paper is under review by another journal as unacceptable.*

SUBMISSION AND PREPARATION OF MANUSCRIPTS

1. Submit four (4) copies and retain the original for your files. Copies may be Xerox, mimeograph or multilith, but not carbons.
2. Enclose a stamped self-addressed postcard for acknowledgment of manuscript receipt. *Manuscripts will not be returned.* Please do not send your original copy.
3. All copy must be typed, doublespaced (including indented material, footnotes and references) on 8½ by 11 inch white opaque paper. Lines must not exceed six (6) inches. Margins must be a minimum of one inch.
4. Include four (4) copies of an abstract of no more than 150 words.
5. Type each table on a separate page. Insert a location note at the appropriate place in the text, e.g., "Table 2 about here." Send copies, retain originals.
6. Figures must be drawn in India ink on white paper. Send copies, retain originals.
7. Clarify all symbols with notes in the margins of the manuscript. Circle these and all other explanatory notes not intended for printing.
8. Footnotes should not be placed in the body of the text. Type them (doublespaced) and attach them as a separate appendix to the text. Number them consecutively throughout the text. Footnotes are to be kept to a minimum and used only for substantive observations. Source citations are made within the text rather than in footnotes.
9. Acknowledgments, credits and grant numbers are placed on the title page with an asterisk.

REFERENCE FORMAT

A. *In the text:* All source references are to be identified at the appropriate point in the text by the last name of the author, year of publication and pagination where needed. Identify subsequent citations of the same source in the same way as the first, not using *"ibid.," "op. cit.," or "loc. cit."* Examples follow:

1. If author's name is in the text, follow it with year in parentheses. [". . . Duncan (1959) . . ."]
2. If author's name is not in the text, insert, in parentheses, the last name and year, separated by a comma. [". . . (cf. Gouldner, 1963) . . ."]
3. Pagination (without "p." or "pp.") follows year of publication after a colon. [". . . Kuhn (1970:71)."]
4. Give both last names for dual authors; for more than two use "et al." in the text. When two authors have the same last name, use identifying initials in the text. For institutional authorship, supply minimum identification from the beginning of the complete citation. [". . . (U.S. Bureau of the Census, 1963:117) . . ."]
5. Separate a series of references with semicolons and enclose them within a single pair of parentheses. [". . . (Burgess, 1968; Marwell et al., 1971; Cohen, 1962) . . ."]

B. *In the appendix:* List all source citations by author, and within author by year of publication, in an appendix titled "References." The reference appendix must be complete and include all references in the text. The use of "et al." is not acceptable in the appendix; list the names of all authors. (See A. 4. for text format.)

If there is more than one reference to the same author and year, distinguish them by the letters, a, b, etc. added to the year. [". . . Levy (1965a:331) . . ."] Give the publisher's name in as brief a form as is fully intelligible. For example, John A. Wiley and Sons should be "Wiley." If the cited material is unpublished, use "forthcoming" with name of journal or publisher; otherwise use "unpublished."

Use no underlining, italics or abbreviations.

Examples follow:

1. *Books:* Jud, Gerald J., Edgar W. Mills, Jr. and Genevieve Walters Burch
   1970 Ex-Pastors. Philadelphia: Pilgrim Press.
   U.S. Bureau of the Census
   1960 Characteristics of Population. Volume 1. Washington, D.C.: U.S. Government Printing Office.
   Bernard, Claude
   [1865] 1957 An Introduction to the Study of Experimental Medicine. Tr. Henry Copley Greene, New York: Dover.
2. *Periodicals:* Conger, Rand
   Forthcoming "The effects of positive feedback on direction and amount of verbalization in a social setting." Pacific Sociological Review.
   Merton, Robert K.
   1963a "The ambivalence of scientists." Bulletin of The Johns Hopkins Hospital 112:77–97.
   1963b "Resistance to the systematic study of multiple discoveries in science." European Journal of Sociology 4:237–82.
3. *Collections:* Davie, M.
   1938 "The pattern of urban growth." Pp. 133–61 in G. Murdock (ed.), Studies in the Science of Society. New Haven: Yale University Press.

See recent issues for further examples.

Revised 1980 ASR

AMERICAN SOCIOLOGICAL REVIEW

Official Journal of the American Sociological Association

AUGUST, 1980 VOLUME 45, NUMBER 4

ARTICLES

Dependence, Inequality, and the Growth of the Tertiary: A Comparative Analysis of Less Developed Countries .... Peter B. Evans and Michael Timberlake 531

Banks and Corporate Lending: An Analysis of the Impact of the Internal Structure of the Capitalist Class on the Lending Behavior of Banks .... Richard E. Ratcliff 553

Unconfounding the Confluence Model: A Test of Sibship Size and Birth-Order Effects on Intelligence .... Lala Carr Steelman and James A. Mercy 571

Race and Sex Differences in Career Dynamics .... Rachel A. Rosenfeld 583

Class and Income Inequality in Korea .... Hagen Koo and Doo-Seung Hong 610

Comment: Converse: The Structure of Attitude Systems in the General Public: Comparisons of a Structural Equation Model .... Charles M. Judd and Michael A. Milburn 627

Rejoinder to Judd and Milburn .... Philip E. Converse 644

How Jews Avoid Alcohol Problems .... Barry Glassner and Bruce Berg 647

Three Parametric Techniques for Contingency Table Analysis: A Nontechnical Commentary .... Michael Swafford 664

RESEARCH NOTE

Sex and Involvement in Deviance/Crime: A Quantitative Review of the Empirical Literature .... Douglas A. Smith and Christy A. Visher 691

COMMENTS AND REPLIES

Comment on Beck et al., *ASR*, October, 1978
On "Stratification in a Dual Economy" .... Robert M. Hauser 702

Reply to Hauser
Social Stratification in Industrial Society: Further Evidence for a Structural Alternative .... E. M. Beck, Patrick Horan, and Charles M. Tolbert, II 712

Comment on Newport Article, *ASR*, August, 1979 .... Andrew M. Greeley 719

Reply to "Comment on Newport Article, *ASR*, August, 1979" .... Frank Newport 720

OTHER

The Editor's Items 721

Editor: RITA J. SIMON

NEW EDITOR

Effective with the February 1981 issue, William H. Form will become the editor of the American Sociological Review. Beginning July 1, 1980, send all manuscripts to
Prof. William H. Form, Department of Sociology, 326 Lincoln Hall,
University of Illinois, Urbana, IL 61801.

Deputy Editors: CLARK McPHAIL AND JOE L. SPAETH

Associate Editors: PAUL BURSTEIN, ROBERT EVAN COLE, THOMAS J. FARARO, BERNARD FARBER, DANIEL GLASER, MICHAEL J. HINDELANG, GISELA HINKLE, ROSABETH KANTER, JAMES R. KLUEGEL, JOHN LOFLAND, MURRAY MELBIN, MICHAEL MOCH, ANTHONY OBERSCHALL, VALERIE K. OPPENHEIMER, JEFFREY PFEFFER, ALEJANDRO PORTES, SAMUEL PRESTON, BARBARA RESKIN, RICHARD RUBINSON, CHARLES W. TUCKER, JOHN WILSON, MAYER N. ZALD, LYNNE G. ZUCKER.

Copy Editor: ELIZABETH ROBINSON NEUMANN

Managing Editor: LINNA McDADE

Editorial Assistant: HELEN CURLEY

Executive Officer: RUSSELL R. DYNES

SCOPE AND MISSION OF AMERICAN SOCIOLOGICAL REVIEW: The *American Sociological Review* publishes work of interest to the discipline in general, new theoretical developments, results of research that advance our understanding of the most fundamental social processes, and important methodological innovations. Like other publications of the Association, emphasis is given to exceptional quality. Unlike the more specialized journals of the Association, the *American Sociological Review's* primary focus is on whatever has the most general bearing on the knowledge of society.

Concerning manuscripts, address: Editor, *American Sociological Review,* Department of Sociology, University of Illinois, Urbana, Illinois 61801. (217) 333-6515.

The *American Sociological Review* (ISSN 0003–1224) is published at 49 Sheridan Avenue Albany, New York, bimonthly in February, April, June, August, October, and December.

Applications for permission to quote from this journal should be addressed to the Executive Office

Copyright © 1980 American Sociological Association.

Annual membership dues of the American Sociological Association, $20–$120 ($10 of this amount may cover a subscription to the *American Sociological Review*); Associate, $25; International Associate, $25; Student Associate, $12.

Application for membership and payment of dues should be made to the Executive Office. Subscription rate for members, $10; nonmembers, $15; institutions and libraries, $30; nonmember students, $10. Single issue, $4.

New subscriptions and renewals will be entered on a calendar year basis only.

Change of address: Six weeks advance notice to the Executive Office and old address as well as new are necessary for change of subscriber's address.

Concerning advertising, changes of address, and subscriptions, address: Executive Office, American Sociological Association, 1722 N Street, N.W., Washington, D.C. 20036.

Claims for undelivered copies must be made within the month following the regular month of publication. The publishers will supply the missing copies when losses have been sustained in transit and when the reserve stock will permit.

Second class postage paid at Washington, D.C. and additional mailing offices.

MEMBERS OF THE COUNCIL, 1980

Officers of the Association

PETER H. ROSSI, President
University of Massachusetts, Amherst
HELEN MacGILL HUGHES, Vice President
Cambridge, Massachusetts
WILLIAM FOOTE WHYTE, President-Elect
Cornell University
RENEE C. FOX, Vice President-Elect
University of Pennsylvania

JAMES F. SHORT, JR. Secretary
Washington State University
HERBERT L. COSTNER, Secretary-Elect
University of Washington
HUBERT M. BLALOCK, JR., Past President
University of Washington
RUSSELL R. DYNES, Executive Officer
American Sociological Association

Elected-at-Large

PAULINE B. BART
University of Illinois, Chicago Circle
NORMAN BIRNBAUM
Amherst College
ERNEST Q. CAMPBELL
Vanderbilt University
ARLENE K. DANIELS
Northwestern University
IRWIN DEUTSCHER
University of Akron
WILLIAM GAMSON
University of Michigan

HELENA Z. LOPATA
Loyola University of Chicago
THOMAS F. PETTIGREW
Harvard University
MORRIS ROSENBERG
University of Maryland
IMMANUEL WALLERSTEIN
SUNY, Binghamton
CHARLES V. WILLIE
Harvard University
MAURICE ZEITLIN
University of Cal., Los Angeles

(Articles in the REVIEW are indexed in the Social Sciences Index, Psychological Abstracts, Sociological Abstracts, Ayer's Guide, University Microfilms, Abstracts for Social Workers, International Political Science Abstracts, and United States Political Science Documents.)

AMERICAN SOCIOLOGICAL REVIEW

DEPENDENCE, INEQUALITY, AND THE GROWTH OF THE TERTIARY: A COMPARATIVE ANALYSIS OF LESS DEVELOPED COUNTRIES*

PETER B. EVANS
Brown University

MICHAEL TIMBERLAKE
Memphis State University

American Sociological Review 1980, Vol. 45 (August):531–552

High levels of economic inequality found in less developed countries have been attributed to the penetration of their economies by investments of multinational enterprises based in more developed nations of the West. This attribution has been widely supported by both historical and quantitative research. There are several interpretations concerning why this might be so, but the one offered here is that foreign investments cause high levels of inequality by distorting the evolution of the labor-force structure. It is suggested that Third World economies penetrated by foreign capital will have unusually rapidly growing proportions of the labor force employed in the tertiary, and it is growth of this proportion which mediates some of the effects of dependence on inequality. Our quantitative analysis of cross-national data (a) corroborates previous research linking dependence to inequality, (b) indicates that dependence is associated with growth of the tertiary, and (c) suggests this is one important link between dependence and inequality.

Watching a superfluous human operator run an automatic elevator is an experience shared by almost everyone who has lived in or visited Third World countries. The experience is more than just a curiosity; it exemplifies a central social dilemma facing Third World countries. The elevator operator's time is valued on the basis of a combination of the "productivity" of the task and the difficulty of finding a replacement. Both are negligible—and so is the wage attached to the job. In consequence, the elevator operator must, in all likelihood, work 70 or 80 hours a week for a wage that does not even insure food and shelter.

The poverty of the elevator operator is, of course, no more severe than that of peasants and agricultural laborers in rural areas, but it is, in at least one sense, more discouraging. It is a poverty situated in the high-rise heart of the "modern" sector of underdeveloped economies. While the proportion of the labor force locked into the poverty of the traditional rural sector can be expected to shrink over time, the proportion located, like the elevator operator, in the urban tertiary sector is likely to grow. The elevator operator is a disturbing example precisely because of the juxtaposition of modernity and poverty.

Other aspects of the elevator operator's situation are also typical and disturbing. The elevator is automatic because the technology that went into its production was created in a developed country where an elevator which really needed an operator would be considered an expensive anachronism. Even if a multinational corporation did not produce the elevator, it almost certainly licensed the technology that was used to produce it. The operator's plight exemplifies one of the dilemmas that poor countries face in trying to deal with the technology produced by rich countries and with the multinational corporations that control this technology. Finally, the elevator operator's situation,

---

* Direct all communications to: Michael Timberlake; Department of Social Relations; Johns Hopkins University; Baltimore, MD 21218.

We would like to thank Lynn Weber Cannon for her helpful advice and comments, and we are grateful for the instructive remarks of several anonymous reviewers. Thanks are also due Sheryl Williams for her help in preparing the tables. The authors have contributed equally to this research; the order of authorship is alphabetical.

like that of domestic servants and others in the tertiary sector, epitomizes the disparities of life chances that are characteristic of societies. Many of the people who ride in the elevator probably make more in a day than the operator does in a month.

The quiet plight of the elevator operator brings together three generally acknowledged features of the current course of development of Third World countries: (1) Much larger proportions of the labor force are employed in the service or tertiary sector than were employed in currently developed countries at similar levels of industrial development. (2) The growth of industry is accompanied by an influx of foreign capital and by increasing dependence on foreign technology. (3) Levels of inequality are substantially in excess of those found in developed countries.

We cannot offer a solution to the elevator operator's plight, but we hope to contribute something to the way in which it is understood. Specifically, we will argue that rapidly growing tertiary sectors, high levels of dependence on foreign investment, and high levels of inequality are empirically associated in Third World countries, or at least were empirically associated during the sixties. The evidence we will present suggests that countries which were hosts to large amounts of foreign investment (relative to their wealth and population) were likely to experience both high levels of inequality and rapid growth of tertiary employment. While we cannot claim that our data are sufficient to prove causal linkages, our findings must certainly be considered reinforcement for those who argue that foreign investment has helped cause high levels of inequality by distorting the distribution of the labor force.

Dependence, Inequality, and the Growth of the Service Sector

Of the connections we will try to make, the one between foreign investment and inequality has been best explored in the literature. It has been argued, on the basis of case studies and theory, that penetration by international capital and dependence on the international market are associated with the generation of an inegalitarian model of development (cf. Dos Santos, 1970; Frank, 1967; Barnet and Muller, 1975; Amin, 1976; Cardoso and Faletto, 1979; Evans, 1979). Beginning with Cutright's (1967) finding that reliance on foreign trade was associated with higher levels of inequality, the assertions of the *dependendistas* have been corroborated by a number of different quantitative studies.

Galtung (1971:110), using a measure of "trade composition," found that reliance on exports of primary products and reliance on trade with a single partner were highly associated with higher levels of sectoral income inequality. Jackman (1975) replicated Galtung's "trade composition" findings. Chase-Dunn (1975) has shown that reliance on foreign direct investment was associated with income inequality and Rubinson (1976) replicated his findings. A recent review of the literature (Bornschier et al., 1978) shows the relation between dependence and inequality to be one of the most robust quantitative, aggregate findings available. Subsequent work continues to support their conclusion (e.g., Hout, 1979).

What is needed is better understanding of the processes which account for the connection between dependence on foreign capital and higher levels of inequality. Earlier theorists of imperialism, like Paul Baran (1968), might have argued that the relationship occurred because foreign capital supported "semifeudal" or *comprador* elites and impeded industrialization. In the seventies it seemed equally plausible to argue, as Bill Warren (1973) did, that foreign investments are likely to be associated with the intensification of capitalist accumulation in the Third World. Since it is generally acknowledged that the highest levels of inequality occur at intermediate rather than at the lowest levels of development (Adelman and Morris, 1973; Paukert, 1973; Cline, 1974), the nature of capitalist accumulation rather than the absence of growth is a more likely place to look for the connections between dependence and inequality.

If dependence exacerbates inequality, it is likely to be because of the *kind* of growth it fosters rather than because it entirely suppresses growth. Among the characteristics of the modern sector, created under the aegis of foreign capital, that might be related to inequality, the growth of the tertiary is a promising possibility.

The relation between the growth of the tertiary and foreign direct investment has been much less thoroughly explored than the relation between dependence and inequality. There are, however, a number of arguments which provide the basis for connecting the two, at least hypothetically. First, foreign direct investment enhances the possibility of the transfer of capital intensive productive techniques. Barnet and Muller (1975:166) put it most forcefully when they say that, "The one characteristic of global corporate technology with the most devastating consequences for poor countries is that it destroys jobs." We do not want to overstate this argument by suggesting that multinational managers can be distinguished from local managers of capital by a perverse, economically irrational proclivity for job-destroying technologies. However, it seems likely that a connection exists between the strength of ties with foreign capital and the existence of a general bias (shared by local firms) toward capital-intensive techniques. At the very least, we agree with Stewart's (1977:87) statement that "within the limited choice that is available more recent and capital intensive techniques are often selected than is strictly necessary."

Capital-intensive industry means that the secondary sector cannot absorb the growing numbers of people leaving the rural areas. In addition, the size of the exodus from the rural areas depends in part on a bias toward the underutilization of labor in agriculture. Numerous studies of Latin American agriculture have demonstrated the relation between the existing organization of agrarian landholdings and inability of agriculture to provide incomes for the rural population (e.g., Feder, 1971; Barraclough and Domike, 1966; Cline, 1970). R. Paul Shaw (1976:20) argues that the organization of agricultural landholdings is "the major cause of a misallocation of human resources away from potentially productive agricultural areas." While underutilization of rural labor is usually attributed to the traditional *latifundia* owners, an equal bias toward excessively "labor saving" agrarian strategies may also be characteristic of multinational firms. Foreign investment in agriculture is likely to result in capital-intensive rural enterprises which absorb relatively little labor themselves and take land away from peasant cultivators. In Brazil in the early seventies, for example, gigantic foreign-owned cattle ranches were set up in the states of Para and Mato Grosso, employing few people and expelling communities of peasant farmers (see Davis, 1978).

Finally, multinational corporations are likely to exacerbate what Todaro (1969) has called the "city lights effect." Todaro suggests that rural dwellers will be willing to migrate to urban *slums* as long as they have the expectation of eventually finding employment that will provide high incomes—relative, that is, to the incomes they know are possible in their present rural living areas. Their expectation—and whether they act on it—obviously depends on what information they receive about the modern sector. Multinationals have a proven effectiveness in generating demand for modern consumer goods even in the poorest, most "traditional" segments of Third World societies (see, e.g., Ledogar, 1975). Armed with an intensified desire for modern consumer goods and an unrealistic media-generated expectation of the extent to which those goods are consumed by city dwellers, the rural citizen becomes an even more likely candidate for migration to the city and entry into a marginal job in the urban tertiary sector.

While foreign investments are only one contributing element in this process, it is not unreasonable to expect that high levels of foreign economic penetration will be associated with more stress on the quest for modern consumer goods which, in the absence of expanding employment opportunities in the secondary sector, will be associated with what Amin (1976:260–70) has labeled the "hypertrophy of the tertiary." It is well established that currently developing countries tend to have larger tertiaries than early industrializers (Timberlake, 1979; Jakobson and Prakash, 1971). Soares (1969:194) notes that, at the point in the industrialization process when about half the labor force was in agriculture, about two-thirds of the rest of the labor force was in the secondary sector in early industrializers, but it is the tertiary sector of currently developing countries that contains the majority of the nonagricultural labor force. What is less well established is how the growth of the tertiary might in turn be related to inequality.

Some would argue against postulating such relationship. Lisa Peattie (1975), for example, has stressed the fact that even in the "informal" service sector of urban slums there are possibilities for making reasonable incomes. Unfortunately, while examples of successful petit bourgeois commercial entrepreneurship in slum communities may be impressive as individual cases, they have little to do with the overall quantitative impact of the growth of the tertiary. A more aggregate level argument might be based on average incomes in tertiary and primary sectors. Since tertiary incomes are generally higher, it could be argued that a transfer of impoverished rural workers from agriculture to the tertiary will increase their wages and, thereby, diminish inequality. This argument, however, does not take into account the importance of the tertiary within sector inequality in shaping the total income distribution. Because the tertiary includes everyone from the most highly paid doctors and lawyers to the most poorly paid domestic servants and street vendors, its growth means the growth of a sector in which income distribution is extremely polarized. One argument connecting the tertiary and inequality would, then, be based on income distribution within the tertiary itself.

There is another way in which the growth of the tertiary may contribute to inequality. The larger the mass of poorly paid, underemployed workers in the tertiary sector, the weaker the bargaining position of those workers who have been fortunate enough to secure jobs in the secondary sector. A poor, unemployed, rural population also provides a "reserve army of labor," but not one that is as readily available as that group, or population, which is employed at the bottom of the tertiary sector. Since tertiary workers are not likely to take jobs away from skilled industrial workers, the principal effect of their presence will be on the bargaining power of unskilled and semiskilled secondary workers. If the bargaining power of the lower ranks of secondary workers deteriorates, then we would expect increased inequality within the secondary sector.

Overall, we postulate that rapid growth of the tertiary is associated with a pattern of development in which the fruits of industrial growth and increased productivity accrue only to a restricted segment of the labor force, resulting in higher levels of inequality among those nations that have been more successful at "dependent development" (see Evans, 1979). This pattern of growth is what Taylor and Bacha (1976:198) refer to as the "unequalizing spiral" in which growth creates a "twist in the Lorenz curve with the poor receiving a smaller (and the rich a larger) share of the proceeds. . . ." In contrast to more optimistic models (e.g., Fei and Ranis, 1964), we agree with Taylor and Bacha (1976:216) that once dependent development has been institutionalized "there seem to be few ways to slow down or reverse the concentration process."

Our argument is not just that investment dependence, inequality, and growth of the tertiary are all components of the "unequalizing spiral" of dependent development. We would like to go further and argue that growth of the tertiary is an important intervening mechanism which mediates the effect of investment dependence on inequality. That is to say, that if investment dependence did not result in rapid growth of the tertiary its effect on inequality would be significantly reduced. In order to show this, of course, we must be able to show not only that investment dependence, growth of the tertiary, and inequality are all interconnected but also that when investment dependence and growth of the tertiary are both included in a model predicting inequality, a significant portion of the effects of investment dependence are not direct, but indirect—via the growth of the tertiary.

The Proposed Analysis

While the relationship between inequality and external economic dependence appears to hold for all nations—even controlling for level of economic development (Chase-Dunn, 1975:727; Rubinson, 1976)—*our* interest is in the developing countries. We are concerned with the ways in which foreign capital *shapes* economic growth. A heavy influx of foreign capital into an economy which was already industrialized should not have the same effects as those which we are postulating.

Our aim, then, is to test a rudimentary version of the line of reasoning which we have laid out. The test results can be only approximate, as we must rely on rather crude indicators of some of our key concepts.

First, we will attempt to replicate the previously discovered relation between foreign investment and inequality, hypothesizing:

(1) The greater the amount of foreign capital located in a given less-developed country, the higher the level of income inequality that will characterize that country.

Next, we would like to examine the relation between foreign investment and relative growth of the tertiary sector, hypothesizing:

(2) The greater the amount of foreign capital located in a given less-developed country, the greater will be the growth of the proportion of the labor force employed in the tertiary sector.

Third, we will examine the relation between growth of the tertiary sector and inequality, hypothesizing:

(3) The greater the increase in the proportion of the labor force employed in the tertiary sector, the greater is the level of income inequality. Furthermore, these overall effects are likely to be the result of the negative impact of the growth of the tertiary on the income of the poor.

Finally, we will examine the combined effect of foreign investment and the growth of the tertiary on inequality, hypothesizing:

(4) When the growth of the tertiary and investment dependence are considered together, the effect of foreign investment on inequality will be reduced, suggesting that some of the effect of foreign investment on inequality is mediated through its effect on the growth of the tertiary.

The Data and Measurement

Dependency. Two measures of economic dependence are used. The first, which we call Investment Dependence I, is "debits on investment income." Chase-Dunn (1975) uses this measure, and he has made it available to us. This measure is taken from the International Monetary Fund *Balance of Payments Yearbook* (1950–1955).

> It reports all profits made by foreign direct investment in the "host" country (regardless of whether or not they are repatriated). The average debits on investment income are computed for the period from 1950 to 1955 . . . and divided by the population. This variable, due to its badly skewed distribution, is converted to a logarithmic scale to make it suitable for use in linear regression analysis. (Chase-Dunn, 1975:728)

Since Investment Dependence I is the product of both the stock of foreign capital and of the rate of return on that capital during the period in question, it is a "flow" measure of investment dependence.[1]

The second measure of economic dependence (Investment Dependence II) consists of the total amount of direct investments by Organisation for Economic Co-operation and Development (OECD) nations in less developed countries. It is taken from an OECD (1972:4) publication.

> This study presents, by host country . . . estimates of the stock of direct private in-

---

[1] While Chase-Dunn's measure does represent accumulated profits over the five-year period, 1950–1955, and is a stock measure in this sense (cf. Bornschier et al., 1978:661), it depends on the rate of profit during this period as well as on the amount of capital accumulated previous to the 1950 to 1955 period; it is, therefore, also a "flow" measure when contrasted to a *purely* stock measure (like our Investment Dependence II).

> vestment of D.A.C. countries in LDCs at the end of 1967. By the stock of foreign investment in this report is understood the net book value to the direct investor of affiliates (subsidiaries, branches and associates) in LDCs.

Like Investment Dependence I, this measure of investment dependence has been divided by host country population and converted to a logarithmic scale. Unlike Investment Dependence I, it is a purely stock measure, representing the long-term accumulation of foreign capital. One advantage of such a measure is that it is relatively stable over time so that measurements from 1967 can be considered good indicators of levels ten years earlier. This feature is important to our analysis, since we must use the measures from the mid-sixties as indicators of earlier levels in order to maximize our number of cases.

There may be some objection to the use of ratio variables such as these since it may be difficult to distinguish the effects of investments from the effects of population (cf. Bollen and Ward, 1979). Effects of the log of investments, controlling separately for the log of population, have been estimated elsewhere (Timberlake, 1979:142–85), with results essentially the same as those presented here. We decided, here, to use per capita measures as population size had usually shown no significant effect on any of the dependent variables and because it would not, therefore, have been useful to include in the path diagrams.

Inequality. Paukert (1973) was the source of income inequality data for most of the cases in our analysis. He presents data showing the percentage distribution of income by households into quintiles from the poorest 20% to the richest 20%, with the richest quintile being further disaggregated into the 81–95 percentile group and the top (richest) 5%. A Gini index computed on the basis of the distribution of income into these quintiles is also presented by Paukert. These data are supplemented with data presented by Jain (1975), thus allowing us to increase the number of cases in the analysis. Of the data Jain presents, we use *only* the results of income surveys of national samples of households or income recipients. The fact that measures of relative size of tertiary labor force are available for two time periods will allow us to make causal inferences from our regression analysis when this is the dependent variable. However, our ultimate concern is understanding variation in levels of income inequality across Third World countries, and inequality data are available only for around 1960 for any significant number of countries. Analyses involving inequality as a dependent variable are therefore cross-sectional and, at best, only suggestive of possible causal ordering.

The tertiary sector. Labor force data have been prepared by Moir (1975; 1976) and made available to us. She has collected data on the distribution of the labor force into economic sectors for many countries for around three time periods: 1950, 1960, and 1970. These data were collected and grouped in accordance with a scheme which classifies as part of the tertiary the following groups of industries: wholesale trade and other nonretail commerce; retail trade; transport; storage and communications; government; armed forces; educational services; health services; other community, business and recreation services; domestic services; and restaurants and other personal services (Moir, 1975:60–2).

These data are grouped in such a way that it is impossible to isolate and measure that part of the tertiary that is "superfluous," even if that were, in principle, possible to determine. We have therefore contented ourselves with using the percentage of the labor force in the whole tertiary, and a measure of change over time in that percentage. We became somewhat less concerned with this shortcoming by discovering that the simple correlation is relatively high (.82) between the percentage in the total tertiary and the percentage in what might be termed the "traditional" tertiary (domestic service, and restaurants and other personal services) among the small number of countries (9) for which the latter measure is available in 1960.

Moir's data for around 1950 and around 1960 are used in the regression analysis. In regression equations in which change in the relative size of the tertiary is to be explained, we use the percentage of the labor force employed in the tertiary industrial groups in 1960 as the dependent variable, with the 1950 measure included as an independent control variable. This allows us to estimate the effects of the other independent variables on the relative size of the tertiary labor force in 1960 independently of its size in 1950. That is, we are estimating effects on *change* in the percentage of the labor force in the tertiary from 1950 to 1960. This method of explaining change has been suggested by Heise (1970) as a method of causal analysis which avoids the pitfalls of using change scores as dependent variables in regression analysis (also see Lord, 1956 and 1958; Bohrnstedt, 1969). Since the actual year in which the labor force data were recorded varies somewhat among the countries, we have also included in these equations the actual number of years that separate the "1950" measure from the "1960" measure. In this way we control for the fact that some tertiary sectors have been given more time to change than others.

We use a measure of change in the tertiary in order to estimate the effects of tertiary growth on levels of inequality. Again, inclusion of early and late measures of the relative size of the tertiary in the regression equation is a method of measuring the effects of change in that variable which is preferable to the use of a simple difference score, since the latter does not fully control for the effects of the initial level of the variable that is changing. However, when this was attempted in estimating the effects of change in the tertiary on inequality, the early and late measures of tertiary employment proved to be too highly intercorrelated to allow meaningful interpretation of the parameter estimates. Instead, we created a change score by computing the difference between the measure of the proportion employed in the tertiary in 1960 and an estimated proportion employed in 1960. The predicted score was computed on the basis of the least squares estimate from the following equation:

$$\hat{Y} = a + b_1X_1 + e \quad (1)$$

where a = constant

e = error

X_1 = proportion of the labor force employed in the tertiary, 1950

$\hat{Y}$ = estimated proportion employed in the tertiary in 1960.

The difference in $\hat{Y} - Y$ is the measure of change in tertiary employment used in the parts of the analysis in which income inequality is the dependent variable and change in the tertiary is one of the independent variables. It is also used in the path analysis. It represents that part of tertiary employment in 1960 that cannot be attributed to the level of tertiary employment in 1950. Since this residualized measure is, by definition, statistically independent of the early measure of tertiary employment, it is superior to the measure of difference between actual 1960 and actual 1950 tertiary employment.

Level of economic development. Per capita domestic product is included in regression equations in order to control for the possible effects of differences in the level of economic development within our sample of nations. The GDP estimates of Hagen and Hawrylyshyn (1969) are used. We use a 1960 measure, rather than an earlier one, in order to avoid further reducing the number of cases included in the tables. This should not be problematic since such indicators of development level are very highly correlated over time.

The cases. Since our concern is with the effects of foreign investment on industrializing nations, we eliminated all those nations with per capita incomes higher than $950 in 1965. Japan was also excluded. The effect of using a strict income criteria was to exclude most of the OPEC countries and include some of the poorer countries of Western Europe. In addition, lack of data eliminated a number of nations which would have otherwise been included. The "sample" consists of 56 poor countries. Among these 56 countries, very few have data on every one of our variables. In order to allow the broadest possible test of each of our hypotheses, we have included in each table all countries in the sample for which data were available for the variables under consideration in that table. The number of cases ranges between 49 and 18, except that the parameters of the path diagrams are estimated for two sets of "common cases" with 18 and 21 countries, respectively (see Appendix C and Appendix D). The total sample, and the cases used in each table, are shown in Appendix A.

Analysis of Results

The positive relation between investment dependence and inequality in poor countries is thoroughly reconfirmed in Tables 1a and 1b. In 1a we have used Chase-Dunn's measure of dependence and shown that in our sample both the strength of the effect of investment dependence on inequality and the statistical significance of the relation are even greater than they were in Chase-Dunn's original sample (cf. Chase-Dunn, 1975:734). Investment dependence is not only significantly related to the overall Gini index. It also has strong negative effects on the income shares of all three of the lower quintiles and a strong positive effect on the income of the top 5% of the population.

Using our other measure of investment dependence enables us to replicate these findings with a larger number of cases. Since this other measure of investment dependence is simply a measure of the amount of capital invested in a given less developed country and does not depend on the rate of profits obtained in that country, Investment Dependence II provides a good check on the possibility that the relation discovered by Chase-Dunn was due to effects associated with differential rates of profit rather than those associated with different total amounts of foreign investment. Tables 1a and 1b indicate that whether one uses a "flow" measure like Chase-Dunn's or a "stock" measure like Investment Dependence II, foreign investment is associated with inequality. The relation between Investment Dependence II and the Gini index is strong and significant, as are the negative effects on the incomes of the lower 80% of the population and the positive effects on the upper 5%.

These findings indicate that when foreign investment is relatively great in LDCs, then overall income inequality is also relatively great. Furthermore, these statistical effects on overall inequality appear to result from both a negative relationship of investment dependence with the relative amount of income held by the less well-off and its positive relationship with the proportion of income accruing to the rich. The data used here are cross-sectional, thus disallowing causal inference, but they are consistent with the theoretical argument that, in Third World countries, dependence on the capitalist core helps keep the poor impoverished while the rich get richer.

Impressed by these findings but remaining skeptical, we tried to explore the possibility that the relation might be spurious by introducing various additional variables into the equation. We thought, for example, that introducing population growth into the equation might diminish the relation between dependence and inequality. The relation held up well in the face of our experiments. For example, when population growth rate 1950–1960 was included in the regression equation that produced the results shown in Table 1a, the Beta associated with it was small (.013) and did not approach statistical significance. Furthermore, the estimated effect of Investment Dependence I was not reduced in either substantive or statistical significance (Beta = .671). We must conclude that, in the absence of strong future findings to the contrary, reliance on foreign investment must be considered to have negative statistical effects on equality in poor countries.

Next, we examine the effects of dependence on foreign capital on the change in relative size of the tertiary sector of the labor force. As Tables 2a and 2b show, both our measures of investment dependence have significant positive effects on growth of the tertiary. The more a poor country was dependent on foreign capital, the more rapidly the proportion of the labor force in its tertiary sector was likely to expand between 1950 and 1960.

The same is not true of the labor force in the secondary sector. When the same procedures used to test the relation between

Table 1a. Regressions of Gini Index of Income Inequality and Proportion of Total National Income Accruing to Each of Six Percentile Groups of the Income Distribution on Investment Dependence I and Per Capita GDP

Dependent Variables / Independent Variables		Gini Index of Income Inequality	% Income to Poorest 20%	% Income to 21–40% Grouping	% Income to 41–60% Grouping	% Income to 61–80% Grouping	% Income to 81–95% Grouping	% Income to Richest 5%
	a	31.514	9.007	12.875	16.262	20.990	23.113	17.746
Investment Dependence I	b	8.824**	−2.141**	−2.312**	−2.039**	−1.324	1.808	5.986**
	Beta	.671	−.720	−.616	−.533	−.292	.284	.498
	s.e.	2.505	.542	.736	.798	1.038	1.460	2.544
	F	12.41	15.58	9.86	6.52	1.63	1.53	5.53
Per Capita GDP	b	−.012	.003	.002	.003	.004	−.004	−.007
	Beta	−.178	.184	.125	.139	.160	−.143	−.111
	s.e.	.012	.003	.004	.004	.005	.007	.012
	F	.87	1.02	.41	.45	.49	.39	.26
	R^2	.35	.40	.31	.22	.06	.06	.20
	n	29	29	29	29	29	29	29

Notes: ** = statistically significant to at least the .05 level.

a = intercept

b = regression coefficient

Beta = standardized regression coefficient

s.e. = standardized error of regression coefficient

F = F ratio

Table 1b. Regressions of Gini Index of Income Inequality and Proportion of Total National Income Accruing to Each of Six Percentile Groups of the Income Distribution on Investment Dependence II and Per Capita GDP

Independent Variables	Dependent Variables	Gini Index of Income Inequality	% Income to Poorest 20%	% Income to 21–40% Grouping	% Income to 41–60% Grouping	% Income to 61–80% Grouping	% Income to 81–95% Grouping	% Income to Richest 5%
	a	37.886	7.560	11.007	14.640	20.694	24.370	21.752
Investment Dependence II	b	5.088**	−.811**	−1.183**	−1.463**	−1.594**	−.124	5.137**
	Beta	.786	−.585	−.715	−.766	−.741	−.033	.839
	s.e.	1.106	.246	.293	.334	.379	.768	1.023
	F	21.15	10.89	16.35	19.23	17.68	.03	25.23
Per Capita GDP 1960	b	−.029**	.001	.006**	.010**	.013**	.009	−.040**
	Beta	−.440	.094	.366	.532	.596	.224	−.630
	s.e.	.041	.002	.003	.003	.004	.008	.010
	F	6.62	.28	4.28	9.26	11.43	1.21	14.22
	R^2	.32	.27	.28	.29	.28	.04	.36
	n	49	49	49	49	49	49	49

See notes to Table 1a.

investment dependence and the growth of the tertiary were repeated for the secondary sector, neither measure showed a significant positive relation. The relation was negative and almost significant in one case and insignificant in the other. The findings were, in short, what would be expected from our theoretical discussion of the effects of foreign capital. Foreign capital may draw a poor country's population into the market economy and into the nonagricultural labor force, but it does not appear to play a significant role in the expansion of employment opportunities in the secondary sector.

Turning to the relation between labor force structure and inequality, we found that only change in the tertiary—and not its size relative to other sectors of the labor force at any given point in time—was related to inequality. We examined the relation between various cross-sectional measures of labor force structure and inequality and, while there were a number of suggestive findings, none of them was sufficiently robust to merit real confidence. We did, however, discover a strong and consistent relation between the growth of the tertiary and level of inequality.

Those poor countries in which the tertiary had grown unusually rapidly between 1950 and 1960 had higher levels of inequality than those poor countries whose tertiaries had grown less. These results are shown in Table 3. Table 3 indicates that the growth of the tertiary has strong and statistically significant effects on overall levels of inequality, and its most powerful effects are on the income shares of the bottom 40% of the population.

It is important to emphasize at this point that what we have found is not simply a relationship between the growth of the nonagricultural labor force and of inequality. Obviously, the growth of the tertiary goes together with a decline of the proportion of the labor force in agriculture. But it is not the movement of labor out of agriculture, in itself, that is related to growing inequality. What is important is that this labor must be absorbed into the tertiary rather than into the secondary sector. We have already shown that there

Table 2a. Regression of % in Tertiary Sector of Labor Force 1960 on That % 1950, Investment Dependence I, Per Capita GDP, and Time Lag

Dependent Variables Independent Variables	Regression Coefficient (b)	Standardized Regression Coeff. (Beta)	Standard Error of b	F Ratio
Investment Dependence I	.032**	.261	.007	18.06
% in Tertiary 1950	.635**	.683	.072	77.36
Per Capita GDP	.000**	.201	.000	5.42
Time Lag	.002	.032	.003	.37
R^2 = .95				
n = 24				

** = −.008.

Also see notes to Table 1a.

is no association between the growth of the secondary sector and higher levels of inequality. It is true that a shrinking primary sector is associated with a smaller income share for the poorest quintile. But when we included both change in the tertiary and level of primary employment (in the equation predicting the Gini index and share of the poorest quintile), the effects of level of primary employment were reduced to statistical insignificance.[2]

One other alternative hypothesis should also be rejected. Since the results shown in Table 3 are essentially cross-sectional, it might be argued that the relationship we have found exists because relatively rapidly growing tertiary sectors are the result of income inequality. Ideally, we would need inequality data as well as tertiary data for two points in time in order to determine the direction of causation in the relationship between the tertiary and inequality, but we have inequality data for only around 1960. However, since we have measures of the relative size of the tertiary for 1970 (as well as for 1960), we can estimate the effects of income inequality in 1960 on change in the tertiary from 1960 to 1970. When we did this, we found that level of inequality in 1960 had no significant effects on growth of the tertiary from 1960 to 1970 (Beta = −.077). Thus, it seems reasonable to view, tentatively, the relationship between inequality and growth of the tertiary as being the result of the impact of the tertiary on the income distribution, rather than vice versa.

The final step in our analysis was to reexamine the relation between investment dependence and inequality, introducing the growth of the tertiary as an additional variable in the regression equa-

Table 2b. Regression of % in Tertiary Sector of Labor Force 1960 on That % in 1950, Investment Dependence II, Per Capita GDP, and Time Lag

Dependent Variables Independent Variables	Regression Coefficient (b)	Standardized Regression Coeff. (Beta)	Standard Error of b	F Ratio
Investment Dependence II	.018**	.298	.006	8.35
% in Tertiary 1950	.688**	.664	.094	53.09
Per Capita GDP	.000	.077	.008	.34
Time Lag	.004	.084	.003	1.99
R^2 = .93				
n = 26				

** = −.024.

Also see notes to Table 1a.

---

[2] The addition of a measure of change in primary employment into the equation that produced the results showing the effects of tertiary change on the Gini index and income share of the poorest quintile (Table 3) reduced the magnitudes of the tertiary effects slightly (Beta = .506, F = 3.35, and Beta = −.612, F = 5.50, respectively). However, the effects of change in primary employment were rather small and statistically insignificant (Beta = −.205, F = .59, and Beta = .107, F = .18, respectively).

Table 3. Regressions of Gini Index of Income Inequality and Proportion of Total National Income Accruing to Each of Six Percentile Groups of the Income Distribution on Change in the Tertiary and Per Capita GDP

Independent Variables	Dependent Variables	Gini Index of Income Inequality	% Income to Poorest 20%	% Income to 21–40% Grouping	% Income to 41–60% Grouping	% Income to 61–80% Grouping	% Income to 81–95% Grouping	% Income to Richest 5%
Change in Tertiary 1950–1960 (Residualized Change Score)	a	49.671	4.887	7.814	11.675	18.392	29.825	27.274
	b	151.611**	−37.134**	−39.325**	−25.750	−6.404	19.340	89.777*
	Beta	.656	−.691	−.633	−.353	−.066	.123	.435
	s.e.	44.622	9.690	12.226	16.909	23.539	37.388	45.245
	F	11.54	14.68	10.34	2.32	.07	.27	3.94
Per Capita GDP	b	−.007	.000	.002	.002	.003	−.009	.002
	Beta	−.139	.030	.138	.116	−.158	−.266	.004
	s.e.	.010	.020	.003	.004	.005	.008	.010
	F	.52	.03	.49	.25	.42	1.25	.03
	R^2	.38	.46	.36	.11	.02	.06	.20
	n	22	22	22	22	22	22	22

Notes: * = statistically significant to at least the .10 level.
Also see notes to Table 1a.

tion. A range of results from this addition would be consistent with our theoretical model. Growth of the tertiary was considered to be an intervening variable and, therefore, more causally proximate to inequality. But we also assumed that other mechanisms, such as those discussed by Chase-Dunn and Rubinson, would operate and might, for some countries, be more important than effects which operated through labor force structure. Thus, investment dependence might continue to have a strong relation with inequality even when the growth of the tertiary was included in the regression equation, insofar as this relation represented effects mediated through other mechanisms. If the effects of investment dependence were reduced to insignificance, it would suggest that, at least for the sample used, growth of the tertiary was the principal intervening variable connecting investment dependence and inequality. Only if the growth of the tertiary ceased to have any effect on inequality once investment dependence was introduced into the equation, would the results have been inconsistent with our theoretical model.

The actual results, shown in Tables 4a and 4b, are quite favorable to our hypothesis. In both tables, the growth of the tertiary continues to have (substantively) rather strong effects on the Gini index, even with investment dependence included in the equation. The effects of change in the tertiary on income accruing to the poorest quintile are negative, as predicted, and moderately strong, but its effects are statistically significant only in the equation with Investment Dependence II (Table 4b). If we restrict our attention to the effects on the overall index of inequality and the income share of the poorest quintile, it is important to note that the effects of the investment-dependence variables were reduced to statistical insignificance, in each case. The magnitudes of the effects of the investment dependence variables are, likewise, dramatically reduced—compared with those estimated for the subsets of 18 and 21 cases common to Tables 4a and 4b, respectively (see Appendix C). These findings indicate that growth of the tertiary does mediate some of the previously

Table 4a Regressions of Gini Index of Income Inequality and Proportion of Total National Income Accruing to Each of Six Percentile Groups of the Income Distribution on Investment Dependence I, Change in Tertiary, and Per Capita GDP

Dependent Variables Independent Variables		Gini Index of Income Inequality	% Income to Poorest 20%	% Income to 21–40% Grouping	% Income to 41–60% Grouping	% Income to 61–80% Grouping	% Income to 81–95% Grouping	% Income to Richest 5%
	a	46.207	6.799	8.313	12.062	18.969	29.401	24.482
Investment Dependence I	b	1.361	−1.260	−.145	.264	.466	−.374	1.030
	Beta	.106	−.448	−.042	.072	.100	−.058	.093
	s.e.	3.864	.837	1.082	1.268	1.841	2.450	3.786
	F	.12	2.26	.02	.04	.06	.02	.07
Change in Tertiary 1950–1960	b	164.587**	−23.220	−44.839**	−41.060*	−26.862	47.010	89.403
	Beta	.639	−.410	−.648	−.557	−.287	.365	.403
	s.e.	70.633	15.308	19.783	23.171	33.653	44.788	69.214
	F	5.43	2.30	5.14	3.14	.64	1.10	1.67
Per Capita GDP	b	−.006	.004	.001	−.002	−.003	−.009	.008
	Beta	−.104	.337	.096	−.097	−.132	−.320	.159
	s.e.	.013	.003	.004	.004	.006	.008	.010
	F	.20	2.23	.16	.14	.19	1.21	.37
	R^2	.47	.48	.42	.30	.08	.14	.31
	n	18	18	18	18	18	18	18

See notes to Tables 1a and 3.

Table 4b. Regressions of Gini Index of Income Inequality and Proportion of Total National Income Accruing to Each of Six Percentile Groups of the Income Distribution on Investment Dependence II, Change in Tertiary, and Per Capita GDP 1960

Dependent Variables Independent Variables		Gini Index of Income Inequality	% Income to Poorest 20%	% Income to 21–40% Grouping	% Income to 41–60% Grouping	% Income to 61–80% Grouping	% Income to 81–95% Grouping	% Income to Richest 5%
	a	45.064	5.554	8.37	12.926	21.962*	29.260	21.928
Investment Dependence II	b	2.858	−.451	−.414	−.759	−1.858	.135	3.332*
	Beta	.544	−.380	−.310	−.446	−.910	.040	.728
	s.e.	1.898	.406	.494	.781	.916	1.701	1.878
	F	2.27	1.27	.70	.94	4.12	.00	3.15
Change in Tertiary 1950–1960	b	113.684**	−31.580**	−34.174**	−15.491	20.302	17.338	44.327
	Beta	.511	−.629	−.605	−.215	.235	.118	.229
	s.e.	49.362	10.417	12.844	20.313	23.813	44.222	48.826
	F	5.30	9.19	7.08	.58	.73	.15	.82
Per Capita GDP	b	−.029*	.004	.006	.008	.013*	−.008	−.024
	Beta	−.597	.394	.480	.473	.700	−.232	−.552
	s.e.	.016	.003	.004	.006	.008	.014	.015
	F	3.59	1.79	2.21	1.39	3.20	.29	2.36
	R^2	.48	.55	.45	.16	.20	.04	.33
	n	21	21	21	21	21	21	21

See notes to Tables 1a and 3.

disclosed effects of investment dependence.

A somewhat more elegant way of illustrating the same argument with these findings is to use path diagrams to display the results of the common case analyses. We have done this, focusing on the Gini index of income inequality. Our fourth hypothesis suggests that change in the tertiary mediates some of the effects of dependency. To test this argument, we need to decompose the total effect of dependency on income inequality. Path analytic techniques provide a way to state this total effect in such a way that the direct and indirect effects can be distinguished.[3]

Figures 1a and 1b, one for each of the two dependency measures, represent path diagrams of the model we are suggesting. The indirect effect of Investment Dependence I on the Gini index through the growing tertiary ($p_{43}p_{31} = .457$) is more than four times greater than the direct effect of this measure of dependency ($p_{41} = .106$). As the direct effect is the only other positive coefficient, one could argue that more than 80% of the positive direct and indirect effects of Investment Dependence I on the Gini index operate through its effects on the bloating tertiary. The results obtained when we compute the direct and indirect effects of Investment Dependence II on the measure of income inequality from Figure 1b are not as dramatic, but they also support the hypothesis that the exacerbating effects of dependence on levels of inequality are mediated by the effects of investment on the tertiary. In this case the mediated effects ($p_{43}p_{31} = .4425$) are somewhat smaller than the direct effects of Investment Dependence II ($p_{41} = .5440$) and they account for about 45% of the total positive direct and indirect effects of dependency ($p_{43}p_{31} + p_{41} = .9865$) on the Gini index. The results shown in the two path diagrams and discussed here provide rather strong support for viewing the growth of the tertiary as an important way in which dependence adversely affects the income distribution.

Discussion

The findings presented here must be considered strong empirical reinforcement for the idea that reliance on foreign capital is likely to contribute to the inequality of the distribution of incomes in poor countries. We have not only replicated Chase-Dunn's and Rubinson's results

---

[3] A discussion of the way in which the path coefficients were estimated can be found in Appendix D.

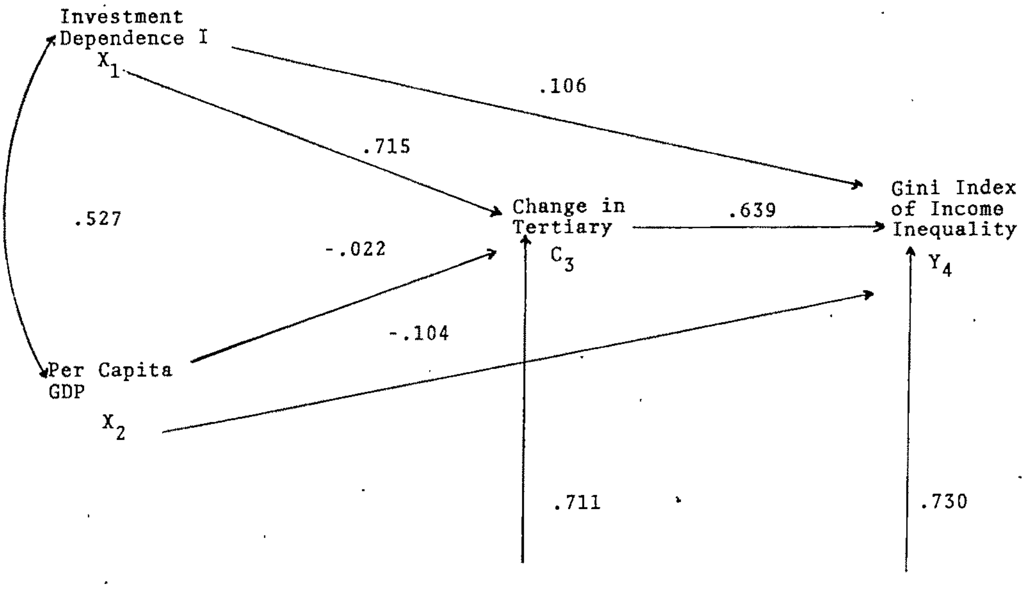


Figure 1a. Path Diagram of Final Model with Investment Dependence I

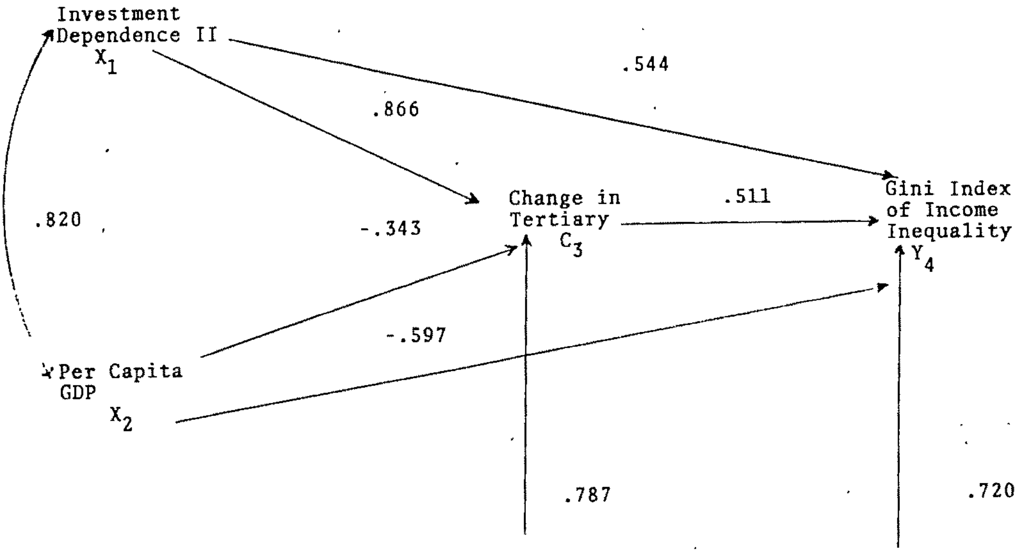


Figure 1b. Path Diagram of Final Model with Investment Dependence II

with additional inequality data and confirmed them, but also tested the hypothesis using a conceptually different measure of investment dependence. While Chase-Dunn's measure of dependence is a "flow" measure—that is, one which indicates the flow of profits from foreign-owned investors during a given period—our second measure of investment is a "stock" measure. That is, it indicates the total stock of foreign investment which has been accumulated in a given country, over time. The fact that the relation between investment dependence and inequality can be confirmed for both flow and stock measures makes the findings all the more convincing.

The credibility of the relation has also been increased because we were able to test it across a broader sample of poor countries than in any previous investigation. Using our second measure of dependency, we were able to include almost 50 less-developed countries in our sample. Given our findings, it is very difficult to argue that the relation between investment dependence and inequality is due to the peculiarities of a restricted subset of Third World countries. It may still be the case that foreign capital is associated with inequality only during a specific historical period. But it seems safe to say that, at least during the sixties, high levels of external economic dependence went along with high levels of inequality throughout the Third World.

We found a fairly strong, positive relation between the growth of the tertiary and our measures of inequality and a clear relation between reliance on foreign capital and the growth of the tertiary sector. Our data also support the idea that the growth of the tertiary can be considered one of the mechanisms through which reliance on foreign capital is related to higher levels of inequality.

Furthermore, we have found that growth of the employment in the secondary sector is not related to higher levels of inequality and that high levels of dependence on foreign investment are not associated with rapid growth of employment in the secondary sector. In short, dependence is not associated with inequality simply because dependence is associated with the growth of the modern sector; the relation between dependence and inequality is a function of the *kind* of modern sector that is created.

The findings presented here have several implications for students of inequality in developing countries. For those trying to elucidate the relation between investment dependence and inequality, our results reinforce the idea that foreign investment exacerbates inequality not because it prevents capitalist accumulation in the Third World but because the capitalist accumulation it fosters is so strongly exclusionary and inegalitarian. For those who are interested primarily in labor force structure, the main contribution of our findings is to suggest that variables not often considered in such studies, like investment dependence, may be of significant help in explaining differences in labor force structure among countries at similar levels of development. It would be most interesting, for example, to discover whether other kinds of dependence, like trade dependence and aid dependence, have similar consequences for the growth of the tertiary.

More immediately, the empirical confirmation of the connection between foreign investment and the growth of the tertiary makes essential further exploration of the reasons for this connection. So far, it has been assumed that foreign investors may influence the shape of the labor force by choosing different technologies of production than local capitalists would. Other kinds of influence deserve more attention. Nontechnological effects of penetration by foreign capital on the labor-absorbing capacity of the agrarian sector deserve further attention. So does the hypothesized tendency of the multinational to exacerbate the "city lights effect."

For those interested in policy rather than in further research, the results do not offer too much in the way of positive suggestions, but may provide some useful suggestive information. On the one hand, they supply yet another piece of evidence against the assumption that the growth of the modern sector will diminish inequality. On the other hand, they suggest that reducing investment dependence will have an impact on inequality only if the social structural consequences of past dependence are also dealt with. If we are correct in assuming that most of the influence of investment dependence is mediated through changes in underlying social structure, the resultant inequality will persist long after the last subsidiary is nationalized. But this is, of course, what the elevator operator has suspected all along.

APPENDIX A
LIST OF COUNTRIES IN THE ANALYSIS

Country	Appears in Table.						
	1a	1b	2a	2b	3	4a	4b
Latin America							
1. Dominican Republic			X	X			
2. Jamaica		X		X	X		X
3. Trinidad and Tobago		X		X	X		X
4. Mexico	X	X	X	X	X	X	X
5. Guatemala			X	X			
6. Honduras	X	X	X	X	X	X	X
7. El Salvador	X	X	X	X	X	X	X
8. Nicaragua			X	X			
9. Costa Rica	X	X	X	X	X	X	X
10. Panama	X	X	X	X	X	X	X
11. Colombia	X	X	X	X	X	X	X
12. Ecuador	X	X	X	X	X	X	X
13. Peru	X	X					
14. Brazil	X	X	X	X	X	X	X
15. Bolivia	X	X					
16. Paraguay			X	X			
17. Chile	X	X	X	X	X	X	X
18. Argentina	X	X	X	X	X	X	X
19. Uruguay	X	X					
Europe							
20. Ireland			X	X			
21. Spain		X			X		X
22. Portugal			X	X			
23. Yugoslavia	X	X					
24. Greece	X	X	X	X	X	X	X
Africa							
25. Senegal		X					
26. Dahomey		X					
27. Niger		X					
28. Ivory Coast		X					
29. Sierra Leone		X					
30. Nigeria		X					
31. Gabon		X					
32. Chad		X					
33. Kenya		X					
34. Tanzania		X					
35. Zambia		X					
36. Rhodesia		X					
37. Malawi		X					
38. South Africa	X		X	X	X	X	
39. Malagasy Republic		X					
40. Morocco		X			X		X
41. Tunisia		X					
42. Sudan	X	X					
Middle East							
43. Turkey	X	X					
44. Iraq	X	X					
45. United Arab Republic	X	X	X	X	X	X	X
46. Lebanon	X	X					
Asia							
47. Taiwan	X	X					
48. Hong Kong		X					
49. South Korea		X					
50. India	X	X	X	X	X	X	X
51. Pakistan	X	X	X	X	X	X	X
52. Burma	X	X					
53. Sri Lanka	X	X	X	X	X	X	X
54. Nepal							
55. Thailand	X	X	X	X	X	X	X
56. Philippines	X	X	X	X	X	X	X
57. Indonesia	X	X					
Total Appearances	29	49	24	26	22	18	21

APPENDIX B

CORRELATION MATRIX OF VARIABLES IN THE ANALYSIS (MAXIMUM CASES)

		1	2	3	4	5	6	7	8	9
1. % in	r	1.000	.931*	.078	−.118	.805*	−.164*	−.101	.392*	.568*
Tertiary 1950	n		29	29	34	34	23	23	27	30
2. % in	r		1.000	.432*	−.180	.774*	.004	−.274	.344*	.504*
Tertiary 1960	n			29	49	49	33	33	35	46
3. Change in	r			1.000	−.358	.423*	.606*	−.679*	.646*	.600*
Tertiary Employment 1950–60	n				29	29	22	22	24	26
4. Population	r				1.000	−.107	−.260*	.241*	−.284*	−.215*
Size 1960	n					106	51	51	42	90
5. Per Capita	r					1.000	.106	−.361*	.571*	.656*
GDP	n						51	51	42	90
6. Income Inequality	r						1.000	−.799*	.572*	.476*
Gini Index	n							51	29	49
7. Proportion of	r							1.000	−.671*	−.519*
Income Accruing to Poorest 20%	n								29	49
8. Investment	r								1.000	.732
Dependence I	n									38
9. Investment	r									1.000
Dependence II	n									
Mean		.221	.235	.008	202.981	208.2	47.96	5.167	2.136	3.100
SD		.097	.100	.034	758.036	168.9	9.54	2.086	.741	1.454

* Significant to at least the .05 level.

APPENDIX C

THE COMMON CASES ANALYSIS

The results shown in Tables 1a–4b are based on the maximum possible number of cases. Since data on some variables are missing for some countries, this leads to differences in the cases included in the tables, depending on the variables involved. Inferences made on the basis of comparisons between or among tables can be questioned because differences or similarities may be the result of switching cases rather than due to "real" differences. We have therefore replicated these results on two sets of "common cases." One set consists of the 18 cases for which we have measures of Investment Dependence I, proportion of the labor force in the tertiary in 1950 and 1960, per capita GDP, and inequality. The other set of cases consists of the 21 countries with measures of Investment Dependence II, and the other 4

Table C1. Standardized Regression Coefficients from Tables 1a–3 and from "Common Cases" Analyses

Table Number	Dependent Variables	Independent Variables	Standardized Regression Coefficient (Beta) Common Cases n = 18	n = 21	Maximum Cases (Betas from Tables 1a–3)
1a	Gini Index	Investment Dep. I	.563**		.671**
		Per capita GDP	−.118		−.178
1b	Gini Index	Investment Dep. II		.986**	.786**
		Per capita GDP		−.772**	−.440**
2a	Tertiary 1960	Investment Dep. I	.290**		.261**
		Tertiary 1950	.636		.683
		Per capita GDP	.202*		.201**
		Time Lag	.057		.032
2b	Tertiary 1960	Investment Dep. II		.290**	.298
		Tertiary 1950		.649**	.664**
		Per capita GDP		.100	.077
		Time Lag		.098	.084
3	Gini Index	Tertiary Change	.702**	.689**	.644**
		Per Capita GDP	−.070	−.216	−.130

Notes: * = significant to between the .05 and .10 level.
** = significant to at least the .05 level.

Table C2. Correlation Matrix of Variables in Analysis Using Cases Common with Table 4a (n = 18)

	1	2	3	4	5	6	7	8
1. % in Tertiary 1950	1.000	.916*	.000	−.240	.771*	−.126	−.015	.418*
2. % in Tertiary 1960		1.000	.399*	−.424*	.846*	.160	−.254	.662*
3. Change in Tertiary Employment 1950–60			1.000	−.493*	.354	.677*	−.605*	.703*
4. Population Size 1960				1.000	−.395*	−.455*	.372	−.520*
5. Per Capita GDP					1.000	.178	−.404*	.527*
6. Income Inequality Gini Index						1.000	−.848*	.501*
7. Proportion of Income Accruing to Poorest 20%							1.000	−.558*
8. Investment Dependence I								1.000
Mean	.236	.259	.002	443.07	268.34	47.722	5.072	2.281
SD	.090	.083	.334	999.10	148.66	8.600	1.892	.672

* Significant to at least the .05 level.

variables. Table 4a is already based on the first of these sets of cases, and Table 4b is based on the second set. The results in Tables 1a–2b have been reestimated for the appropriate set of common cases, depending upon which of the two investment dependence measures was used. Table 3 has been reestimated for both sets of common cases. Table C1 summarizes these results by presenting the standardized regression coefficients (Betas) from the common cases analysis in comparison to the corresponding Betas from the "maximum cases" analysis (Tables 1a–3). Both measures of investment dependence have fairly strong, positive, and statistically significant effects on the Gini index of income inequality in the replications of Tables 1a and 1b. Both investment dependence measures continue to have moderately strong and statistically significant positive effects on growth of the tertiary, and growth of the tertiary continues to have significant positive effects on the Gini index in both sets of cases. In short, the results obtained earlier are very similar to the results obtained when attention is restricted to sets of common cases. Tables C2 and C3 present correlations among the variables in the analysis for the two sets of common cases.

Table C3. Correlation Matrix of Variables in Analysis Using Cases Common with Table 4b (n = 21)

	1	2	3	4	5	6	7	8
1. % in Tertiary 1950	1.000	.911*	.000	−.245	.795*	−.231	−.054	.550*
2. % in Tertiary 1960		1.000	.406*	−.392*	.877*	.043	−.341	.732*
3. Change in Tertiary Employment 1950–60			1.000	−.406*	.368	.509*	−.706*	.585*
4. Population Size 1960				1.000	−.373*	−.436*	.361	−.450*
5. Per Capita GDP					1.000	.037	−.149	.820*
6. Income Inequality Gini Index						1.000	−.753*	.353
7. Proportion of Income Accruing to Poorest 20%							1.000	−.424*
8. Investment Dependence II								1.000
Mean	.237	.262	−.003	393.32	283.30	47.143	5.148	3.634
SD	.087	.090	.037	930.93	163.74	8.089	1.826	1.534

* Significant to at least the .05 level.

REFERENCES

Adelman, Irma, and Cynthia Morris
1973 Economic Growth and Social Equity in Developing Countries. Stanford: Stanford University Press.

Amin, Samir
1976 Unequal Development: An Essay on the Social Formations of Peripheral Capitalism. New York: Monthly Review Press.

Baran, Paul
1968 The Political Economy of Growth. New York: Monthly Review Press.

Barnet, Richard, and Ronald Muller
1975 Global Reach: The Power of the Multinational Corporations. New York: Simon and Schuster.

Barraclough, S., and A. L. Domike
1966 "Agrarian structure in seven Latin American countries." Land Economics 42.

Bohrnstedt, George W.
1969 "Observations on the measurement of change." In E. G. Borgatta (ed.), Sociological Methodology 1969. San Francisco: Jossey-Bass.

Bollen, Kenneth, and Sally Ward
1979 "Ratio variables in aggregate data analysis: their uses, problems, and alternatives." Sociological Methods and Research 7:431–500.

Bornschier, Volker, Christopher Chase-Dunn, and Richard Rubinson
1978 "Cross-national evidence of the effects of foreign investment and aid on economic growth and inequality: a survey of findings and a reanalysis." American Journal of Sociology 84:651–83.

Cardoso, F. H., and Enzo Faletto
1979 Dependency and Development in Latin America. Berkeley: University of California Press.

Chase-Dunn, Christopher
1975 "The effects of international economic dependence on development and inequality." American Sociological Review 40:720–39.

Cline, W. R.
1970 Economic Consequences of Land Reform in Brazil. Amsterdam: North Holland Publishing.
1974 "Distribution and development: a survey of the literature." The Journal of Development Economics 1:359–400.

Cutright, Phillips
1967 "Inequality: a cross-national analysis." American Sociological Review 32:562–77.

Davis, Shelton
1978 Victims of the Miracle: Development and the Indians of Brazil. New York: Cambridge University Press.

Dos Santos, Teotonio
1970 "The structure of dependence." Papers and Proceedings, American Economic Review 60:231–6.

Duncan, Otis D.
1971 "Path analysis: sociological examples." In Blalock (ed.), Causal Models in the Social Sciences. Chicago: Aldine.

Evans, Peter
1979 Dependent Development: The Alliance of Multinational, State, and Local Capital in Brazil. Princeton: Princeton University Press.

Feder, Ernest
1971 The Rape of the Peasantry: Latin America's Landholding System. Garden City, N.Y.: Doubleday.

Fei, John C., and Gustov Ranis
1964 Development of the Labor-Surplus Economy. Homewood: Irwin.

Frank, Andre Gundar
1967 Capitalism and Underdevelopment in Latin America. New York: Monthly Review Press.

Galtung, Johan
1971 "A structural theory of imperialism." Journal of Peace Research 8:81–117.

Hagen, Everett E., and Oli Hawrylyshyn
1969 "Analysis of world income and growth, 1955–1965." Economic Development and Cultural Change 18:1–96.

Heise, David R.
1970 "Causal inference from panel data." In E. G. Borgatta and G. W. Bohrnstedt (eds.), Sociological Methodology 1970. San Francisco: Jossey-Bass.

Hout, Michael
1979 "Persistent dependence: the case of Latin America from colonial times to the present." Paper presented at the annual meeting of the American Sociological Association, Boston.

International Monetary Fund
Balance of Payments Yearbook. Washington, D.C.: IMF.

Jackman, Robert W.
1975 Politics and Social Equity: A Comparative Analysis. New York: John Wiley and Sons.

Jain, Shail
1975 Size Distribution of Income: A Compilation of Data. Washington, D.C.: The World Bank.

Jakobson, Leo, and Ved Prakash
1971 "Urbanization and urban development: proposals for an integrated policy base." In Jakobson and Prakash (eds.), Urbanization and National Development. Beverly Hills: Sage.

Ledogar, Robert J.
1975 Hungry for Profits: U.S. Food and Drug Multinationals in Latin America. New York: IDOC/North America, Inc.

Lord, Frederic M.
1956 "The measurement of growth." Educational and Psychological Measurement 16:421–37.
1958 "Further problems in the measurement of change." Educational and Psychological Measurement 18:437–51.

Moir, Hazel
1975 "Relationships between urbanization levels and labor force structure: a cross-national analysis." Ph.D. dissertation, Department of Sociology, Brown University.

1976 "Relationships between urbanization levels and the industrial structure of the labor force." Economic Development and Cultural Change 25:123–35.

Organisation for Economic Co-operation and Development. Development Assistance Directorate.
1972 Stock of Private Direct Investment by D.A.C. Countries in Developing Countries, End 1967. Paris.

Paukert, Felix
1973 "Income distribution at different levels of development: a survey of evidence." International Labor Review 108:97–125.

Peattie, Lisa Redfield
" 'Tertiarization' and urban poverty in Latin America." In Wayne Cornelius and Felicity Trueblood (eds.), Latin American Urban Research 6:109–23.

Rubinson, Richard
1976 "The world-economy and the distribution of income within states: a cross-national study." American Sociological Review 41:638–59.

Shaw, R. Paul
1976 Land Tenure and the Rural Exodus in Chile, Colombia, Costa Rica and Peru. Gainesville: University of Florida Press.

Soares, Glaucio
1969 "The new industrialization and the Brazilian political system." In James Petras and Maurice Zeitlin (eds.), Latin America: Reform or Revolution. New York: Fawcett.

Stewart, Frances
1977 Technology and Underdevelopment. New York: Macmillan.

Taylor, Lance, and Edmar Bacha
1976 "The unequalizing spiral: a first growth model for Belindia." Quarterly Journal of Economics XC:197–218.

Timberlake, Michaél
1979 "Economic dependence, internal urban and labor force structure, and problems of development: a quantitative, cross-national study." Ph.D. dissertation, Department of Sociology, Brown University.

Todaro, Michael
1969 "A model of labor migration and urban development in less developed countries." American Economic Review LIX:138–48.

Warren, Bill
1973 "Imperialism and capitalist industrialization." New Left Review 81:3–46.

ERRATA

■ Three errors unfortunately occurred in "Synthesis and Comparison of Stratification Theories: A Reply" (to Attewell and Fitzgerald's Comment on an earlier article) by Robert V. Robinson and Jonathan Kelley in the April, 1980, *ASR*.

On page 328, column two, last paragraph, a type-correction line for the previous paragraph was, incorrectly, inserted as the second line of the first sentence. The second line should have read "zation of control, we defined the capitalist". The entire correct sentence—Employing this definition and operationalization of control, we defined the capitalist class as those who control the means of production and exercise authority (that is, have employees or subordinates) and the petite bourgeoisie as those who control the means of production but do not exercise authority.—was critical to Robinson and Kelley's reply, toward explaining their position and toward understanding the remainder of the article.

On page 329, Table 1, second column, the "Control (1)" figure—.184—for "Dahrendorf: Authority" should have read: .184*—the asterisk indicating that it, too, was significant at the .05 level.

On page 332, Table 3, the third-column figure—.115—for "Marx: Control (cols. 1 and 2) or Ownership (cols. 3 and 4)" should have read .115*—the asterisk indicating that it is significant at $p < .05$. Because of the omission of the asterisk, Attewell and Fitzgerald's claim (that [Robinson and Kelley] slighted the Marxian model by employing a job earnings measure) seems to be more substantiated than it, in fact, is.

BANKS AND CORPORATE LENDING: AN ANALYSIS OF THE IMPACT OF THE INTERNAL STRUCTURE OF THE CAPITALIST CLASS ON THE LENDING BEHAVIOR OF BANKS*

RICHARD E. RATCLIFF
Syracuse University

American Sociological Review 1980, Vol. 45 (August):553–570

This paper analyzes the lending patterns of commercial banks in a major metropolitan area. The banks are studied in terms of their ties, through the members of their boards of directors, to networks of economic power and upper class social interaction within the metropolitan structure of the capitalist class. The central research question concerns whether the extent of a bank's centrality to these networks substantially influences the decisions made by each bank regarding the allocation of the capital it commands. While attempting to consider the broader social consequences of private economic power, the paper addresses three problems found in many recent studies of interlocking directorates: the general lack of attention paid to questions of social consequences as opposed to issues of structure; conceptual confusions central to explanations that focus on notions of "cooptation"; and the omission of measures on the dimension of economic power. The empirical analysis, dealing with the impact of class structure, economic power and institutional size measures upon the distribution of bank loan capital, reveals that the main determinants of lending are the class structure characteristics of banks as these are reflected in the network linkages of their boards of directors. In particular, banks whose directors are tied to numerous local corporations which represent major concentrations of economic power and, to a lesser extent, to the most exclusive upper class social circles are those most likely to emphasize concentrated lending to capitalist borrowers and, correspondingly, most likely to withhold capital from mortgage loans. It is also shown that these differences in lending behavior are not related to the profitability of the banks themselves. It is argued that these patterns are best explained by a model of capitalist class coordination in which dominant groups exercise their abilities to influence banks and corporations in ways that benefit their interests. These findings are examined in terms of the general economic stagnation and even decline of older urban areas.

In recent years we have witnessed an impressive proliferation of studies of networks of interlocking corporate directorates (Allen, 1974; 1976; Levine, 1972; 1977; Sonquist and Koenig, 1975; Bearden et al., 1975; Bunting and Liu, 1977; Warner and Unwalla, 1967; Mariolis, 1975; 1977; Mintz and Schwartz, 1977; Smith and Desfosses, 1972; Pennings, 1978; Bonacich and Domhoff, 1977; Stanworth and Giddens, 1975). While the present study also examines such networks, it differs substantially from most of the previous work. It focuses not just on the structure of private economic power, but, more importantly, on the consequences of the exercise of that power. Thus, in looking at the flow of capital in a metropolitan area, this study is most concerned with joining the question of "Who holds positions of power?" with the question of "So what?" In particular, the analysis examines the ties that commercial banks in St. Louis, Missouri have to networks of economic power and upper class social interaction within the metropolitan structure of the capitalist class and, then, considers whether a bank's centrality to these networks substantially influences the lending decisions made by each bank regarding the allocation of the capital it commands.

Critical Issues in the Study of Interlocking Directorates

This paper addresses three major problems which exist in many recent studies of interlocking directorates and related social network. These problems are: first, the general lack of attention to

---

* Direct all communication to: Richard E. Ratcliff; Department of Sociology; Syracuse University; Syracuse, NY 13210.

I would like to thank Kathryn Strother Ratcliff, Kay Oehler, Mark Gallops, Mary Peters, Mary Elizabeth Gallagher and John Zipp for comments and assistance in completing this paper. This research has been supported by the National Science Foundation under Grant No. SOC77-18036.

questions of effects, outputs, and significant social consequences associated with differing structures of organizational interlocks and social networks; second, a vagueness of the dominant theoretical orientations concerning the issue of what motivates the establishment of interconnections among corporations and how these interconnections relate to larger structures of power; and third, the typical omission of measurable indices of economic and social power and importance other than those defined by the dimensions of "interconnectedness" used to identify network patterns. Each of these problems merits some discussion.

a. Interlocking directorates and social consequences. Most recent studies of interlocking corporate directorates have been limited to considering the structural patterns represented by the interlocks. There have been inquiries regarding whether corporations are formed in interest group clusters (Allen, 1978; Sonquist and Koenig, 1975); whether the positional centrality of banks indicates patterns of bank dominance in interlock relationships (Mariolis, 1975); and whether the interlock patterns suggest efforts by firms to establish supportive and cooptive relationships with other firms of importance to their operations (Allen, 1974; Burt, 1978a; 1978b). While these studies raise important issues, what is largely missing is any attempt to demonstrate whether interlock patterns, and more specifically the structures of private economic power embodied in such patterns, lead to consequences of substantial importance for the larger society.

This lack of any attention to questions of the consequences of private economic power parallels a similar trend in the extended debates concerning community power (Aiken and Mott, 1970). These debates, which originally stimulated much of the research on interlocking directorates discussed here, not only have tended to become overly focused on questions of structure, but also have increasingly dealt less with private economic power. Growing out of the unresolved disputes over "structures," there have been some attempts in the community power literature to look at outputs. Most of these attempts, however, have been focused on the public realm, such as considering how organizational factors influence a range of outputs including the level of public spending for various programs, the success of different communities in obtaining federal grant-in-aid funds, the policy decisions regarding school integration, and the imposition of water fluoridation (Clark, 1975). There have been few studies concerning outputs associated with private decisions, such as those made by major corporations and banks and their owners and managers. Most of those that do exist have been largely centered around case studies that emphasize qualitative rather than quantitative analyses (Hayes, 1972; Hartman, 1974).

This lack of attention to consequences is especially striking because studies of interlocking corporate directorships, as well as those of community power structures, were originally motivated by theoretical and substantive questions that certainly were grounded in concerns over the social implications of private economic power. For example, Louis Brandeis, writing in 1913 in reaction to an early legislative study of corporate concentration, saw corporate interlocks as "the root of many evils," including the suppression of competition and the centralization of economic power that represented a major threat to a democratic society (Brandeis, 1913:13). C. Wright Mills saw interlocking directorates as representing a level of coordination which is tied less to "any specific property interest" than it is to "class-wide property" and that embodies "the community of interest," and "the unification of output on policy, that prevails among the propertied class" (Mills, 1956:122–3). A more recent government report suggested that interlocking directorates could

> . . . lead to a concentration of economic and fiscal control in a few hands. There is in this the danger of a business elite, an ingrown group, impervious to outside forces, intolerant of dissent, and protective of the status quo, charting the direction of production and investment. . . . (U.S. Senate Committee on Governmental Affairs, 1978:6)

Even more generally, the concern with interlocking directorates has been linked with questions of whether large corporations, given the increasing concentration among corporations in recent decades, have emerged as the dominant centers of power within the society (Useem, 1978; 1979).

While the declarations by Mills and others concerning the social importance of consequences that might flow from corporate interlocks have not stimulated much recent research on the question, among those studying such interlocks there have been some limited exceptions. One area of corporate behavior where there has been attention to outputs concerns corporate profit rates (Gogel, 1977; Burt, 1978a; Norich, 1980). The study of profit rates as an output in studies of corporate structure does have some justification. Profit rates are relevant to an understanding of the ability and success of firms in seeking to maximize their own interests and to achieve greater firm efficiency. In addition, critics of the "managerial thesis" have identified profit rates as a reasonable standard for evaluating the supposed distinctions between corporations of different ownership types (Norich, 1980). More generally, profit rates also provide a standard for examining the ability of firms and groups of firms to accumulate concentrations of capital.

Despite the relevance of profit rates for some questions, corporate profits are not clearly related to the theoretical and substantive problems concerning the societal effects of private economic power that originally motivated most studies in this area. Researchers studying profit rates as outputs have generally not shown what importance the differences in profit rates of firms have for understanding questions that reach beyond the issue of corporate interconnections. While profit rates do have the advantage of being measurable, using available data, this advantage does not override their fundamental limitations.

This study does focus on consequences. The analysis presented here examines the lending behavior of banks, an area of economic behavior that clearly does have broad social implications especially in studies of contemporary urban problems. Banks have a unique character as private enterprises: they have the function of accumulating surplus capital from depositors throughout their communities and then making investment decisions on how this accumulated capital is to be allocated in the form of loans. Patterns of loan decisions can be assumed to represent policy priorities established by the officers of the bank and by its board of directors (Reed et al., 1976:149). In their loan decisions, banks are, in effect, deciding on how a substantial portion of the surplus capital, accumulated from the entire community and owned by a diverse range of depositors in the community, is to be distributed. While a broad range of demands exists for that capital from prospective borrowers, banks are able to determine which demands are met. As a result, they routinely influence "the allocation of real resources" in the society and, in effect, "what is produced, how it is produced, and to whom it is distributed" (Harvey, 1975:125). This quasi-public character of banks, despite their status as private, profit-seeking enterprises, has been long recognized in this country. Brandeis, writing over a half century ago, stated that because "bank resources are, in the main, not owned by the stockholders nor by the directors," and because the "dependence of commerce and industry upon bank deposits as the common reservoirs of quick capital is so complete . . . deposit banking should be recognized as one of the businesses 'affected with a public interest' " (Brandeis, 1913:15). Similarly, a modern textbook on banking states, "The major reason banks are chartered is to serve the credit needs of their communities. If this cannot be done, there is little justification for their existence" (Reed et al., 1976:150). Government regulation of banking has also reflected the understood obligation of banks to meet community credit needs (U.S. Senate Committee on Banking, 1977).

Bank lending decisions are of particular importance in areas such as St. Louis, which not only face a diverse range of internally based demands for capital, but which also are called upon to provide capital for investments in more rapidly growing sections of the country (Ratcliff, 1980a; 1980b). The resulting loan distributions of banks are thus related to issues of disinvestment within the metropolitan area, and especially to such critical questions as the availability of capital for investments in local housing, and to the processes of stagnation, disinvestment, and decline in older metropolitan areas (Bradford and Morino, 1977).

b. Ambiguities in theories of interorganizational relationships. Recent treatments of interlocking directorates have not clearly specified what motivates the establishment of interlocks. The most general theoretical approach offered to explain interlocking corporate directorships has centered around the notion of "cooptation." For example, Allen has suggested a "theory of interorganizational elite cooptation" which sees interlocking directorates as representing "an attempt by corporations to anticipate environmental contingencies and to control their relationships with other corporations" (Allen, 1974:393). He notes further that such interlocks constitute "a cooperative strategy anticipating or controlling potentially disruptive unilateral actions by other organizations" (Allen, 1974:403). Within such a framework, interlocks between banks and corporations are seen as quite rational cooptive arrangements. Thompson suggests that corporations seek bank interlocks in order to increase "their access to financial resources for the duration of the cooptive arrangement" (Thompson, 1967:35).

While this perspective is generally plausible, it suffers from being both vague and potentially tautological. The statement of a cooptive motivation becomes of little use in evaluating theories of power and control over corporations if it simply means that firms, like people, tend to act in their own self-interest in ways that allow them to experience gains or control losses. In the context of proposing a solution, one proponent of the cooptation approach acknowledges this problem when he states that discussions of "interlocking directorates as cooptive devices do not advance theoretical understanding of corporate interlocking as much as provide a sociological label for it" (Burt, 1978a:2; 1978b).

An additional unresolved problem in this cooptation theoretical approach is that the model assumes a basically atomized organizational set in which individual organizations are analyzed in terms of whether they have chosen either to be or not to be linked with other organizations. The factors motivating such choices are assumed to be tied to the problems and prospects experienced by individual corporations. Thompson, for example, argues that cooptation as a strategy is most likely to be pursued by organizations in a weakened position seeking to "avert threats to their stability or existence" (Thompson, 1967:35). Organizations are thus seen as individualized decision-making units that are drawn into relationships of dependency, thus losing their autonomy in order to reduce uncertainty. Such an individualized approach does not appear to do justice to the problem. In particular, the "cooptation theory" perspective fails to account for any structures of power and control that are not firmly based in any one organization. This limitation is unfortunate since one of the motivating concerns in the whole area of social networks has been that the relevant units of organizational control and coordination might be larger structural configurations.

The present paper builds upon a model of capitalist class coordination. The concern here is with the extent to which interconnected and coordinated groups within the capitalist class are able to exert power and control over sets of firms in a way that is advantageous to the dominant groups involved in them. In such a system, it would not be necessary to assume that a dominant group is synonymous with any one organization in the network. Instead, power and control could be based in a group that simultaneously has interest in a number of organizations. Interlocking directorates can be seen as important manifestations of power relationships of the dominant groups.

This class coordination perspective draws upon analyses of "finance capitalists" and "interest groups" in that both corporations and banks are seen as being tied to more inclusive networks of power within the capitalist class (Sweezy, 1942:chap. 14; 1953). Rudolf Hilferding, whose 1910 book focused attention on finance capitalists, saw interlocking directorates as a crucial aspect of these networks, often indicating other less visible dimensions of interconnections such as ownership. Hilferding wrote about the "personal union" and "community of interests" embodied in ". . . a circle of persons who, thanks to their own possession of capital or as representatives of concentrated power over other people's capital (bank directors), sit upon the governing boards of a large number of corporations" (Hilferding [1910] cited in Sweezy, 1942:261). Imbedded in Hilferding's analysis were two conflicting views of the role of banks in their relationships with corporations. On the one hand, banks were seen as the organizers of the capitalist class, imposing order, discipline and some centralized planning on a potentially overcompetitive economic system in order to achieve some overall level of improved internal efficiency. On the other hand, banks were seen as centers of speculative and even predatory activity leading to corporations being raided and "run into the ground" by banks seeking their own profits. Even without resolving these views, Hilferding saw banks and finance capitalists as becoming increasingly dominant over corporations.

The research reported here is not based on any assumption that banks are the dominant forces in their relationships with corporations. While the special character of banks as holders and distributors of capital makes them likely centers of class organization and coordination, both banks and corporations are seen here as possibly interconnected points of decision making within larger networks of class interests. Rather than expecting any one bank or corporation to act as a centralized point of dominance, it is more useful to see an ongoing effort by groups within the capitalist class with lines to both banks and corporations attempting to maximize benefits in different forms at a number of separate points. A similar view has been suggested by Zeitlin:

> Neither "financiers" extracting interest at the expense of industrial profits nor "bankers" controlling corporations, but finance capitalists on the boards of the largest banks *and* corporations preside over banks' investments as creditors *and* shareholders organizing production, sales, and financing, and appropriating the profits of their integrated activities. (Zeitlin, 1976:900, emphasis in the original)

Thus, while the cooptation model sees corporations and banks as distinct islands of self-interest searching for strategic alliances, the class coordination model sees both banks and corporations as vehicles for the interests of groups able to dominate, or at least heavily influence, the actions of individual firms.

Despite the conceptual clarity in the contrast between the cooptation and the class coordination perspectives, it is difficult to frame an unambiguous test to determine which is more appropriate. One partial test is allowed by the focus on bank lending decisions. Patterns of lending decisions can be examined simultaneously in terms of the effects of structural characteristics of the banks and, most notably, of their size, and in terms of the effects of corporate and upper class organizational interlocks. In important ways, the cooptation perspective would suggest that bank behavior would follow individual structural characteristics, while the class coordination perspective would emphasize the influences of network linkages.

While this comparison is important, this study also presents an opportunity for a more fundamental test of the usefulness of the two contrasting perspectives. This test involves an analysis of bank profit rates. Even though the use of profit rates was criticized earlier as an output measure of the social impact of private economic power, profit rates are directly relevant to the issue of what forces motivate organizational behavior. The key issue here is whether banks appear to be acting as independent units, seeking to maximize the benefits they receive individually, or whether they are being motivated by some broader set of interests.

Given what some people have seen as the dominant role banks exercise over corporations in lending relationships (as is reflected in the power of banks to deny or approve loans), a perspective that emphasizes basic organizational autonomy would presumably also make certain assumptions about the distribution of bank profits. If, as the cooptation model assumes, firms seek interlocks in order to stabilize and improve their positions, then banks that are successful in establishing corporate interlocks should enjoy the highest rate of profit. Such a linkage between the achievement of strategic interlocks and the attainment of higher profits has been specifically predicted in one elaboration of the cooptation model (Burt, 1980). From this same perspective it is reasonable to expect that banks most involved in lending to corporate and other "blue-chip" borrowers would also experience higher rates of profits (Starr, 1975).

In contrast, according to the class coordination perspective, it is not necessary that banks with more interlocks will be more profitable. If larger class networks control both banks and corporations, it is not possible to say whether economic gains among interlocked banks and corporations would be realized as profits by either individual banks or corporations or in some other form by other firms in the same network. It is quite possible, since the interests of corporations are most served by ready access to low cost capital, that lending to corporations will not necessarily yield high profits for banks. Banks could be much more important to groups that control them as accumulators of available capital than as centers of profit. Thus, whereas the cooptation perspective would presumably predict a direct relationship between corporate lending and bank profitability, the class coordination thesis makes no such prediction.

c. Economic power and network interconnections. Studies of interlocking directorates, with their focus on relationships among organizations, have represented a critical perspective on more traditional organizational studies that have only examined more individualized or static dimensions of power. However, actual research accomplishments have lagged behind the image of an approach that seeks to integrate complex interrelationships of power. With the emergence of more sophisticated mathematical, computer-based and statistical techniques in studies of organizational interlocks, research has become increasingly limited in its ability to incorporate multiple dimensions of power. Thus, most studies of interlocks are concerned with only one dimension of interorganizational importance—that of the interconnections themselves (cf. Levine, 1972; Sonquist and Koenig, 1975). Regardless of their differing individual characteristics, all firms typically are treated as equal units and then analyzed in terms of whether they are more or less central, based on their patterns of interconnections.

The omission of dimensions directly relating to economic power is unfortunate because vast differences in size and in power are so characteristic of the corporate realm. In the present study, the goal is to characterize the interconnections between banks and corporations in terms of not only their intensity, but also the corporate power and potential capital demands that they represent. The former dimension is measured by the number of large corporations tied to a bank. In order to incorporate the dimension of corporate power, I have assumed that corporations can be compared according to their total number of employees.

There are several advantages in using the number of employees as a measure of corporate power, including the availability of data and the reasonable comparability of this measure across sectors of the economy. Ideally one would compare firms on their total assets. However, given the focus in this study on the entire population of corporations of some substantial size in one metropolitan area (many of which are not only not listed in the major stock exchanges but also do not even have their stock publicly traded), it became clear that it was not practical to attempt to gather asset information. Data on sales are available for most corporations, and could roughly be estimated for the rest, but sales data produce even more problems than number of employees in making comparisons across different sectors of the economy. For example, retail firms, public utilities, transportation firms, and nonbanking financial corporations have very different relationships of sales to actual firm sizes than do manufacturing firms. Further, manufacturing firms also differ greatly among themselves. The number of employees (information which generally is available) does give a reasonable approximation of the economic and social importance of each firm.

Having discussed the strategies employed in this paper in relation to the three problems existing with most studies of interlocking directorates, I shall now move to present the results of my own research. First, however, I shall present a more integrated statement of the theoretical approach I am pursuing.

Banks and the Command of Corporate Capital

The general thesis guiding this research follows the capitalist class coordination model. It holds that banks tend to operate not only as profit-seeking enterprises in their own right, but also as the capital-accumulating appendages of the groups and interests within the class to which they are most closely connected. It is argued that this dual character of banks becomes most relevant for those banks which are closely tied to dominant groups at the center of the class where there exists the concentration of both demands and opportunities for capital investments. Banks can be expected to loan the funds they command in ways that reflect their class structure positions (Ratcliff, 1980a; 1980b). The internal structure of the class can be divided along two dimensions of centrality: economic power and upper class social interaction. The dimension of economic power can be represented in terms of ties to the corporate structure while upper class social interaction is represented by ties to exclusive private upper class organizations (Ratcliff et al., 1979). While the association of the first dimension with greater capital demands is apparent, research has also shown how upper class social ties are important in integrating the sometimes divergent economic interests of leading capitalists (Domhoff, 1970; Baltzell, 1958). Banks more closely tied to centers of either economic power or upper class social interaction, and especially those tied to both, should concentrate their lending in the direction of capitalist borrowers similarly located in the class structure, with corporate loans in particular predominating. In contrast, banks more peripheral to the dominant groups within the class can be expected to be subject to a more diverse range of demands for capital, as well as a set of opportunities less concentrated in the corporate sphere, with the consequence that their loans will be less exclusively directed toward major capitalist borrowers. One result is that the more marginal banks should be substantially more involved in mortgage lending.

This thesis has found support in research recently completed (as part of this same project) on mortgage disinvestment by banks in the St. Louis area (Ratcliff, 1980a; 1980b). Looking at home mortgage loans made by banks during 1975 and 1976, considerable variation was found among banks, both in the extent of their involvement, relative to their sizes, in providing home mortgage loans, and in the extent to which they provided home loans in older low and moderate income neighborhoods within the metropolitan area. Evidence of disinvestment and withdrawal of capital from home mortgage lending in such neighborhoods was shown to be characteristic of those banks most closely tied to the centers of economic power and upper class social integration within the capitalist class.

The Research Data

This study focuses on all 78 commercial banks which in 1975 had their headquarters either in St. Louis City or St. Louis County, the two dominant counties in the St. Louis SMSA, and on the 890 directors of these banks. Missing information caused the deletion of one bank from the present analysis. "Loan portfolio" data for the last day of 1975 were obtained on each of the banks from the Federal Reserve System. These data come from the "Reports of Condition" that each commercial bank is required to file periodically with federal regulatory agencies. In these reports, banks must specify the total dollar amount of outstanding loans they have in each of over 20 categories.

In this analysis, two distinct types of loans are treated as "outputs." The first type consists of loans in categories where the borrowers appear to be either corporations, partnerships, or individual capitalists obtaining loans in order to make or maintain investments in profit-seeking activities outside of real estate. These "loans to capitalists" represent a combination of several different categories of loans: commercial and industrial loans; loans to U.S. commercial banks and other financial institutions; and loans given either to brokers or nonbrokers for the purchase of securities. The second type includes all conventional and government guaranteed mortgage loans on one to four unit residential properties (predominantly single family homes). Specific measures used are the percentages that each loan type is of all loans outstanding from each bank.

These data do have certain limitations. Most notably, it is not possible to identify the recipients, the sizes, or the geographic destination of individual loans. Thus the recipients of commercial and industrial loans could be either major or small corporations, and the loans could be used either locally or elsewhere. The lack of location information for the home mortgage data is less of a problem because a separate study has been made of that issue using geographically specific data made available under the Home Mortgage Disclosure Act (HMDA) (Ratcliff, 1980a; 1980b).[1]

In order to determine the economic "networks" within which each bank was situated, all interlocks between bank directors and the largest corporations in the St. Louis area were identified. Determining the list of St. Louis corporations and their directors was a complex task. The major published directories of corporations, such as the Dun and Bradstreet *Million Dollar Directory*, proved to be very incomplete on the metropolitan level. A number of other directories were consulted in order to identify the population of St. Louis corporations of comparable size to those in the *Million Dollar Directory* based on sales and/or number of employees. Through these processes, 350 St. Louis based corporations were identified which were still in business in 1976, and which were not subsidiaries of other corporations on the same list (some firms left in the study were subsidiaries of other, usually nonlocal, corporations that, nonetheless, maintained local boards of directors). Information on file with the Secretary of State of Missouri providing the names of the board of directors of each Missouri corporation was consulted in cases where the data were not provided in directories. The search yielded over 3,000 directorship positions. For each of the 350 St. Louis corporations, the total number of employees was ascertained.

With these data two measures of economic interconnections were calculated for each bank. The first measure is the total number of firms among the 350 to which the bank is directly linked through a shared director. The second measure is the total combined number of employees for all corporations with which each bank is linked. Neither of these measures is affected if a bank has more than one director link to any one corporation since the interlock and the total number of employees are each only included once.

The bank directors were also examined to determine which had been involved in any of five organizations found to be the most exclusive in upper class social circles in St. Louis (Ratcliff et al., 1979). These organizations consist of two men's clubs, one country club, the *St. Louis Social Register*, and the Veiled Prophet Society, a secret organization of St. Louis men which holds a highly exclusive debutante ball each year. Membership lists or directories were obtained for the first four organizations. For the Veiled Prophet Society, a search through newspaper archives was conducted in order to determine all bankers who had either had a daughter as a debutante or a wife as a "Lady of

---

[1] For mortgage analyses, the HMDA data have the advantage (in addition to geographic specificity) of reporting new loans for a specific year, thus being sensitive to recent policy priorities of a bank. Loan portfolio data, on the other hand, have the advantage of indicating the overall commitment of a bank to mortgage lending at a particular point in time. Even though mortgage loans are given for a long term, often up to twenty-five or thirty years, they also can be bought or sold on the secondary mortgage market. Therefore, a current year's loan decision might not represent the accumulated commitment of such loans for a particular bank.

Honor" in the Ball during any of the previous twenty years. Involvement in these organizations was found to be highly intercorrelated and distinct from other clubs and social organizations for which membership information was collected. The particular measure of the closeness of each bank to networks of "upper class social prominence" used here is the proportion of bank directors in each bank who are tied to at least one of the five exclusive social organizations. The great advantage of these data on exclusive social organizations is that they make possible the incorporation of the dimension of upper class networks in a manner that is conceptually and empirically distinct from the data on corporate or economic networks.

The size of the banks is measured by the total deposits of each on the last day of 1975. The holding company status of each bank was also determined. Missouri prohibits branch banking but allows multibank holding companies to operate. Banks owned by holding companies maintain separate boards of directors and, according to laws and regulations, are supposed to operate in a generally independent fashion. In order to examine the impact of holding company status, holding company affiliated banks are distinguished from independent banks in the analysis.

Because of the highly skewed distributions of the measures of bank deposit size and of total employees to which the bank is linked, logarithmic (base 10) transformations were performed in order to produce more normal distributions useful in regression analyses (cf. Schuessler, 1971:408).

Yet another set of data incorporated in this analysis concerns the profitability of banks. These data come from income reports submitted yearly to federal regulatory agencies. The present analysis utilizes a conventional measure of bank profitability: net income of the bank before taxes and before security gains and losses divided by the total assets of the bank (Reed et al., 1976:434). These data have an advantage over nonbanking corporate profit data in that there is much less variation among banks in accounting techniques due to the common rules and regulations imposed by the federal bank regulators.

The Research Findings

The focus of the analysis is on the determinants of lending patterns among St. Louis banks Initially, differences in loan distributions for banks in different size categories will be examined. Next, a series of regression equations will be presented in order to identify and compare differences in the amount of variance in lending patterns explained by particular variables and sets of variables. Finally, the distribution of loans and other bank characteristics are examined in relation to bank profitability.

a. The distribution of bank loans. The distribution of bank loan capital among lending categories is presented in Table 1 by the size ranks of the banks. For all banks taken together, all three categories of loans to capitalists account for just under one-half (45.8%) of the outstanding loans. The largest single category, commercial and industrial loans, includes over one-third (36.8%) of the loan portfolios. Far smaller amounts are represented by loans to banks and financial institutions (5.8%) and loans for the purchase of securities (3.2%). About one-sixth (15.8%) of the loan totals are invested in home mortgages. Loans not accounted for here are in such loan categories as multifamily residential loans, and credit card and auto loans.

Banks differ in their loan distributions according to their size. The top five banks have concentrated their loans more in lending to capitalists and much less in providing loans for home mortgages. The three categories of smaller banks, which do not differ significantly from each other in their loan distributions, have fewer loans to capitalists and more loans for mortgages. Taken just by themselves, these findings provide some support for the argument that differences in bank lending practices are a function of the size of the bank. Economists interested in banks typically see size as a major determinant of bank performance. In particular, larger banks are seen as trying to emphasize wholesale banking operations with businesses rather than engaging in retail or consumer-oriented banking. It is argued that wholesale banking is both more efficient and more profitable (Starr, 1975:86; Leinsdorf and Etra, 1973:81). However, in Table 1, in contrast to those that follow, data are not presented in a way that allows us to consider the effects of bank size in relation to the effects of the positions of banks within the structure of the capitalist class.

Table 1. The Distribution of Total Bank Loans as of 12/31/75 among Lending Categories by Banks Grouped According to Size

Size Rank of Bank	Loans to Capitalists: % of Loans for Commercial and Industrial Loans	Loans to Capitalists: % of Loan to Banks and Financial Institutions	Loans to Capitalists: % of Loans for the Purchase of Securities	Combined % of Loans to Capitalists	% of Loans for Home Mortgage Loans	Total Amount for All Types of Loans*	(N)
Top 5 Banks	46.9	8.5	5.1	60.5	7.2	$2,136,165	(5)
Banks Ranked 6 to 15	26.6	3.1	.5	30.2	27.3	$522,323	(10)
Banks Ranked 16 to 40	23.1	2.6	1.0	26.7	27.0	$747,099	(25)
Banks Ranked 41 to 77	23.8	1.8	1.3	26.9	24.3	$467,052	(37)
	36.8	5.8	3.2	45.8	15.8	$3,872,539	(77)

* In thousands of dollars.

b. Zero-order correlations. In Table 2, the zero-order correlations reveal a generally consistent pattern of support for the arguments developed above. The intensity of a bank's lending to capitalist borrowers is strongly related to the different measures indicating the closeness of the bank's ties to networks of economic power and upper class social interaction within the local capitalist class. The relevant correlations, which include those between loans to capitalists and the range of corporation ties (r = .573), the combined size of linked corporations measured by total employees (r = .547), and upper class social ties (r = .540), are all strong. However, also as expected, a strong positive relationship exists between capitalist-oriented lending and bank size in deposits (r = .402). The pattern of relationships for home mortgage lending is, again as expected, the reverse of that for capitalist lending. These relationships, which are weaker than those for loans to capitalists, indicate that the more closely tied a bank is to the centers of power and prominence, the less involved it is in mortgage lending. Here, however, bank size is only weakly related (r = −.070). Holding company banks are more likely to be involved in loans to capitalists (r = .179) and less likely to emphasize home mortgage loans (r = −.255).

c. Internal class structure and loans for capitalists. In Table 3, the selected regression models, with different combinations of the study variables, allow us to examine statistically the relative contributions of the factors as determinants of the percentage of total loans going to capitalist borrowers. By comparing the increments of explained variation (R^2) added by particular factors when the ef-

Table 2. Zero-Order Correlations between Bank Lending to Capitalist Borrowers and to Home Borrowers, the Capitalist Class Connections of Bank Board Members and the Size and Holding Company Status of the Bank

	2	3	4	5	6	7
1. Combined Loans to Capitalist Borrowers	−5.30	.573	.547	.540	.402	.179
2. Loans for Home Mortgages		−.342	−.476	−.289	−.070	−.255
3. Number of Top 350 Corporations with which Bank Is Interlocked			.589	.724	.687	.182
4. Total Number of Employees in Top 350 Local Corporations with which Bank Is Interlocked				.585	.464	.247
5. Proportion of Bank's Directors Who Belong to Top Upper Class Organizations					.575	.184
6. Total Deposits of Banks						.198
7. Holding Company Status of the Bank						

(N = 77)

1. Percentage of total loans outstanding on 12/31/75 to commercial and industrial borrowers (except for real estate), to banks and other financial institutions and to brokers and nonbrokers for the purchase of securities.
2. Percentage of total loans secured by one to four family residences.
3. Number of different Top 350 St. Louis-based corporations with which bank is interlocked through one or more of its directors.
4. Total combined number of employees in all Top 350 local corporations with which bank is interlocked (logarithmic transformation, base 10).
5. The proportion of the bank's directors who belong to one or more of the five leading upper class organizations in the St. Louis area.
6. The total deposits of the bank as of 12/31/75 (logarithmic transformation, base 10).
7. The holding company status of the bank in 1975: banks affiliated with holding companies are coded 1; independent banks are coded 0.

fects of others are controlled, we have a standard for evaluating the relative strength of different influences.

The most notable discoveries in Table 3 are, first, that the explanatory model is quite strong ($R^2 = .436$) and, second, that the measures for capitalist class structure emerge as the major determinants of the relationship. When the explanatory power of the three measures of capitalist class structure (in Equation C, $R^2 = .434$) is compared to one with the two measures of bank size and holding company status (in Equation D, $R^2 = .172$), the strength of the class structure measures is apparent. This comparison is examined here in terms of how each equation relates to the full five-variable equation (Equation A). While the class structure measures produce an increment in explanatory power of 25% over just the institutional characteristics alone (Equation A − Equation D = .254), essentially no increment is associated with bank size and holding company status in addition to that explained by the class structure measures alone (Equation A − Equation C = .002).

Almost all of the explanatory power associated with capitalist class structure comes from two measures of corporation interlocks rather than from the measure of upper class social ties. The explanatory increment for the full five-variable model associated with upper class social ties (.010) is considerably smaller than that for the number of corporate interlocks (.029) and the total number of employees (.070). However, the weakest increments are those associated with bank size and holding company status (both are .001). Based on those increment differences and on the standardized regression coefficients, the measure of the combined total number of employees in interlocked corporations is the strongest determinant relative to other variables in the model.

d. Home mortgage loans. In general, home mortgage lending follows a parallel but opposite pattern from that of loans to capitalists. As shown in Table 4, the overall model is generally quite strong ($R^2 = .328$) in predicting the percentage of a bank's total loans going to home mortgages. While once again a much larger

Table 3. Comparisons of the Explanatory Power of Capitalist Class Connections, Deposit Size and Holding Company Status in Determining the Proportion of Lending to Corporations and Capitalists for Profit-Seeking Investments.

	(Standardized Regression Coefficients)					
Variables[1] Included in the Regression Equation	Corporate Interlocks	Total Employees in Interlocked Corporations	Upper Class Club Ties	Bank Deposits	Holding Company Status	R^2
A. All Variables	.288	.347	.152	−.049	.023	.436
B. Corporate Interlocks and Total Employees	.347	.384				.425
C. Corporate Interlocks, Total Employees and Upper Class Ties	.262	.349	.146			.434
D. Bank Deposits and Holding Company Status				.381	.104	.172
E. Corporate Interlocks, Total Employees, Upper Class Ties and Holding Company Status	.261	.345	.145		.020	.435
F. Corporate Interlocks, Total Employees, Upper Class Ties and Bank Deposits	.287	.351	.152	−.047		.435
G. Corporate Interlocks, Total Employees, Bank Deposits and Holding Company Status	.365	.382		−.031	.024	.426
H. Total Employees, Upper Class Ties, Bank Deposits and Holding Company Status		.400	.269	.058	.019	.407
I. Corporate Interlocks, Upper Class Ties, Bank Deposits and Holding Company Status	.395		.260	−.032	.066	.366

Comparisons of Relative Changes in R^2

	Equations Compared	Interpretation of the Comparison	Increment in R^2
1.	G–D	Corporate Interlocks and Total Employees. Net Bank Deposits and Holding Company Status.	.254
2.	G–B	Bank Deposits and Holding Company Status. Net of Corporate Interlocks and Total Employees.	.001
3.	A–B	Upper Class Ties, Bank Deposits and Holding Company Status. Net of Corporate Interlocks and Total Employees.	.011
4.	A–C	Bank Deposits and Holding Company Status. Net of Corporate Interlocks, Total Employees and Upper Class Ties.	.002
5.	A–D	Corporate Interlocks, Total Employees and Upper Class Ties. Net of Bank Deposits and Holding Company Status.	.254
6.	A–E	Bank Deposits. Net of Corporate Interlocks, Total Employees, Upper Class Ties and Holding Company Status.	.001
7.	A–F	Holding Company Status. Net of Corporate Interlocks, Total Employees, Upper Class Ties and Bank Deposits.	.001
8.	A–G	Upper Class Ties. Net of Corporate Interlocks, Total Employees, Bank Deposits and Holding Company Status.	.010
9.	A–H	Corporate Interlocks. Net of Total Employees, Upper Class Status, Bank Deposits and Holding Company Status.	.029
10.	A–I	Total Employees. Net of Corporate Interlocks, Upper Class Status, Bank Deposits and Holding Company Status.	.070

[1] Variables are described in Table 2.

increment in explanatory power is associated with the capitalist class structure measures (.264), the bank size and holding company measures do exhibit some independent strength here (.094). Nevertheless, when the effects of the other measures are controlled, bank size is actually positively related to mortgage lending activity. The measure of total interlocked employees has the greatest overall strength (an increment of .104 over the other four variables). Ties to upper class social organizations would appear to have a very weak independent influence in this instance.

e. Bank profits and loans to capitalists. The one empirical issue left to consider concerns the relationship between lending patterns and bank profitability. Table 5 presents the zero-order correlations between the study variables and profitability. The general finding is one of almost no relationship between profits and either the proportion of loans to capitalists (r = .016) or the proportion of loans for home mortgages (r = .010). Thus, there is no evidence here either that greater corporate or class interlocks or "blue-chip" corporate lending leads to high profits or that home mortgage lending hurts bank profits. This interpretation finds further support from several regression equations, not shown here, that examine the combined effect of the variables. For example, when the measures of loans to capitalists and loans for mortgages are examined together, the resulting regression equation explained virtually none of the variance in profitability ($R^2 = .0003$). If the profits of the 25 members of the Federal Reserve System are adjuisted upwards by 10% to reflect the reported disadvantages for such member banks which must maintain sizable interest-free balances in the Federal Reserve System, the results are only marginally affected. Thus, bank profitability is not explained by the measures incorporated in this study.

Discussion and Conclusion

This study has focused on the lending decisions of banks as important social consequences that are associated with the internal structure of the capitalist class. The findings presented here provide strong support for the thesis that bank lending practices are influenced by the linkages that banks have with the centers of economic power and upper class social interaction within the capitalist class. These data indicate that those banks which are most closely tied to these centers serve, in effect, as capital-accumulating appendages of core capitalist groups. The consequences of having strong linkages to such groups is, thus, that banks tend to direct their loans in ways that best meet the capital needs of these groups. The banks with the most extensive ties to the centers of the class have concentrated their loans with capitalist borrowers and have avoided loans in categories such as home mortgages that indicate a broader distribution of loan capital within a community.

These patterns appear to be relatively independent of the size of the banks. The banks emphasizing lending to capitalists are those ones whose directors are tied to numerous local corporations which represent major concentrations of economic power and, to a lesser extent, are tied to the most exclusive upper class social circles. In contrast, banks that occupy more marginal positions within the structure of the class, even if they are relatively large, are much less engaged in lending to capitalists and much more engaged in mortgage lending. An important part of the findings is that the dimension of corporate economic power, as measured by the total number of employees in corporations interlocked with each bank, is a major determinant in relation to other network characteristics of the flow of capital out of banks.

The director interlocks between large corporations and certain banks thus seem to embody the "personal union" of overlapping propertied interest that Hilferding (1923) portrayed as rising to dominance in capitalist societies. The mechanism of such dominance that is at issue here is the command of "finance capital," which Hilferding defined as "capital controlled by the banks and utilized by the industrialists" (Sweezy, 1942:266). The St. Louis data reveal that this personal union of bank and corporate interests at the centers of economic power and social prominence within the capitalist class is associated with a major flow of loan capital from the banks to corporations and other related borrowers.

The linkages to banks enable leading

Table 4. Comparisons of the Explanatory Power of Capitalist Class Connections, Deposit Size and Holding Company Status in Determining the Proportion of Lending for Home Mortgage Loans

	(Standardized Regression Coefficients)					
Variables[1] Included in the Regression Equation	Corporate Interlocks	Total Employees in Interlocked Corporations	Upper Class Club Ties	Bank Deposits	Holding Company Status	R^2
A. All Variables	−0.334	−0.422	0.011	0.382	−0.168	.328
B. Corporate Interlocks and Total Employees	−0.095	−0.420				.232
C. Corporate Interlocks, Total Employees and Upper Class Ties	−0.129	−0.434	0.059			.234
D. Bank Deposits and Holding Company Status				−0.020	−0.251	.066
E. Corporate Interlocks, Total Employees, Upper Class Ties and Holding Company Status	−0.124	−0.405	0.065		−0.144	.253
F. Corporate Interlocks, Total Employees, Upper Class Ties and Bank Deposits	−0.329	−0.455	0.007	0.363		.302
G. Corporate Interlocks, Total Employees, Bank Deposits and Holding Company Status	−0.328	−0.419		0.383	−0.168	.328
H. Total Employees, Upper Class Ties, Bank Deposits and Holding Company Status		−0.482	−0.125	0.258	−0.164	.289
I. Corporate Interlocks, Upper Class Ties, Bank Deposits and Holding Company Status	−0.463		−0.120	0.361	−0.220	.224

Comparisons of Relative Changes in R^2

	Equations Compared	Interpretation of the Comparison	Increment in R^2
1.	G–D	Corporate Interlocks and Total Employees. Net of Bank Deposits and Holding Company Status.	.262
2.	G–B	Bank Deposits and Holding Company Status. Net of Corporate Interlocks and Total Employees.	.096
3.	A–B	Upper Class Ties, Bank Deposits and Holding Company Status. Net of Corporate Interlocks and Total Employees.	.096
4.	A–C	Bank Deposits and Holding Company Status. Net of Corporate Interlocks, Total Employees and Upper Class Ties.	.094
5.	A–D	Corporate Interlocks, Total Employees and Upper Class Ties. Net of Bank Deposits and Holding Company Status.	.264
6.	A–E	Bank Deposits. Net of Corporate Interlocks, Total Employees, Upper Class Ties and Holding Company Status.	.075
7.	A–F	Holding Company Status. Net of Corporate Interlocks, Total Employees, Upper Class Ties and Bank Deposits.	.026
8.	A–G	Upper Class Ties. Net of Corporate Interlocks, Total Employees, Bank Deposits and Holding Company Status.	.000
9.	A–H	Corporate Interlocks. Net of Total Employees, Upper Class Status, Bank Deposits and Holding Company Status.	.039
10.	A–I	Total Employees. Net of Corporate Interlocks, Upper Class Status, Bank Deposits and Holding Company Status.	.104

[1] Variables are described in Table 2.

capitalist interests to greatly expand the scope of financial resources to which they have access. In recent decades commercial banks have become increasingly important sources of captial compared with holders of private funds, including not

Table 5. Zero-Order Correlations between Bank's Capitalist Class Connections, Size in Deposits and Loan Distribution and Bank Profitability

	Zero-Order Correlation with Bank Profitability**
1. Combined Loans to Capitalists (Percentage of Total Loans)*	−.016
2. Loans for Home Mortgages (Percentage of Total Loans)*	.010
3. Number of Top 300 Corporations with which Bank Is Interlocked*	.045
4. Total Number of Employees in Top 300 Local Corporations with Which Bank Is Interlocked*	.002
5. Proportion of Bank's Directors Who Belong to Top Upper Class Organizations*	.060
6. Total Deposits of the Bank*	.201
7. Holding Company Status of bank*	.049
(N = 77)	

* Measures described in Table 2.

** Profitability is defined as the net income of the bank before taxes and before security gains and losses in 1975 divided by the total assets of the bank as of December 31, 1975 (Reed et al., 1976:434).

only large corporations but also investment bankers and other private investors (Soref, 1980). Much of the money in commercial banks comes from a diverse range of deposits, including individual checking and savings accounts, government accounts, certificates of deposit held by pension funds and other similar nonbusiness institutions, accounts of churches and other private organizations and small business accounts, and would not be as available for corporate loans if banks tied to corporate interests did not serve to accumulate these funds (Reed et al., 1976:74–9).

This analysis does not support the arguments of Hilferding regarding the ascendancy of bank dominance over corporations. The argument offered here is that it is not likely to be useful to attempt to locate the base of these power relationships in either banks or corporations as distinct organizations. Rather, I have portrayed a model of capitalist class coordination in which control is most likely to be located in larger structures of propertied interests that tie banks and corporations together. The data presented here are not sufficient to validate this model in any ultimate sense. Certainly, any thesis of bank dominance is contradicted by the finding in this analysis that bank profitability is not associated either with extensive corporate interlocks or with patterns of concentrated lending to capitalists. Nevertheless, it would be arguable that these findings would be consistent with a model of corporate dominance over banks if it could be shown that the corporations most interconnected with banks, and most favored in the receipt of bank loans, tended to be more profitable. In such a model, corporations might be seen as forcing banks to provide loans to them on advantageous terms, thus, making the banks less profitable. However, such an interpretation is highly tenuous given the lack of data suggesting that corporations are in a position to dominate and control banks (Kotz, 1978; Soref, 1980).

These patterns in the command of capital are not only important in regard to the operation of banks, but also relate to the health and vitality of the entire metropolitan economy. When certain borrowers gain sufficient capital through loans, a normal consequence is that other borrowers are denied such capital. Moreover, in St. Louis, which can be characterized as a declining industrial area, the concentration of loans found among capitalist borrowers probably represents a significant outflow of capital from the metropolitan area. Corporate investments are most likely to be made in higher growth areas, and since large corporations operate on a multistate and often multinational basis, they are in an advantaged position with respect to the transfer of capital investments. Here, it should be noted that all interpretations regarding the geographic transfer of capital must necessarily extend beyond the data presented in this analysis. It is to be remembered that the data used here only identify the categories of borrowers and do not reveal the particular uses or the geographic locations of investments resulting from these loans. Nevertheless, there is considerable evidence that in the last several decades the largest St. Louis based corporations have moved a major share of their investments and jobs out of the St. Louis area (Defty, 1975; *Business*

Week, 1976). The transfer of capital out of the St. Louis area has also been clearly documented in terms of the mortgage lending activities of St. Louis financial institutions (Ratcliff, 1977).

The evidence thus suggests that St. Louis, despite its image as a "declining" older industrial metropolis, is actually a producer of a net capital "surplus" that is exported for investment elsewhere, presumably especially in the "Sunbelt" and other high growth areas. This outflow of capital certainly does not represent a lack of investment needs in the St. Louis area. If St. Louis were to reverse the long-term decline in the number of manufacturing jobs, and if the housing needs of the area's residents were to be met, sizable investments would be an absolute necessity. The viability of the St. Louis area, as is the case with other older industrial areas, is very much dependent on the success of locally oriented investors in obtaining sufficient investment capital. Given these currents of investment and disinvestment within an older metropolitan area, the leading St. Louis capitalists appear to be playing complex and even contradictory roles. At the same time that they are most closely tied to locally based structures of power and prominence, they also appear to be most involved in the outward flow of investments from the metropolis. These tensions reflect the dual needs of the leading capitalists to protect both their local class bases and their long-term interests by taking advantage of national economic trends (Ratcliff, 1980a). Because many of the banks most centrally located in the capitalist class are also the ones that are progressively growing larger and more powerful among banks, the prospect for the future is that the imbalances of capital concentration and capital disinvestment will grow even more severe.

REFERENCES

Aiken, Michael and Paul E. Mott
1970 The Structure of Community Power. New York: Random House.
Allen, Michael Patrick
1974 "The structure of interorganizational elite cooptation: interlocking corporate directorates." American Sociological Review 39:393–406.
1976 "Management control in the large corporation: comment on Zeitlin." American Journal of Sociology 81:885–94.
1978 "Economic interest groups and the corporate elite structure." Social Science Quarterly 28:597–615.
Baltzell, E. Digby
1958 Philadelphia Gentlemen: The Making of a National Upper Class. New York: Free Press.
Bearden, James, William Atwood, Peter Freitag, Carol Hendricks, Beth Mintz, and Michael Schwartz
1975 "The nature and extent of bank centrality in corporate networks." Paper presented at the annual meeting of the American Sociological Association, August.
Bonacich, Phillip and G. William Domhoff
1977 "Overlapping membership among clubs and policy groups of the American ruling class: a methodological and empirical contribution to the class-hegemony paradigm of the power structure." Paper presented at the annual meeting of the American Sociological Association, September.
Bradford, Calvin and Dennis R. Marino
1977 "Redlining and disinvestment as a discriminatory practice in residential mortgage loans." Prepared for U.S. Department of Housing and Urban Development. Washington, D.C.: U.S. Government Printing Office.
Brandeis, Louis
1913 "The endless chain: interlocking directorates." Harper's Weekly, December 6:13–5.
Bunting, David and Tsung-hua Liu
1977 "Economic and social aspects of interlocking." Paper presented at the annual meeting of the American Sociological Association, August.
Burt, Ronald S.
1978a "A structural theory of interlocking corporate directorates." Survey Research Center Working Paper #13, University of California-Berkeley.
1978b "Cooptive corporate actor networks: a reconsideration of interlocking directorates involving American manufacturing." Survey Research Center Working Paper #18, University of California-Berkeley.
1980 "Autonomy in a social topology." American Journal of Sociology 85:892–925.
Business Week
1976 "Emerson Electric's rise as a low cost producer." November 1:47–8.
Clark, Terry Nichols
1975 "Community power." Pp. 271–96 in Alex Inkeles, James Coleman and Neil Smelser (eds.), Annual Review of Sociology. Palo Alto: Annual Reviews.
Defty, Sally Bixby
1975 "New jobs called top area need." St. Louis Post-Dispatch. In a special supplement, "Toward 2000: St. Louis Horizons."
Domhoff, G. William
1970 The Higher Circles: The Governing Class in America. New York: Vintage.

Gogel, Robert M.
1977 Interlocking Directorships and the American Corporate Network. Ph.D. Dissertation, Department of Sociology. University of California, Santa Barbara.

Hartman, Chester
1974 Yerba Buena: Land Grab and Community Resistance in San Francisco. San Francisco: Glide.

Harvey, David
1975 "The political economy of urbanization in advanced capitalist societies: the case of the United States." Pp. 119–63 in Gary Gappert and Harold M. Rose (eds.), The Social Economy of the Cities. Beverly Hills: Sage.

Hayes, Edward C.
1972 Power Structure and Urban Policy: Who Rules in Oakland? New York: McGraw-Hill.

Hilferding, Rudolf
[1910] 1923 Das Finanzkapital. Wiener Volksbuchhandlung. Wien.

Kotz, David M.
1978 Bank Control of Large Corporations in the United States. Berkeley: University of California Press.

Leinsdorf, David and Donald Etra
1973 Citibank: Ralph Nader's Study Group Report on First National City Bank. New York: Grossman.

Levine, Joel
1972 "The sphere of influence." American Sociological Review 37:14–27.
1977 "The network of corporate interlocks in the United States: an overview." Paper presented at the Advanced Symposium on Social Networks, Dartmouth College, August.

Mariolis, Peter
1975 "Interlocking directorates and control of corporations: the theory of bank control." Social Science Quarterly 56:425–39.
1977 "Interlocking directorates and finance control: a peak analysis." Paper presented at the annual meeting of the American Sociological Association, September.

Mills, C. Wright
1956 The Power Elite. New York: Oxford University Press.

Mintz, Beth and Michael Schwartz
1977 "The structure of power in American business." Paper presented at the annual meeting of the American Political Science Association, September.

Norich, Sam
1980 "Interlocking directorates, the control of large corporations, and patterns of accumulation in the capitalist class." Pp. 83–106 in Maurice Zeitlin (ed.), Classes, Class Conflict and the State. Cambridge, Mass.: Winthrop.

Pennings, Johannes
1978 "Interorganizational relationships: the case of interlocking directorates." Paper presented at the American Sociological Association meetings, San Francisco, August.

Ratcliff, Richard E.
1977 "Mortgage investment and disinvestment: the record of banks and savings and loan associations in the St. Louis area." In Housing and Neighborhood Investment in Missouri. Missouri Housing Development Commission, September.
1980a "Declining cities and capitalist class structure." Pp. 115–38 in G. William Domhoff (ed.), Power Structure Research. Beverly Hills: Sage.
1980b "Banks and the command of capital flows: an analysis of capitalist class structure and mortgage disinvestment in a metropolitan area." Pp. 107–32 in Maurice Zeitlin (ed.), Classes, Class Conflict and the State. Cambridge, Mass.: Winthrop.

Ratcliff, Richard E., Mary Elizabeth Gallagher and Kathryn Strother Ratcliff
1979 "The civic involvement of bankers: an analysis of the influence of economic power and social prominence in the command of civic policy positions." Social Problems 26:298–313.

Reed, Edward E., Richard V. Cotter, Edward K. Gill and Richard K. Smith
1976 Commercial Banking. Englewood Cliffs, N.J.: Prentice Hall.

Schuessler, Karl
1971 Analyzing Social Data. Boston: Houghton Mifflin.

Smith, Ephraim and Louis R. Desfosses
1972 "Interlocking directorates: a study of influence." Mississippi Valley Journal of Business and Economics 7:57–69.

Sonquist, John A. and Thomas Koenig
1975 "Interlocking directorates in the top U.S. corporations: a graph theory approach." Insurgent Sociologist 5:196–229.

Soref, Michael
1980 "The finance capitalists." Pp. 62–82 in Maurice Zeitlin (ed.), Classes, Class Conflict, and the State. Cambridge, Mass.: Winthrop.

Stanworth, Philip and Anthony Giddens
1975 "The modern corporate economy: interlocking directorships in Britain, 1906–1970." Sociological Review 23:5–28.

Starr, Roger
1975 Housing and the Money Market. New York: Basic.

Sweezy, Paul
1942 The Theory of Capitalist Development. New York: Monthly Review.
1953 "Interest groups in the American economy." Pp. 158–88 in Paul M. Sweezy (ed.), The Present As History. New York: Monthly Review.

Thompson, James
1967 Organizations in Action. New York: McGraw-Hill.

U.S. Senate Committee on Banking
1977 Community Credit Needs. Hearing Before the Committee on Banking, Housing, and

Urban Affairs. Washington, D.C.: U.S. Government Printing Office.

U.S. Senate Committee on Governmental Affairs
1978 "Interlocking directorates among the major U.S. corporations." Staff study prepared by the Subcommittee on Reports, Accounting, and Management. Washington, D.C.: U.S. Government Printing Office, January.

Useem, Michael
1978 "The inner group of the American capitalist class." Social Problems 25:225–40.
1979 "Studying the corporation and the corporate elite." American Sociologist 14:97–107.

Warner, W. Lloyd and Darab B. Unwalla
1967 "The system of interlocking directorates." Pp. 121–57 in Lloyd W. Warner (ed.), The Emergent American Society Large-Scale Organizations. New Haven: Yale University.

Zeitlin, Maurice
1976 "On class theory of the large corporations: response to Allen." American Journal of Sociology 81:894–904.

MANUSCRIPTS FOR THE *ASA ROSE SOCIOLOGY SERIES*

Manuscripts (100 to 300 typed pages) are solicited for publication in the *ASA Arnold and Caroline Rose Monograph Series*. The Series welcomes a variety of types of sociological work—qualitative or quantitative empirical studies, and theoretical or methodological treatises. An author should submit three copies of a manuscript for consideration to the Series Editor, Professor Suzanne Keller, Department of Sociology, Princeton University, Princeton, NJ 08540.

UNCONFOUNDING THE CONFLUENCE MODEL: A TEST OF SIBSHIP SIZE AND BIRTH-ORDER EFFECTS ON INTELLIGENCE*

LALA CARR STEELMAN AND JAMES A. MERCY
Emory University

American Sociological Review 1980, Vol. 45 (August):571–582

This paper explores the theoretical validity of the confluence model in explaining the effects of sibship size and birth order on intelligence. Using a nationally representative sample the effects of age, sex, marital disruption, socioeconomic status, race, and other potentially confounding variables, unincorporated in other studies, are controlled. The results suggest that (a) the inverse relation between sibship size and IQ lessens for the economically advantaged, (b) the only child breaks the otherwise consistent linear decline between sibship size and IQ in the below-poverty condition only, (c) the nonteacher handicap for only children is questionable, and (d) close spacing, as Zajonc defines it, may have detrimental consequences for children irrespective of birth order.

INTRODUCTION

The confluence model, an explanation of cognitive differences, is in vogue today. The objective of this paper is to examine the impact of its two primary variables, sibship size and birth order, on the intellectual development of children (Zajonc and Markus, 1975).

The problem with which the confluence model deals has generated a considerable body of research; however, further study is justified for two reasons. First, previous research has yielded inconsistent and even contradictory findings. Secondly, the theoretical explanations and hypotheses offered for the results derived are inconsistent. The source of these deficiencies stems from a lack of methodological rigor, particularly in regard to the lack of control over important alternative explanatory variables, the nature of the samples used, and the use of different measures of the dependent variable (i.e., intellectual development) from study to study.

Because of the methodological shortcomings of prior studies, this problem requires rethinking and redoing. The strategy of this paper is to take a critical look at explanations advanced by others to account for sibship size and birth-order effects through the analysis of a more appropriate and representative data source. Our study addresses (under more stringent conditions than have been used previously) the validity of the confluence model in predicting cognitive outcomes.

THEORETICAL REVIEW

Many previous scholars have offered ad hoc explanations to account for the effects of sibship size and birth order on intelligence. The bulk of the evidence suggests that birth order taken alone inversely relates to intellectual performance, and, similarly, sibship size taken alone inversely relates to intellectual performance. To explain these patterns, several hypotheses have been postulated: physiological, differential treatment of children by parents, confluence model, and admixture explanations.

The physiological explanation of birth-order effects proposes that the mother's uterus becomes fatigued with each successive pregnancy through loss of essential nutrients, and that this depletion leads to eventual psychological differences (Belmont and Marolla, 1973; Bayer, 1967). Record et al. (1970) cite evidence

---

* Direct all communications to: Lala Carr Steelman; Department of Sociology; Emory University; Atlanta GA 30322.

The authors wish to express their gratitude to Alan Lizotte for his helpful criticisms and guidance. This work was, in part, supported by NICHD contract #N01-HD-92802. We are grateful to the principal investigators of this project, John Doby, S. Mitra, and Martin Levin for making these data available to us. We would also like to thank Carl W. Tyler, Jr., Rose McGee, Maggie Stephens, and two anonymous reviewers for their assistance.

taken from twin studies which make this argument less viable. Twins score lower than nontwins on IQ tests. But twins whose cotwins die during the first four weeks after birth score equal to nontwins (Record et al., 1970). The authors suggest that this points to the importance of the postnatal environment in affecting cognitive outcomes (Record et al., 1970). However, nutrients are not necessarily distributed evenly between twins in the prenatal environment; thus, the possibility that cognitive differences may be linked to prenatal development still remains (Bulmer, 1970:34–5). Although the physiological view may have validity, no attempts are made to deal with it in this paper because of inadequate information.

Another explanation offered is that, as family size increases, deficiencies or changes in parental care take place. When families grow, parents have less time to devote to later children, thus the latter spend more time in the company of siblings (Nisbet and Entwistle, 1967). This time factor is particularly relevant to the mother since she has traditionally been held responsible for caretaking duties (Kagan, 1977). Parental expectations may also vary by birth order with parents showing greater concern over the accomplishments of the early born, especially the firstborn, and becoming increasingly more lax in their attitudes towards achievement with each new child (Sampson, 1965). Data used to substantiate this position fail to measure whether or not differences in parental treatment actually occur and then translate into test differences. They just assume that they do.

The effect of birth order by itself on intellectual performance generally indicates that firstborn excel over others on ability tests (Altus, 1966), eminence (Schacter, 1963), likelihood of earning a doctorate (Bayer, 1967), college attendance (Bayer, 1966; Glass et al., 1974) and verbal ability scores (Glass et al., 1974). But Glass's (1974) findings only held for those from higher socioeconomic backgrounds, and Schacter (1963) attributed his result to the marked surplus of firstborn in the college population cohort. As an independent variable, family size (defined as the number of siblings) consistently appears to relate negatively to intellectual level (Anastasi, 1956; Zajonc et al., 1979). Nisbet and Entwistle's (1967) inquiry into size effects on the test scores of Scottish children found the typical inverse relation, but the impact was more pronounced in the lower class than in the higher class. Similarly, Marjoribanks et al. (1975) found that sibsize effects were ameliorated by the social class background of the test-taker. Rankin et al. (1979) tested the impact of sibsize on the test scores of Samoan children and found a curvilinear relation with those from both larger and smaller families underscoring those in between. The findings on size and order effects are discrepant, especially in the presence of social class indicators, but they generally convey this message: those reared in small families or those born early score higher on tests of intellectual development. Interpretation of these early efforts is made difficult by the strong interrelationship between sibship size and birth order. Probably because of small sample sizes, some of the pioneer studies did not examine the independent effects of size or order on intellectual performance while holding the other variables constant (Adams, 1972).

It took Belmont and Marolla's (1973) study to pave the way. Their investigation delved into the performances of 386,114 Dutch men, born between 1944 and 1947, who represented the entire male population for this age cohort on the Raven Progressive Matrices. During the time that these subjects were born, a severe famine took place in the Netherlands. Nevertheless, maternal malnutrition was not found to affect the intellectual performance of survivors at military age (Stein et al., 1972). In this study two ideas were tested: whether there were birth-order effects irrespective of sibship size and whether there were size effects irrespective of birth order (Belmont and Marolla, 1973). Next, mean Raven scores were examined under varying conditions of father's occupational status, defined as manual, nonmanual, and farm. Birth-order effects persisted across class conditions, while size effects diminished only under the farm condition. Generally, the data indicated separate effects for each variable. This meant that the firstborn scored higher than secondborn and so on successively, and, further, that the firstborn in small families scored higher than the firstborn in large ones. One irregularity from this otherwise consistent linear decline was the only child, who, given the trend, should have scored highest, but who, instead, ranked parallel to the firstborn of four. Also, the rate of decline for the last born, in relation to the next to last born, was greater than for other adjacent siblings.

In another study, Breland (1974) elaborated on the effects of sibship size and ordinal position on student performance on the National Merit Scholarship Qualifying Test taken by gifted tenth- through twelfth-graders. He concluded that the early born from small families scored highest and the later born from large families scored lowest on verbal subtests, but not on nonverbal ones. He too found the only child exception. Existing relationships were neither altered by the presence of the covariates (family income, mother's age, mother's education and father's education), nor by the sexual composition of the sibship. These two foregoing studies added new dimension to the problem sparking the acclaimed and tentative theory that follows.

In response to these new findings, Zajonc and Markus (1975) proposed the confluence model. Under this model, a child's intelligence is seen as a function of the intellectual environment in which he grows up. "Intellectual environment can be thought of in this context as being some function of the absolute intellectual level of its (the family's) members" (Zajonc, 1976:227). Membership includes the child. Children are not born with the same intelligence as adults; they must develop it. Therefore, sibship size bears directly on a child's development because the greater the number of children relative to the number of adults, the less beneficial the intellectual milieu. The birth-order effect, however, is an indirect product of spacing. If one considers a pair of siblings, the younger one would suffer more from close spacing than the older one. To substantiate this point, Zajonc (1976) observed that birth-order effects happened in some samples but not in others. When they did not occur, Zajonc (1976) attributed it to wider spacing practices. To assess this possibility, he used birthrates as an indirect measure of spacing and assumed that the higher the birthrate, the shorter the average intervals separating children. Comparing the birthrates for samples where birth-order effects did and did not occur, Zajonc (1976) found that birthrates were indeed lower for the latter samples, a difference he used as support of the spacing/birth-order factor.

One other factor is included in the model to account for the only and last child exceptions. As it has been explained so far, the confluence model would predict that the only child should rank highest on intellectual performance because the only child is reared in an environment undiluted by the presence of other children. Added to the model, then, is the tutoring factor; namely, that children profit from teaching younger children because it gives them the opportunity to experience the active part of learning. The benefits of being an only child are, hence, counteracted by the only child's nonteacher handicap (Zajonc and Markus, 1975). Similarly, the steeper drop in test scores of the last born as compared to other adjacent siblings is attributable to the same handicap. But as Falbo (1978) observes, the nonteacher handicap does not explain why the secondborn in families of two outperform only children. Zajonc (1976) took the confluence model, applied it to SAT outcomes, and found a good fit. Davis et al. (1977) also found support for the confluence model on Israeli eighth-graders' performances on mathematical subtests. However, in their study the last born sometimes showed an unanticipated upswing in performance. Two studies, in which lower class subjects were excluded from the sample, found only a weak connection between the confluence model and test performance (Cicirelli, 1976; Grotevant et al., 1977). Though the data reviewed so far generally support the confluence model, many of their implications remain untested. This model awaits a rigorous test that uses more precise measures of the crucial factors involved.

Recently reawakened interest in birth-order effects has prompted critics to exclaim, "Not here, not now!" (Schooler, 1972). Opponents of birth-order effects have tried to erase fascination with it, arguing that the correlation between birth order and intellectual competence is due to class differences in fertility. The role that parent's social class plays on a child's progress is enormous. Kagan (1977) called it one of the few firm facts in psychology. Larger families are more likely to come from the lower class; thus, size effects may be mediated by socioeconomic differences.

Page and Grandon (1979) noted that interfamily differences, like socioeconomic status, might play a larger role on cognitive development than the confluence variables. They claimed that the apparent effects of family size, derived from aggregate data, far from explaining population differences, are better accounted for as a result of group admixtures, defined as differences such as social class or ethnic group membership. Their study shows that in the presence of social class indicators, the importance of the confluence model variables is diminished. Using data taken from two large samples of United States and Colombian student test-takers, the confluence model explained less than 5% of the variance in the United States sample and less than 1% in the sample of Colombian students (Page and Grandon, 1979; Velandia et al., 1978).

Similarly Kunz and Peterson (1977) found that size and order effects disappeared when controlling for social class. Their data consisted of the grade-point averages of over 6,000 high school students from across the nation. Using linear regression techniques, they examined the effects of class, size, and order on grade-point variation. Class accounted for a significant portion of the variance (still, only 6%) while the other two predictors had little explanatory value. Grade-point average, nonetheless, is a weak measure of cognitive ability when compared with standardized test scores.

So the results on sibship size, birth order and social class effects are mixed. Belmont and Marolla (1973) found consistent effects across classes, Breland (1974) found effects across classes but only on verbal subtests, and Kunz and Peterson (1977) found effects vanishing upon control of social class. Others (Page and Grandon, 1979; Velandia et al., 1978) have shown that the explanatory power of the confluence model diminishes in the presence of socioeconomic and other between-group differences.

Another source of confusion in prior studies is their failure to take other explanatory factors, aside from socioeconomic status, into account. Zajonc (1976) acknowledges that differences in psychological outcomes might be due to factors outside the confluence model. Two of these confounding factors are race and marital disruption. The factors by no means exhaust the list of plausible alternatives. They represent only two of many factors. The consequences of marital disruption (widowhood, divorce, separation, remarriage, and step-parenthood) have all been linked to the impeded cognitive development of children, since parents serve as role models, resource providers, nurturers, and socializers. For one person to manage all of these diverse tasks alone is difficult. Zajonc (1976) fits marital disruption into the confluence model saying that the intellectual environment is dampened by parent absence because it lowers the number of adults figuring into the overall intellectual average. Unfortunately, one cannot assume that marital disruption distributes itself evenly across family sizes. There is evidence to the contrary. For example, Falbo's (1978) recent reinterpretation of Breland's (1974) data highlights the uneven nature of this distribution. Using census figures for women who had completed the childbearing years, Falbo (1978) estimated that only children were 5% more likely to reside in father absent households than multiple children. This factor alone accounted for 25% of the only child deviation from the inverse linear trend that Breland (1974) found connecting sibship size to test performance.

Similarly, race may influence the findings because blacks show both higher fertility and lower test performances than whites. Zajonc (1976) subsumes race under the confluence model noting that blacks are more likely than whites to be located in family configurations which are less conducive to development. The black configuration is apt to be characterized by father absence and closer spacing between siblings. Failure to control for these alternative factors in prior studies makes their results somewhat suspicious. Many of the foregoing problems are solved in this paper because of the quality of the sample studied. These new data permit a stronger test of the confluence model and the competing explanations given to account for sibship size and birth-order effects on intellectual capacity.

SAMPLE

The data used in this paper come from Cycle II of the Health Examination Survey. Fieldwork for this project was conducted from July, 1963, to December, 1965. The survey utilized a multistage, stratified probability-sampling technique of loose clusters of persons in land-based segments. Of the originally selected subjects, 96% were included in the final sample. The sample is thought to be highly representative of noninstitutionalized American children, excluding Indians living on reservations. These youths range in age from 6 to 11, which is a limitation to this study because they may represent incomplete families. Nevertheless, information on the family structure at the time of the child's examination is available.

The tests used to measure the dependent variable are the child's standardized scores on the Wechsler vocabulary and block-design subtests.[1] These exams are normalized by four-month age intervals and adjusted for sex differences. Trained psychologists administered them to the subjects individually.

Other relevant information collected in the survey include birth certificate information and interviews with the child's parents. The cases that were picked for analysis provide a tighter test of the confluence model through the elimination of confounding factors.

Only the following subjects are included:

1. Whites.
2. Children residing in homes with both of their natural parents and no other adults over 20 years old (this stipulation removed another possible contaminant—the possible influence of extended family members or other adults).
3. Children whose parents answered no to the following question, "Are there children living outside the household?"[2]

These criteria were met by 3,428 subjects. Since the oldest child could be no older than 20, densely spaced families are likely to be overselected. Zajonc (1976) anticipated birth-order effects under conditions of close spacing, which is defined as high birthrates. This sample comes from a cohort whose birthrates were high ranging from 23.1 to 24.2 by Vital Statistics estimates (U.S. Dept. HEW., 1969). For all of the above reasons, this sample should provide a stronger test of the confluence model than has been done previously. For further description of these data, see NCHS publications (1967; 1971a; 1971b; 1972a).

ANALYTIC TECHNIQUE

The data are analyzed in three phases. First, the IQ means are cross-classified by sibship size and birth order while controlling for poverty status (defined as above and below $4,000 annual family income). This cutoff point was chosen because it represents the nearest category to the poverty threshold in 1964.[3] This distinction provides the clearest separation of the lower from the middle and upper classes. In the second phase, a regression analysis was performed. This analysis was done separately for above- and below-poverty status subjects. It was decided that the cross-classification tables, when controlling for poverty status, revealed patterns which were different enough to justify such an approach. In the third phase, a partial correlation analysis was done (again, separately for above- and below-poverty groups) to clarify the magnitude of the effect of each of the independent variables on the dependent variable, while controlling for the presence of other variables in the models.

---

[1] These subtests represent only two of the 12 tests that comprise the Wechsler Intelligence Scale. The sum of the scores for the vocabulary and block design subtests correlate .88 with the full Wechsler Intelligence Scale according to NCHS (1972b) estimates.

[2] This was done because the measure of birth order was the age rank of the child among children under 20 years of age in the household. Therefore, only cases where birth order was equal to age rank are included.

[3] Given the constraints of the data, we decided that the fairest measure of poverty status is based on the average family size in our subsample which is 5.7. using estimates that represent the population as of March, 1964 for family sizes of five and six ($3,685 and $4,135, respectively), we derived an estimate for a mean family size of 5.7 by taking the difference between income values for families of five and six, multiplying that difference by .7, and adding that

For the purposes of the regression and correlational analyses, the dependent and independent variables are operationally defined in the manner described in Table 1.

Table 1. Variables Used in Regression and Correlational Analysis and Their Operational Definitions, Means, and Standard Deviations

Status Level	$\bar{X}$	S.D.
Below Poverty Status		
IQ Scores, Index of Intellectual development (IQ)—sum of the Wechsler Vocabulary and Block Design Test Scores	94.29	13.87
Sibship Size (S)—ranges from 1 to 16	4.59	2.27
Birth Order (B)—ranges from 1 to 10+	2.47	1.54
Income (I)—1 = < $500; 2 = $500–999; 3 = $1,000–1,999; 4 = $2,000–2,999; 5 = $3,000–3,999	3.94	1.17
Mother's Working Status (M)—the usual activity of mother; 0 = mother working, 1 = mother not working	.80	.40
Above-Poverty Status		
IQ Scores, Index of Intellectual Development (IQ)—sum of the Wechsler Vocabulary and Block Design Test Scores	105.58	13.03
Sibship Size (S)—ranges from 1 to 11	3.61	1.63
Birth Order (B)—ranges from 1 to 10+	2.25	1.23
Income (I)—6 = $4,000–4,999; 7 = $5,000–6,999; 8 = $7,000–9,999; 9 = $10,000–14,999; 10 = $15,000+	7.82	1.09
Mother's Working Status (M)—the usual activity of mother; 0 = mother working, 1 = mother not working	.72	.45

RESULTS

In Table 2a the mean scores are presented by sibship size and birth order. A check of the marginal mean scores shows a monotonic linear decline in score by birth order and a monotonic linear decline in score by sibship size with the exception of the only child. This pattern adheres to confluence model predictions. But closer examination of the within-cell scores shows an inconsistent effect of birth order within sibship sizes. Marginal means which suggest birth-order effects are a function of the correlation between sibship size and birth order.

Tables 2b and 2c illustrate the array of means cross-classified further into above- and below-poverty conditions. For the above-poverty group, IQ scores show a consistent mean decline by size, with no exceptions. For the below-poverty group, the same pattern exists except for the only child who scores parallel to the firstborn of five. However, the birth-order effect fails to hold within sibship sizes for either the above- or below-poverty status groups. Clearly, controlling for class considerably alters the nature of the relationship between sibship size and IQ as is further demonstrated by an inspection of the zero-order correlation coefficients presented in Table 3. These figures indicate that sibship size has a much stronger negative correlation with IQ in below-poverty (−.23) than in above-poverty groups (−.13). Notice that this result holds despite the deviation of the only child from a linear trend in the below-poverty condition.

Table 4 shows the full and the final regression models for prediction of IQ scores. Unstandardized regression coefficients are used to measure effects. Inspection of these results demonstrates that income and sibship size are the most powerful predictors of IQ scores of the variables included in the final model for

---

figure to $3,685. This yields an approximate poverty income cutoff point of $4,000 which corresponds to one of the categorical cutoff points in our data (Orshansky, 1965).

Table 2a. Mean IQ by Birth Order by Sibship Size

	Birth Order						
Sibship Size	1	2	3	4	5	6+	Row Mean
1	105.90	—	—	—	—	—	105.90
2	106.45	106.67	—	—	—	—	106.56
3	105.19	106.22	107.22	—	—	—	106.18
4	102.44	102.58	103.20	105.39	—	—	103.20
5	102.85	100.57	102.19	103.01	100.63	—	101.95
6+	92.71	94.55	97.27	99.57	100.95	98.91	98.09
Column Mean	104.64	104.49	103.75	102.83	100.87	98.91	
Total x̄ = 103.99	N = 3428						

Table 2b. Mean IQ by Birth Order and Sibship Size among Below-Poverty Status Subjects

	Birth Order						
Social Class and Sibship Size	1	2	3	4	5	6+	Row Mean
1	90.46	—	—	—	—	—	90.46
2	97.24	99.46	—	—	—	—	98.06
3	93.03	97.86	105.21	—	—	—	97.94
4	95.74	97.79	95.98	96.50	—	—	96.46
5	91.94	93.83	91.47	96.91	95.00	—	93.45
6+	86.79	89.82	90.00	91.93	92.09	85.00	89.64
Column Mean	93.90	96.05	95.22	93.94	92.79	85.00	
Total x̄ = 94.29	N = 483						

Table 2c. Mean IQ by Birth Order and Sibship Size among Above-Poverty Status Subjects

	Birth Order						
Social Class and Sibship Size	1	2	3	4	5	6+	Row Mean
1	108.05	—	—	—	—	—	108.05
2	107.65	107.19	—	—	—	—	107.41
3	106.69	106.92	107.40	—	—	—	106.99
4	104.09	103.45	103.87	106.15	—	—	104.21
5	105.34	102.52	103.99	104.05	102.35	—	103.01
6+	97.59	97.62	100.70	102.52	104.15	104.31	101.92
Column Mean	105.45	105.65	104.98	104.56	103.65	104.31	
Total x̄ = 105.58	N = 2945						

both below- and above-poverty conditions. Closer scrutiny of these coefficients shows that the gradient of income effects on IQ scores is greater in above- (3.12) than in below-poverty groups (2.60). Conversely the gradient of the effect of sibship size appears to lessen more in the above- (−1.00) than in the below-poverty group (−1.22). However, a test of the null hypothesis that the corresponding slopes for income and sibship size are equal across the models cannot be rejected on the basis of statistical significance (Dixon and Massey, 1969:209). While these results suggest a trend in which the gradients of the effects of income and sibship size on IQ scores are changing as social class increases, the result may be due to chance.

Another important distinction between the two models is the inclusion of mother's working status in the final model for the above-poverty group and its absence in the below-poverty final model. Only in the above-poverty group does the mother's continual presence in the home relate positively to IQ.

The variance explained by the confluence variables, sibship size and birth

Table 3. Zero-Order Correlation Coefficients

	Sibship Size	Birth Order	Income	Mother's Working Status
All Subjects				
IQ	−.20	−.07	.38	.01
Sibship Size		.65	−.19	.12
Birth Order			−.02	.00
Income				−.03
Mother's Working Status				
Below-Poverty Status Subjects				
IQ	−.23	−.09	.24	−.03
Sibship Size		.70	−.13	.06
Birth Order			.03	−.05
Income				.10
Mother's Working Status				
Above-Poverty Status Subjects				
IQ	−.13	−.05	.27	.03
Sibship Size		.64	−.05	.12
Birth Order			.03	.00
Income				.01
Mother's Working Status				

order, amounts to 4.5% among all subjects when these variables are considered by themselves in the model. This figure approximates that of the 4.6% of the variance explained found by Page and Grandon (1979) owing to confluence variables in a sample of U.S. high school students.[4] However, when comparing the amount of variance explained by sibship size and birth order across poverty status, we find they account for 6.1% in the below-poverty group and only 2.0% in the above-poverty group. Sibship size accounts for the great majority of the variance explained in both cases.

Thus, the regression analysis reveals five important results. First, income and sibship size, in that order, are the most important predictors of IQ scores in both models. Second, birth order fails to relate significantly to outcomes in either model. Third, a comparison of the corresponding slopes of income and IQ in the two models reveals the gradient of their effects is not statistically different. However, an interesting trend is suggested. Fourth, mother's working status is a significant independent variable only in the above-poverty group. Fifth, the confluence model has greater explanatory power among below-poverty status subjects than among those in the above-poverty status.

Tables 5 and 6 show the results of the correlations of each of the independent variables with IQ scores while systematically partialling out the effects of the other independent variables. Table 6 shows that, when controlling for all the variables in the below-poverty condition, only income and sibship size correlate with IQ significantly. This result is consistent with the corresponding regression model for the below-poverty group. The contribution of income to variance reduction in the full model (.21) is slightly greater than that of sibship size (−.18). Similarly, Table 7 shows that all of the third-order partial correlations in the above-poverty group are significant, with birth order barely significant. But in this case income's contribution to variance reduction (.26) is greater relative to that of sibship size (−.12) as compared with the findings in the below-poverty condition.

To determine whether income and sibship size provide a significantly better or worse fit of the data in the below- than in the above-poverty group, a test of the null hypothesis that the corresponding partial correlation coefficients for income and sibship size in the two models were equal was performed (Blalock, 1972:406). This

---

[4] Page and Grandon (1979) also included "birth order squared" and "last child" among the confluence variables. However, neither variable contributed more than a negligible amount to the variance explained.

Table 4. Unstandardized Variables and Their Standard Errors in Parentheses Predicting IQ Scores

Independent Variables	Full Model	Final Model
Below-Poverty Status		
Intercept	90.5104	89.6383
Income	2.5313 (.5305)	2.6084 (.5184)
Sibship Size	−1.5531 (.3837)	−1.2235 (.2667)
Birth Order	.7042 (.5625)	
Mother's Working Status	−0.9899 (1.5369)	
Adjusted R²	.0962	.0957
Above-Poverty Status		
Intercept	83.8592	83.8692
Income	3.0875 (.2115)	3.1206 (.2107)
Sibship Size	−1.2130 (.1863)	−1.0008 (.1422)
Birth Order	.4302 (.2442)	
Mother's Working Status	1.3750 (.0473)	1.2847 (.5163)
Adjusted R²	.0880	.0873

was done while controlling for all other variables in the final model. Only the partial correlation coefficients for sibship size for the two models are significantly different ($p < .05$). This suggests that size provides a significantly better fit in prediction of IQ in the below-poverty group than in the above-poverty group.

The following results appear to be the most interesting and important: first, the marked deviation of the only child from the linear pattern appears in the below-poverty group but not in the above-; second, the absence of a significant effect of birth order in either of the final models; third, the consistent effect of income in both the terms of its gradient and the fit it provides in the below- and above-poverty groups; fourth, the inconsistent effect of sibship sizes in terms of the fit it provides in below- as compared to the above-poverty group, but not in terms of its gradient; fifth, the smaller contribution of the confluence variables to the amount of variance explained among above-poverty status subjects as compared with those in the below-poverty status; finally, the negative influence of a mother's working in the above- but not in the below-poverty groups is noteworthy.

Table 5. Correlational Analysis of Below-Poverty Status Subjects

	IQ with S	IQ with B	IQ with I	IQ with M
Zero-Order	−.23**	−.09*	.24**	−.03
Controlling for:				
S	—	.10*	.22**	−.01
B	−.23**	—	.25**	−.03
I	−.21**	−.10*	—	−.05
M	−.23**	−.09*	.25**	—
S,B	—	—	.21**	−.00
S,I	—	.06	—	−.04
S,M	—	.10*	.23**	—
B,I	−.19*	—	—	−.06
B,M	−.23**	—	.25**	—
I,M	−.20**	−.11**	—	—
S,B,I	—	—	—	−.03
S,B,M	—	—	.21**	—
B,I,M	−.18**	—	—	—
S,I,M	—	.06	—	—

* Significant at the .05 level.
** Significant at the .01 level.

Table 6. Correlational Analysis of Above-Poverty Status Subjects

	IQ with S	IQ with B	IQ with I	IQ with M
Zero-Order	−.13**	−.05**	.27**	.03*
Controlling for:				
S	—	.05**	.26**	.05**
B	−.13**	—	.27**	.03*
I	−.12**	−.06**	—	.03
M	−.14**	−.05**	.27**	—
S,B	—	—	.26**	.05**
S,I	—	.03	—	.05**
S,M	—	.05**	.26**	—
B,I	−.11**	—	—	.03
B,M	−.14**	—	.27**	—
I,M	−.13**	−.06**	—	—
S,B,I	—	—	—	.05**
S,B,M	—	—	.26**	—
B,I,M	−.12**	—	—	—
S,I,M	—	.03*	—	—

* Significant at the .05 level.
** Significant at the .01 level.

CONCLUSIONS

These data partially verify and partially fail to confirm aspects of Zajonc and Markus's (1975) confluence model. They also suggest paths for future research. Under the confluence model, sibship size bears directly on intelligence irrespective of other factors. Since these effects occur in spite of socioeconomic location and in spite of the elimination of other confounding factors, sibship size seems to have an important independent inverse impact on IQ; however, the absence of a birth-order effect weakens the confluence model's validity.

Whether or not size effects occur because of the dilution of the intellectual milieu or because of dilution of factors such as economic resources, for example, remains to be seen. But inasmuch as income is the most powerful predictor, the role that other environmental forces play on a child's test performance stand out. The greater importance of this indicator of social class supports Page and Grandon's (1979) admixture explanation of intellectual differences.

Since family size effects are less pronounced in the above-poverty model, an interaction effect might be posited for future research. For the low income group each additional child drains money from an already squeezed budget, whereas the costs of additional children in high income families may be more easily absorbed. For these reasons, further analysis should include a breakdown of the higher income groups into subgroups to determine if size effects lessen even more with ascension up the income scale.

Some other major components of the confluence model are questioned by our findings. Insofar as the sample analyzed contained relatively closely spaced children (at least equivalent to Zajonc's [1976] definition of closeness), birth-order effects would be expected. They did not occur. If anything, their influence, though insignificant, was the reverse of that expected. But these birth-order patterns do not refute the spacing argument. Rather they suggest that close spacing produces detrimental outcomes for children, irrespective of birth order. Family density may, in part, account for the negative sibship size impact, since as sibship size increases, birth intervals between children invariably decline. What insignificant birth-order effects suggest is that the advantages that other scholars ascribe to an early birth position in the sibship hierarchy are to a great extent exaggerated. They also erode the long held belief by psychologists that individual attention during early and extremely sensitive phases of growth has a critical, irreversible impact on child development.

However, another vivid possibility exists to account for the absence of a birth-order effect in this study. In Zajonc's review (1976) in which he compared samples where birth-order effects did and did not occur, he originally attributed the difference to spacing practices at the aggregate level. Admittedly, birthrates only roughly assess spacing patterns. There is, yet, another striking difference in the samples summarized—the age of the test-taker. In this study and others where birth-order effects did not occur (with the exception of Kunz and Peterson [1977]), the test-takers ranged in age from 6 to 14 (Zajonc et al., 1979). In studies where effects occurred, even if modest, tests were taken by older teenagers. So there could be developmental differences with birth order exerting a "sleeper effect" on intelligence—not appearing until later in life with shifts in cognitive developmental stages.

In addition, the nonteacher handicap explanation of the only-child exception is inapplicable to these data. Since it only occurs in the below-poverty group and since the discontinuity there is so striking, there may be something about being an only child in a low income family that counteracts the advantages of small sibship size. For example, reasons for having one child may vary by socioeconomic position. Also, the only child in a high income family could experience both the advantages of unshared resources plus compensatory activities provided by parent's money such as kindergarten where interaction with peers is available. By contrast, the only child in a low income family lacks contact with siblings and may, because his parents cannot afford it, also lack alternative activities where exposure to other peers is available.

If the tutoring target factor were correct, then last-born children would score lower than older ones because they did not have a younger sibling to teach. Often in this work, the reverse occurs with youngest children having the highest mean IQ within their sibship size group, thus giving support to the idea that what makes only children in low income families different is their isolation from other children rather than their nonteacher handicap.

In regard to the differential treatment explanation, the measure used in this analysis is so crude that only speculations are made to account for its impact. Assuming that mothers who work have less time to spend with children, then, this factor should influence IQ across class conditions. It does not, appearing only in the above-poverty group. This finding may suggest that time spent with the child is not as important as the quality of the interaction between mother and child. The higher income mother is apt to possess knowledge and skills that make her a better coach than her lower income counterpart. Keeping company with her may stimulate a child's intellectual development. Until better measures of the quality of interaction are taken into account, though, this suggestion is mere speculation.

Certainly, this research only scratches the surface of a very complex problem. It appears that sibship size, because children must share resources, intellectual or other, among themselves, is important. This sharing may also impinge on the quality of interaction.

REFERENCES

Adams, Bert M.
1972 "Birth order: a critical review." Sociometry 35:411–39.
Altus, W. D.
1966 "Birth order and its sequelae." Science 151:44–9.
Anastasi, Anne
1956 "Intelligence and family size." Psychological Bulletin 53:187–209.
Bayer, Alan E.
1966 "Birth order and college attendance." Journal of Marriage and the Family 28:480–4.
1967 "Birth order and attainment of the doctorate: a test of the economic hypothesis." American Journal of Sociology 72:540–50.
Belmont, Lillian and Francis A. Marolla
1973 "Birth order, family size, and intelligence." Science 182:1096–101.
Blalock, Hubert M.
1972 Social Statistics. New York: McGraw-Hill.
Breland, Hunter
1974 "Birth order, family configuration, and verbal achievement." Child Development 45:1011–19.
Bulmer, M. G.
1970 The Biology of Twinning Man. Oxford: Clarendon.
Cicirelli, Victor G.
1976 "Sibling structure and intellectual ability." Developmental Psychology 4:369–70.
Davis, D., S. Cahan and J. Bashi
1977 "Birth order and intellectual development: the confluence model in light of cross-cultural evidence." Science 196:1470–2.
Dixon, Wilfrid J. and Frank J. Massey
1969 Introduction to Statistical Analysis. New York: McGraw-Hill.
Falbo, Toni
1978 "Sibling tutoring and other explanations for intelligence discontinuities of only and last borns.' Journal of Population 1:349–63.
Glass, David C., J. Neulinger, and O. G. Brim
1974 "Birth order, verbal intelligence, and educational aspiration." Child Development 45:807–11.
Grotevant, Harold D., Sandra Scarr and Richard Weinberg
1977 "Intellectual development in family constellations with adopted and natural children: a test of the Zajonc and Markus model " Child Development 48:1699–703.
Kagan, Jerome
1977 "The child in the family." Daedalus 106:33–56.
Kunz, Phillip R. and Evan T. Peterson
1977 "Family size, birth order, and academic achievement." Social Biology 24:144–9.
Marjoribanks, Kevin, Herbert J. Walberg and Mark Borgen
1975 "Mental abilities: sibling constellation and social class correlates." British Journal of Social and Clinical Psychology 14:109–16.
National Center for Health Statistics
1967 "Plan, operation and response results of a program of children's examination." Vital and Health Statistics, Programs and Collection Procedures, Series 1, 8, DHEW, 80.
1971a "Sample design and estimation procedures for a National Health Examination survey of children." Vital and Health Statistics, Data Evaluation and Methods Research, Series 2, 44, DHEW, 22.
1971b "Intellectual development of children as measured by the Wechsler Intelligence Scale for children." Vital and Health Statistics, Series 11, 107, DHEW, 41.
1972a "Quality control in a National Health Examination survey." Vital and Health

Statistics, Data Evaluation and Methods Research, Series 2, 44, DHEW, 22.

1972b "Subtest estimates of the WISC full scale IQ's for children." Vital and Health Statistics, Data Evaluation Methods and Research, Series 2, 47, DHEW, 22.

Nisbet, J. D. and N. J. Entwistle
1967 "Intelligence and family size." British Journal of Educational Psychology 37:188–93.

Page, Ellis B. and Gary M. Grandon
1979 "Family configuration and mental ability: two theories contrasted with U.S. data," American Educational Research Journal 16:257–72.

Rankin, Richard J., A. J. H. Gaite and Thomas Heiry
1979 "Cultural modification of effect of family size on intelligence." Psychological Reports 45:391–7.

Record, R. G., T. McKeown, and J. H. Edwards
1970 "An investigation of the difference in measured intelligence between twins and single births." Annals of Human Genetics 34:11–20.

Sampson, Edward E.
1965 "The study of ordinal position: antecedents and outcomes." Pp. 175–228 in Brendon A. Maher (ed.), Progress in Experimental Personality Research. New York: Academic Press.

Schacter, Stanley
1963 "Birth order, eminence, and higher education." American Sociological Review 28:757–68.

Schooler, C.
1972 "Birth order effects: not here, not now!" Psychological Bulletin 78:161–79.

Stein, Z., M. Susser, G. Saenger and F. Marolla
1972 "Nutrition and mental performance." Science 178:708–13.

U.S. Department of Health, Education, and Welfare
1969 Vital Statistics of the United States—Natality, Vol. 1.

Velandia, Wilson, Gary Grandon and Ellis B. Page
1978 "Family size, birth order and intelligence in a large South American sample." American Educational Research Journal 15:399–416.

Zajonc, R. B.
1976 "Family configuration and intelligence." Science 192:227–36.

Zajonc, R. B. and Gregory B. Markus
1975 "Birth order and intellectual development." Psychological Review 82:74–88.

Zajonc, R. B., Hazel Markus and Gregory B. Markus
1979 "The birth order puzzle." Journal of Personality and Social Psychology 37:1325–41.

RACE AND SEX DIFFERENCES IN CAREER DYNAMICS*

RACHEL A. ROSENFELD
McGill University; and the National Opinion Research Center, University of Chicago

American Sociological Review 1980, Vol. 45 (August):583–609

In this paper, career differences by race and sex are analyzed. Careers are defined as trajectories of socioeconomic status and wages and are described by a linear differential equation model. It is assumed that the different groups defined by race and sex tend to be in different labor markets and economic sectors and to face different opportunity structures even within labor market divisions. This assumption guides predictions for and interpretation of results with respect to various aspects of career inequality: initial status and wage level; potential status and wage levels; effects of human capital, family background, and family of procreation variables on initial and potential wage and status levels; speed of advancement. Pooling of cross-sections and time-series techniques are used to estimate the model, with data from the National Longitudinal Surveys of the Labor Market Experience of Young Men and Women.

In the last decade, sociologists have made extensive use of the status attainment approach, which focuses on the effects of individual-level characteristics on achievement, to monitor racial and sexual inequality. (See, for example, the papers in Hauser and Featherman, 1977.) Even more recently, sociologists have begun to investigate the extent to which differences in labor market positions result in the observed inequality by sex and race (e.g., Bibb and Form, 1977; Beck et al., 1978a; 1978b). Spilerman has suggested (1977:551) "the notion of the career [job history] as a strategic link between structural features of the labor market and the socioeconomic attainments of individuals."

With few exceptions (e.g., Sørensen and Tuma, 1978), however, the empirical research on socioeconomic inequality by race and sex, with or without reference to labor market structures, has been done using models and methods developed for the analysis of achievement in cross-section, rather than for the analysis of careers. The purpose of this paper is to contribute to the small body of empirical research on career dynamics. Following Sørensen (1975; 1977), careers are defined as levels of status and wages individuals attain over their work lives and are modeled using a simple linear differential equation.[1]

In the next section, the literature on labor markets is used to develop hypotheses that are to be tested in this paper about sex and race differences in careers. Following this, the model of career trajectories is explained. The career model is then estimated by pooling of cross-sections and time-series methods, using data from the National Longitudinal Surveys of Labor Market Experience of Young Men and Young Women. The results of this estimation are discussed, and conclusions drawn, in the final sections.

SEX, RACE, LABOR MARKETS, AND CAREERS: HYPOTHESES

Careers take place over time. They have beginnings, from which people, on

---

*Direct all communications to: Rachel A. Rosenfeld; National Opinion Research Center; University of Chicago; 6030 South Ellis; Chicago, IL 60637.

An earlier version of this paper was presented at the Annual Meeting of the American Sociological Association, Boston, Mass., August, 1979. This research was supported in part by Canada Council grant 410-77-0530 and by National Institute for Mental Health grant 5-T32 MH 15163-02. I am grateful to Edward Withers, Carol Welch, and F. Yvonne McDonald for their assistance with the data analysis and to François Nielsen, Dennis Hogan, and anonymous reviewers for their helpful comments.

[1] This definition of careers is similar to that used by Form and Miller (1949) and Lipset and Bendix (1952) and is more general than definitions of careers as a succession of related jobs in a hierarchy of prestige through which one moves in an ordered, predictable sequence (Wilensky, 1960) or as jobs within a particular firm (Rosenbaum, 1979; Halaby, 1979). See Spilerman (1977) for an extended discussion of the term "career."

the average, move up.[2] Between the time of their entry into the work force and of their retirement or withdrawal from it, people's income, status, and other rewards change. This change takes place with a certain speed, which may vary across groups with different characteristics and in different types of labor markets. People with given characteristics in given labor markets will, as they live their work lives, approach some highest possible career level—or potential—which they may or may not actually reach, depending on the speed of their mobility.

This process is perhaps most easily grasped in the context of a career within a given firm. The firm has a job hierarchy. An individual entering the firm has credentials enabling him/her to qualify for some position or positions within the firm that, without a change in credentials, will be the highest to which he/she can aspire. To reach such a position usually requires time in the firm, as well as formal credentials. Some people formally qualified for a given position will actually reach that position, others will not. Those with different types of qualifications may have the same potential for a position but reach it with different speeds. For example, those with higher education may be able to go almost directly to their highest possible position while those without the credentials require some time to work their way up. (See Rosenbaum, 1979, for an analysis of such mobility within a firm.) Also, those equally qualified formally—but having different ascribed characteristics—may have access to different career lines and may take more, or less, time to advance. For example, Blau (1977) hypothesizes that, even within internal labor markets, women will have different kinds of career ladders open to them, and that, when those career-ladder positions are also held by men, women will advance more slowly up them than will men.

Although it is easy to visualize the career process by thinking of mobility within a firm, and, although some people do have such careers, average career profiles show the general pattern (of upward mobility approaching some potential) to emerge without the mobility being all within one firm (Sørensen, 1975; 1977). The hypotheses and model developed here refer to the average careers for men and women, by race, regardless of whether these take place totally within particular firms. The model developed in the next section enables us to look directly at (1) initial achievement, (2) factors affecting initial achievement, (3) potential achievement, (4) factors affecting potential achievement, and (5) the speed at which the potential is approached. Hypotheses about race and sex differences along these dimensions are also developed in this section.

Although this paper does not directly use the concept of dual labor markets or segmented economy to investigate differences in opportunity by race and sex, it takes the labor market and segmented economy literature as suggestive of the sorts of differences in opportunity by race and sex that might be behind differences in careers. Various typologies of labor markets and the economy have been offered: primary and secondary markets (Doeringer and Piore, 1971); male and female markets (Oppenheimer, 1970); monopoly, competitive, and state sectors of the economy (see development by Hodson, 1978); core and periphery sectors (Bluestone, 1970, used by Beck et al., 1978a; 1978b); open and closed employment relationships (Sørensen and Tuma, 1978). (See Kalleberg and Sørensen, 1979, for a review of the labor market literature.) Women and blacks have been found to be overrepresented in the secondary labor markets and in the competitive or periphery sector—that is, in those labor market positions that offer generally lower rewards and less chance for advancement (Hodson, 1978; Bibb and Form, 1977; Beck et al., 1978a; 1978b; Boyd and Humphreys, 1979). Further, within sectors and labor markets, there is additional sorting by sex and race (Hodson, 1978; Beck et al., 1978a), with the process of this sorting resembling the general sorting into labor markets (Blau, 1977; Dussault and Rose-Lizée, 1979). Thus, even within markets and sectors, women and blacks are often barred from high-reward, high-opportunity jobs.

---

[2] As Goldner (1965) has shown, not all career mobility is upward. In some managerial situations, downward mobility after a certain age is even considered the norm, as older people lose out in competition with younger. This can be treated as a change in the underlying career model, with a change from a growth to a decline model, or as a change in potential, the result of a change in the individual's characteristics. This will be discussed further in the context of the specific model to be estimated in this paper.

No one typology of labor markets or economic sectors will be able to capture the extent to which there are overall differences in opportunities by sex and race, given the sorting that also goes on *within* industries, occupations, and firms as well as between labor market divisions. This argues for continuing attention to studies of firms, as well as for continuing refinement of typologies of the ways in which labor markets are segmented. These activities are, however, beyond the scope of this paper. Here, instead, differences in careers by sex and race are predicted by using what has been theorized and discovered about the workings of opportunity structures by sex and race among and within markets.[3] I will argue that the estimated parameters of the career model can be used as social indicators of the general position of sex and race groups, summarizing the effects of between- and within-market sorting.[4]

Initial and Potential Levels of Job Rewards

As careers are viewed here, they have beginnings, at entry to the labor market, the levels of which are determined by the labor market in which persons are moving and their qualifications and background. They also have potential levels, which might or might not be reached, likewise determined by the labor market structure and the characteristics of the individuals. Most sex and race comparisons have been done cross-sectionally. Even when beginning job is treated separately, the average current job reward for any given group averages over those near the beginning of their careers, those approaching their potential, and those somewhere in between. When this is done, the differences among average job rewards for the different groups will depend on the career-stage composition of the groups as well as on the levels of rewards they receive at comparable career stages. This paper looks separately at the levels of job rewards at the beginning of the career, six years later (the last year in which observations were actually made, as will be discussed in the next section), and at the potential. Discussions in the labor market literature and existing over-time comparisons suggest several hypotheses about initial and potential wage and status differences, by race and sex.

(1) All women and nonwhite men will have lower average initial and potential wages. Given the wage structure evidenced in core versus periphery or primary versus secondary markets, and the overrepresentation of women and minority members in just those markets offering lower rewards and less opportunity to advance, plus the nature of sorting by sex and race within markets, I expect all women and nonwhite men to start their careers at a disadvantage (as Gordon, 1972:115 suggests), and to have lower potential wage levels than white men.

(2) The gap in average wages between white men and other groups will increase over the work life. Hoffman (1978) discusses the increase in wage differences between black and white males over the life cycle. Nonwhite men (and women generally) might be denied access to jobs

---

[3] Four groups are being compared here: white men, white women, nonwhite men, and nonwhite women. Most of the nonwhites are black. Much of the prediction will involve a contrast between two groupings of these four categories—for example, what is expected for white men on the one hand, and for nonwhite men, white women, and nonwhite women on the other. Although some work has been done that examines race and sex simultaneously, it is still difficult to find the basis for ordering all four groups with respect to each dimension of careers. This ordering is the end product of the empirical analysis in this paper.

[4] One would expect that the type of socioeconomic reward used to define the job level over time would affect the comparisons made of careers. Socioeconomic status changes only with occupation (and such change is a necessary—but not a sufficient—condition for change in status). Income, in contrast, can change without a change in occupation, as typically measured by three-digit census codes. Admittedly, both measures are rough. The use of socioeconomic status and income together, however, may be suggestive of the ways in which careers are structured, in general, for different groups.

that motivate and provide training and advancement and, thus, the chance for higher wages; and so they fall farther and farther behind white men. It is to be expected, then, that the wage differences between white men and other race/sex groups would be greater at their potentials than initially, and that the distance between starting and ending (or potential) wages would be greater for white men than for other groups.

(3) The average status of nonwhite men will be lower initially and at their potential than status of white men, although the average status of white women will be about the same as that of white men at their potential and perhaps even slightly higher at career entry. Given the sorting into occupations, by race, that occurs within and between markets and sectors, I would expect the status of jobs held at any career stage by nonwhite men to be, on the average, lower than that of jobs held by white men. For women, especially white women, this sorting into occupations on the basis of sex has been found, in cross-section, to lead to jobs with average status about the same as that for men, as women are concentrated in lower white-collar jobs which have fairly high status (although low wages). (See Oppenheimer, 1970; Treiman and Terrell, 1975; see also Nilson, 1976, and Bose, 1973, for discussion of the determination of prestige by sex.) Although nonwhite women have been found more often in service jobs—with both low status and low pay—than in clerical jobs, recently there have been shifts of nonwhite women into the traditionally female positions (Jusenius, 1975; Allen, 1979). For young nonwhite women, average status profiles are expected to be somewhere between white women's and nonwhite men's, because they will represent a mix of lower white-collar, service, and blue-collar jobs.

(4) The status distance to be covered, on average, by white men will be greater than that covered by women or nonwhite men. Women and nonwhite men tend to be in those jobs with restricted opportunity to move up. Workers in such jobs are not expected to remain with the job, are not given much on-the-job training, and perhaps do interrupt their employment; thus, they remain at roughly the same occupational level. White men are supposed to be especially likely to be operating in internal labor markets, where one enters a firm at a particular entry port and continues up the career ladder within the firm, protected to a large extent from competition with outsiders (Doeringer and Piore, 1971). I would expect such men to have a greater potential distance to cover than those without such career ladders. Further, Blau (1977) has found that even when women are in internal markets the career ladders in which they predominate tend to be flatter than those for men. Generalizing to all groups sorted into disadvantageous labor market positions, women and nonwhite men are expected to have less actual and potential status mobility than white men. Empirical evidence on the status distance between first and current jobs, by sex and race, is consistent with this hypothesis (see Duncan et al., 1972; Rosenfeld, 1978; Wolf, 1976; Sewell et al., 1977).

Return to Resources

Of course, differences in status and income levels could be due to differences in the characteristics of different groups rather than to segregation in a particular type of labor market and sector. Indeed, those in secondary labor markets and in the competitive sector have been characterized by (and found to have) lower educational and skill levels (Beck et al., 1978b; Hodson, 1978; Doeringer and Piore, 1971; Gordon, 1972; Boyd and Humphreys, 1979). One needs to know, then, about the *returns* to various work-related resources across groups. If returns are the same across groups, then differences in level of achievement would be the result simply of differences in levels of resources (although the chances or motivation to get additional training, education, and so forth, could also be influenced by labor market location).

Also of interest is whether supposedly nonwork-related characteristics are evaluated differently for different groups. The overrepresentation of women and nonwhite men in the less desirable labor market positions has been explained, in part, by the idea of "statistical discrimination," whereby the core or primary market employers reduce their risk of hiring unstable or untrainable employees by using easily visible characteristics—such as race and sex—as screening devices. Any one individual with these traits will not necessarily be a bad risk, but, on the average at least, some employers believe that these group members will be less desirable employees (Phelps, 1972; Thurow, 1975). Spence (1974) has shown how this sort of screening can pay off for the employer even when the trait used to screen is *not* related to productivity. The analysis here is conducted separately, by race and sex groups, to capture such effects of screening on these traits. There may, however, be other easily observed traits, such as marital status, that are used to further channel people into more or less desirable positions. This screening can appear as differences in the effects of these traits for those assumed to be in different positions within the labor market if the screening works differently in different labor markets.

With respect to returns to resources and other traits, I suggest the following hypotheses.

(1) The wage returns to education, training, and other resources will be lower for women and nonwhite men than for white men. The returns to resources are expected to increase over the work life for white men, but not for other groups. The training and skill of workers are important in the more desirable labor market positions. People may be selected into the primary market and competitive sector on the basis of their credentials. Higher returns to education, then, would show up as a result of this selection. Thurow and Lucas (1972) suggest that education inversely represents to the employer the firm's cost in training a worker rather than the ability of a worker to perform a certain job. If this is the case, then the returns to education should increase for those who are in careers where training occurs and promotion is possible—that is, for white men especially. Further, Beck et al. (1978a) have suggested that, for minority group members in the better parts of the labor market, credentials are evaluated differentially, as the employer's protection against making a mistake in hiring (because it is more difficult and costly to fire a mistake in these sectors than in the secondary or periphery areas). Women and nonwhite men would have to be overqualified to get the same positions as white men and, thus, have lower returns to their resources. This might be especially true for blacks, the quality of whose education might be doubtful in the mind of the employers. Beck et al. (1978a) show that minority workers (nonwhite men and both nonwhite and white women) received lower earnings in relation to their education and experience than did white men, especially within the core sector. Bibb and Form (1977) also found differences between blue-collar women and men in wage returns to their education and tenure, even after roughly controlling for sector.

(2) The differences between white men and women, generally, in returns to resources will be less when career position is measured by status than when it is measured by wages. Gordon (1972:117) has said:

> One would expect more evident patterns of returns to education among women, both because stronger discriminatory patterns against women within the labor market help reduce the bargaining strength of women (and permit employers to reward the more educated without fearing for greater mobility), and because many women work in a variety of white-collar clerical jobs for which some general educational skills are useful.

This statement might be more relevant with respect to status than with respect to wages, however. Treiman and Terrell (1975), Featherman and Hauser (1976), and Sewell et al. (1977) provide evidence of greater similarity between women and men in status returns than in wage returns to education and other resources. Some increase over the career in status returns to resources is still expected for white men but not for white or nonwhite women, given the restrictions on women's opportunity.

(3) Marital status and number of children will be used as additional screening traits for men but not for women. Being married and having children will increase

status and wages for men, but will have no effect for women. Although, in principle, family of procreation characteristics are not related to productivity, they may be used as indicators of stability by employers. If this is true, there should be effects of marital status and number of children for men but not for women in the analysis of this paper, which uses data from samples of workers continuously employed. Although some have argued that because—for women—being married and having children constrain both employment and career advancement, there is evidence that these variables are primarily proxies for employment history, with effects on career gains that tend to disappear when extent of employment is explicitly included in a model of status mobility (Rosenfeld, 1978; Boyd, 1977). Controlling for employment, marital status, and number of children would not be of much use to an employer in screening women from jobs with long career ladders. If a woman *is* married, she may be seen as lacking commitment to the labor force; if she is not married (or is married and does not have children), she may be seen as unstable because she *will get* married or *will have* children.

For men, in contrast, having a family may represent both the incentive to go after higher income and status and the signal to an employer that a man is going to be a good employee. This is probably true especially for young white men, just beginning their careers, who potentially have access to jobs with longer career ladders and who cannot use their past employment records as evidence of their stability. The drop in youth unemployment with marriage for young men is one basis for suspecting that marital status for men is, indeed, related to employment patterns. The results in Duncan et al. (1972, chap. 8) are consistent with the hypothesis.

Tests of hypotheses about the levels of status and wages, by race and sex, and about returns to various resources, have been carried out in research from both the status-attainment and the labor-market/economic-sector perspectives. The career model adds to this research primarily by focusing attention on both initial and potential levels of attainment. In cross-section, one cannot be sure at what stage careers are being observed. The attainments of various groups and the effects of independent variables on these attainments will depend on the stage of the process for the different groups. Careers are dynamic—they occur over time—and this dynamism affects even their "static" component.

Speed of Achievement

Perhaps even more important than correct estimation of these cross-sectional aspects of careers is the ability to estimate, directly, the dynamic aspect with this model. That is, one can estimate the speed with which the distance between initial and potential attainments is covered.

Assuming that white men are overrepresented in labor market positions that offer the greatest job mobility opportunities, I hypothesize that:

White men will reach their potential earning more slowly than other groups, but they may reach their potential status more quickly. White men would be expected to reach their maximum earnings more slowly than other groups because of the greater number of rungs on their career ladders (Sørensen, 1977). Essentially, their peak earnings would occur later in their work lives. Women and nonwhite men would be expected to reach their maximum earning ceilings more quickly. White men are thus expected to have greater opportunity for earnings *mobility* over their work life, while other groups are expected to have greater opportunity to *reach* their potential. Oppenheimer's (1974) discussion of career profile differences is consistent with this hypothesis. Looking at age differences in earnings of white men within occupational categories defined by level of peak earnings, she reports that, although young men (18 to 24) tend to be similar in their levels of income regardless of occupational category, their patterns of earnings, by age, differ considerably by occupation—with two general patterns emerging: Earnings for men in most blue-collar and middle- and low-level white-collar occupations peak early, before age 45; but high-level professional, managerial, and sales occupations seem to peak later, after age 45. The late-peaking occupations are those in which white men are most likely to be represented, and from which both women and nonwhite men are likely to be excluded.

For socioeconomic status, the predictions about relative ordering are reversed. Status is a relatively rough measure of level of job. Males entering internal labor markets might be expected to obtain, rather quickly, the highest occupational category they will obtain. Women and black men, who are restricted in the career ladders to which they can gain access, might have some possibility of upward mobility in status, but only after "proving" themselves by extended occupancy in a stereotypic occupation. In this case, they would slowly reach their potential status. If, however, there were no possibility at all of moving up from a typically black or female job (which most often would be in the secondary labor market and/or competitive sector), then it could be that women and blacks would reach their maximum statuses even more quickly than white men.

In summary, I expect to find that: women generally and nonwhite men have lower average initial and potential levels of income and at least nonwhite women and nonwhite men have lower average initial and potential status levels than white men; on average, the status and wage distance to be covered by nonwhite men and by all women will be shorter than that to be covered by white men; education and other resources should give fewer returns in the form of initial and potential levels of income and status to all women and to nonwhite men than to white men, while men (perhaps, particularly, white men) will benefit from having their own families, especially early in their careers; and the speed at which the maximum earnings level is attained will be slower for white men than for other groups, while the relative speed with which maximum status is reached will possibly be slower for women generally and for nonwhite men.

The Career Model

Careers have been described in the previous section as the process whereby people move up in status or in wages, over their work lives, to approach some potentially highest position. A fairly simple model of this sort of process is a partial adjustment model (see, for example, Doreian and Hummon, 1974; 1976):

$$dY/dt = -b\,(Y^* - Y). \qquad (1)$$

Here, Y is either status or income achieved, and Y^* is the potential level of status or income. The rate of change in Y (dY/dt) is proportional (by $-b$, which is positive because, here, $b<0$, as explained below) to the gap that exists between the achieved level of Y and the potential level. If the potential is reached, then change in status or income will cease.[5]

The criterion Y^* could be set in any of a number of ways. Here, it is assumed that the criterion will be a linear function of the individual's characteristics—both job-relevant and others—with the weights associated with the characteristics varying according to the labor market position of the group to which the person belongs:

$$Y^* = a_0 + a_1X_1 + \ldots + a_KX_K \qquad (2)$$

where there are K individual characteristics. This definition of the potential is consistent with the work that has been done to date from both the status attainment and labor market perspectives.

---

[5] I wish to thank an anonymous reviewer for pointing out that the explanation of the model and of the results would be simpler if I explicitly used the partial adjustment form. An earlier version of this paper presented the model as the "negative feedback" model, which Coleman (1968) describes and Sørensen (1975; 1977) uses. The "negative feedback" model specifies the rate of change in Y as a linear function of the level of Y itself and of the values of exogenous variables. The two models are mathematically the same when the criterion Y^* is completely determined by the exogenous variables (ignoring for the moment the problems of misspecification and measurement error). See Rosenfeld and Nielsen (1978), Rosenfeld (1979), and Hannan and Tuma (1979) for a discussion of the two approaches.

Substituting the right side of Equation 2 for Y* in Equation 1, and multiplying Equation 1 through, gives a simple linear differential equation, which is easily solved (Coleman, 1968) to yield an equation defining the career trajectory—that is, the levels of Y over time:

$$Y_t = e^{bt}Y_0 + a_0\,(1-e^{bt}) + a_1\,(1-e^{bt})\,X_1 + \ldots + a_K\,(1-e^{bt})\,X_K\,, \qquad (3)$$

where Y_0 is initial income or status.

The potential Y* as defined here need not be some fixed level. It can change in response to changes in individual characteristics. In socioeconomic achievement terms, this would mean that people could continue to improve their potential position by increasing job-relevant or other resources or could suffer a decrease in ultimate achievement because of depreciation of a resource or an increase in a liability. If, for the sake of simplicity, one assumes that the change in some individual characteristics is linear and occurs independently of changes in job rewards, then allowing for such change is straightforward (see Coleman, 1968, for the general form). If, say, X_1 were to change linearly and independently of Y, then the solution to Equation 1, when the criterion Y* is defined as in Equation 2, would be:

$$\begin{aligned} Y_t &= e^{bt}Y_0 + a_0\,(1-e^{bt}) + a_1\,(1-e^{bt})X_1 \\ &\quad + a_1 \left\{\frac{1-e^{bt}}{bt} - 1\right\} \Delta X_1 + \ldots \\ &\quad + a_K\,(1-e^{bt})\,X_K \qquad (4) \end{aligned}$$

Including the change term does not change the relationship among the other parameters of the model, but adds extra information about the model and its fit (Coleman, 1968:441–3).

As it is stated here, the model has face validity in that, when b is less than 0 and entry level is less than potential level, it predicts a career trajectory that is like that observed, at least for men: one in which there is rapid increase in reward levels early in the career and then a levelling-off (Ornstein and Rossi, 1970; Sørensen, 1975; Rosenfeld, 1978).[6] The speed with which the distance between some entry level and the potential level is covered is indicated by b: the larger b is in absolute value, the more quickly the gap between achieved and potential level is reduced. One could say that groups with a higher absolute value of b have a greater opportunity to reach their potential, while groups with lower absolute values of b have greater opportunities to be mobile (see Rosenfeld and Nielsen, 1978; Rosenfeld, 1979).

To complete the model, one can add an explicit formulation of the way in which individual characteristics determine Y_0:

$$Y_0 = c_0 + c_1\,X_1 \ldots + c_K\,X_K. \qquad (5)$$

The individual characteristics determining the initial reward levels need not be the same as those determining the potential.

The assumption that changes in individual characteristics occur independently of changes in reward level is made for simplicity, and is probably not a realistic assumption. Ideally, one would simultaneously model changes in employment, in rewards received for employment, and in resources and family circumstances. Some attempts have been made in this direction (Doreian and Hummon, 1974; Tachibanaki, 1979), but there is still need for more thought about the conceptual and methodological problems involved in such models. The model presented in this paper is, thus, a first approximation of a dynamic model of careers, valuable in demonstrating the need for attention to the time component of careers and the further steps such attention should take. (See Blalock, 1969; Coleman, 1968; and Hannan and Tuma, 1979, for discussion of multiequation models.) In this paper, after a description of the data and estimation techniques, the model represented by Equations 1 and 5 will be estimated and the various components of the careers will be compared by sex and race.

---

[6] The condition $b < 0$ is necessary because, if b were greater than 0, the trajectory would move from this maximum level at an accelerating rate, which does not make sense substantively. As will be seen later, the income and status trajectories of the sample studied here are like those predicted by Equation 3 or 4 with b less than 0 and initial position less than the maximum.

DATA AND METHOD

The data with which to test the hypotheses about sex and race differences in career dynamics are from the National Longitudinal Surveys of Labor Market Experience (NLS) of Young Women (1968–1975) and of Young Men (1966–1973). The samples were chosen to represent the civilian noninstitutionalized population of the United States aged 14 to 24 at the time of the first survey for each group. Each sample initially included approximately 5,000 individuals, of whom approximately 1,500 were nonwhite. Detailed employment and educational histories were collected over the years of the surveys (which are still in progress). Such longitudinal data are essential if dynamic models are to be estimated. (See the National Longitudinal Surveys Handbook, CHRR, 1979, for further details.)

From each sample, only those who were still employed at the time of every survey date after the first (1967, 1968, 1969, 1970, 1971, and 1973 for the young men; 1969, 1970, 1971, 1972, 1973, and 1975 for the young women) were included in the analysis so that the greatest number of observations could be used in pooling of cross-sections and time-series to estimate the parameters of the model.

This selection of cases implies potential bias. The young women selected are likely to have a greater commitment to the labor market than other young women of comparable age. They are, therefore, more likely to be the ones who have "careers" in the sense of longer career ladders. (See Rosenfeld, 1977, for a comparison of women with and without employment breaks, using data on an older NLS cohort.) To the extent that this is true, differences between the women and men will be attenuated. Although this selection procedure is the result of the conceptual difficulties in measuring the socioeconomic attainments of women not in the labor force, it might be seen as a response to the argument that the reason women have flatter career profiles than men is that they spend less time in, and are less committed to, the workplace.[7] If there are still sex differences now, it will probably not be because of lesser employment experience for the women.

The selection affects the young men in that those in the military during this period (which covers the height of the Vietnam war) are excluded. During the Vietnam era, those with higher educational attainments were less likely to serve, as were those with lower ability and poor health. Black high school graduates, who might be seen as those blacks most likely to be successful in the labor market, were disproportionately likely to serve during the Vietnam period, but, relative to whites, blacks reporting health limitations were also more likely to serve (see Shields, 1977). To some extent, then, whites with better career chances and blacks with poorer career chances might be included. However, given the nature of occupational deferments, the white men in the sample might have made career choices different from those of a cohort entering the labor market during a period of relatively low exposure to the draft. For example, more college-educated young men might have entered teaching than would have, ordinarily. Given sex differences in careers, however, these men might still have an advantage over women in their chances for promotion. Further, counterbalancing the selection of nonwhite men with worse career chances is the selection of those with continuous employment in the face of very high unemployment rates for young black men relative to other groups (Winship and Rosenfeld, 1979).

Comparing the 1970 characteristics of members of the NLS who were employed all years with the 1970 Census description of people having the same sex, race, and age ranges shows the sort of bias in the subsample: members of the subsample of the NLS providing data for the analysis of this paper are more likely than the general population to have gone to college, given that they have at least a high school diploma; the men in the sample are more likely—and the women (especially the nonwhite women), less likely—to be married than the comparable United States population; while both men and women in the subsample are less likely to be doing service work, the women are also somewhat more likely than the general female population to be professionals, clericals, or operatives, and though the men are less likely to be clericals they are more likely to be managers (if they are white) or farm workers (if they are nonwhite) (U.S. Bureau of the Census, 1970).

---

[7] However, Fligstein and Wolf's (1978) results suggest that the *effects* of resources on attainment are not severely biased by selection of currently employed women.

Careers will be indexed by two variables: by socioeconomic status (SEI, see Duncan, 1961) and by hourly earnings on main job (in 1972 cents, with wages adjusted using personal consumption expenditures implicit price deflators in the Economic Report of the President, 1977). Three different sorts of individual-level characteristics will be used to predict the trajectories of SEI and wage: human capital variables, family of origin status, and family of procreation characteristics.

The first set of variables includes human capital variables, which influence an individual's productivity and/or the employer's perception of productivity. As already mentioned, the effects of such variables are expected to be greatest for those with jobs offering the greatest opportunities for advancement—that is, especially for white men. Education is one of the most important of these variables. Highest grade completed by the beginning of a two-year period (e.g., by 1967, 1969, and 1971, for the young men) is one measure of this. Change in this level over each two-year interval is also included. Formal training outside regular school is another way in which people can increase their human capital. Measures of whether training related to white-collar work (1 for professional, technical, clerical, sales, or general course outside regular school, 0 otherwise) or to blue-collar work (1 for skilled or other manual training, 0 otherwise) had been taken by the beginning of a two-year interval, and of whether or not there was additional training of either sort during the two years, were also included.[8] In addition to these measures of formal schooling and training, whether or not the individual reported that a health problem limited her/his extent or type of work (1 for some limitation, 0 for none) was included as a third measure of human capital. Health was assumed to remain relatively constant over an interval and was measured in about the middle of each two-year interval.

The second set of variables includes measures of socioeconomic background. Such factors have been a central concern of status attainment research, which endeavors to test the extent to which characteristics of one's family of origin, as compared with achievements of the individual, affect later achievements. Thinking in terms of intergenerational transmission of access to labor markets, one can imagine that a well-placed father could help a child get on a good career ladder by having knowledge of—and connections with—a firm, trade, or profession. If so, the positive effects of "good" family background would be expected to be especially strong for white men. Generally, though, effects of family background have been found to be largely mediated by education, which is included in the career model (Blau and Duncan, 1967; Duncan et al., 1972; Kelley, 1973; Sewell and Hauser, 1975; Sewell et al., (1977). The measures of family background included in the analysis to be presented here are SEI of father (or head of household) when the respondent was 14, mother's education (in years), and number of siblings reported in 1966 (men) or 1968 (women). All family background variables have values constant over time.[9]

The third set of variables is that of family of procreation characteristics. These include marital status and number of children (for women) or dependents (for men). (Marital status is coded 1 if the individual is currently married, 0 otherwise.) Marital status is assumed to have an effect which is constant over a two-year interval. Number of dependents (for men) and number of children (for women) are measured at the beginning of each interval. Change in the number of dependents or children over the two years is also measured. Responsibilities within the home are measured differently for men and women (dependents versus children) to capture differences in the ways such responsibilities may act on, or be imagined to act on, workers of different sexes.

---

[8] For the initial survey year (1966 for men, 1968 for women), all types of training taken outside regular school up to that date were reported. In each year after the first, however, respondents were asked about type of training taken *longest* in the time since the last survey. The rough measure formed from these questions may thus underestimate somewhat the proportion of persons with a given type of training.

[9] In more complete models of effects of family socioeconomic status, other variables, such as father's education and rural origin, are also included. These three variables were chosen to represent the range of effects family background has while minimizing problems of multicollinearity and missing data.

Means and standard deviations for these variables by sex and race are shown in Table 1.

As mentioned above, longitudinal data are necessary for any study of careers. One problem with the use of longitudinal data, however, is finding a methodology by which to take advantage of the amount of information available in them. To estimate the parameters needed to compare the speed of career development, the potential levels, and the effects of individual characteristics on this potential, one can estimate

$$Y_t = a_0^* + b^*Y_{t-1} + a_1^*X_1 \ldots + a_k^*X_k + e \quad (6)$$

(where some of the independent variables could be change variables) and transform these estimates to obtain the parameters in equations (1) and (2), for example, $\ln b^* = b$, $a_1^*/(1-b^*) = a_1$.[10] One could choose any two observation times within which to estimate these parameters, adjusting the estimates for the length of the interval. This, however, seems to waste information on the other periods for which one has income and status observations. Another solution is to pool the pairs of observations, so that the data from which the estimates are made consist of more than one set of observations for each individual. Using a two-year lag, there are three sets of years for each individual.

One can imagine estimating Equation 6 using ordinary least squares regression (OLS). However, lagged dependent variables often mean serious violations of the assumptions of OLS. If the model is misspecified (as is usually the case with predictions of individual socioeconomic achievement) and the excluded variables are relatively stable or systematically related over time, the error terms will include the effect of these excluded variables and will be correlated over time (autocorrelated)—that is, $E(e_te_t-1) \neq 0$. Further, the error term will be correlated with a regressor, since the lagged dependent variable at one time is the dependent variable another time. The OLS estimators will be both inefficient and inconsistent. The bias is especially great in the OLS estimator of the coefficient for the lagged dependent variable. Such bias can affect all the conclusions from the analysis, since b^* is not only of interest in its own right but also enters into the calculations of all the coefficients of the career model.

To reduce the problems of bias, one models the structure of the errors and then uses an estimating technique that takes into account the modeled error structure. The error term can be hypothesized to be as follows:

$$e = \mu_i + \tau_t + v_{it} \quad (7)$$

where the components are independently distributed with zero means and constant variances. The component for constant individual factors not included explicitly in the general model is μ_i; τ_t is the component for time effects; and v_{it} is the usual well-behaved random error term. Substantively, μ_i would represent unmeasured traits affecting achievement, such as intelligence, motivation, and energy, which differ across individuals but are relatively constant over time. τ_t represents unobserved variables that vary over time and in roughly the same way for all indi-

---

[10] As one anonymous reviewer correctly pointed out, given the interdependence of estimators—and, thus, estimates of speed of mobility (b), level of potential (Y*), and effect of traits on this potential (a)—it is not really possible to test independent hypotheses about all three dimensions of careers. I have done so here because of the conceptual independence of the components and because of the need to demonstrate how to interpret findings with respect to each.

Table 1. Descriptive Statistics for Career Profiles by Race and Sex

	Whites		Nonwhites	
Variables	Mean	(Standard Deviation)	Mean	(Standard Deviation)
A. *Young men employed 1967–1973:*	(N = 707)		(N = 160)	
Age, 1966	20.252	(2.989)	20.088	(2.953)
Mother's education	10.380	(2.839)	7.594	(3.320)
Father's (or household head's) SEI when R was 14	33.298	(23.830)	29.950	(23.311)
Number of siblings	2.761	(2.131)	5.200	(3.104)
Highest grade completed, 1967	12.132	(2.218)	10.531	(2.678)
Change in highest grade, 1967–69	.390	(.702)	.156	(.483)
Highest grade completed, 1969	12.522	(2.281)	10.688	(2.761)
Change in highest grade, 1969–71	.181	(.514)	.088	(.362)
Highest grade completed, 1971	12.703	(2.408)	10.775	(2.835)
Change in highest grade, 1971–73	.303	(.638)	.350	(.711)
White-collar training as of 1967[a]	.209	(.407)	.088	(.284)
Blue-collar training as of 1967	.137	(.344)	.088	(.284)
Additional white-collar training 1967–69	.256	(.437)	.144	(.352)
Additional blue-collar training, 1967–69	.134	(.341)	.113	(.317)
White-collar training as of 1969	.376	(.485)	.213	(.410)
Blue-collar training as of 1969	.231	(.422)	.163	(.370)
Additional white-collar training 1969–71	.283	(.451)	.150	(.358)
Additional blue-collar training, 1969–71	.123	(.329)	.094	(.292)
White-collar training as of 1971	.485	(.500)	.288	(.454)
Blue-collar training as of 1971	.287	(.453)	.219	(.415)
Additional white-collar training 1971–73	.257	(.438)	.150	(.358)
Additional blue-collar training 1971–73	.058	(.234)	.050	(.219)
White-collar training as of 1973	.557	(.497)	.350	(.478)
Blue-collar training as of 1973	.303	(.460)	.244	(.431)
Health limitation, 1968[b]	.092	(.289)	.056	(.231)
Health limitation, 1970	.076	(.266)	.056	(.231)
Health limitation, 1971	.061	(.239)	.063	(.243)
Marital status, 1967[c]	.536	(.499)	.431	(.497)
Marital status, 1969	.683	(.466)	.594	(.493)
Marital status, 1971	.777	(.417)	.688	(.465)
Marital status, 1973	.823	(.382)	.719	(.451)
Number of dependents, 1967	.552	(.867)	.744	(1.172)
Change in number of dependents, 1967–69	.372	(.622)	.556	(.916)
Number of dependents, 1969	.924	(1.055)	1.300	(1.350)
Change in number of dependents, 1969–71	.290	(.673)	.288	(.934)
Number of dependents, 1971	1.214	(1.188)	1.588	(1.438)
Change in number of dependents, 1971–73	.235	(.756)	.275	(.931)
SEI67	37.082	(22.900)	20.706	(16.946)
SEI68	39.085	(23.270)*	22.716	(19.024)**
SEI69	39.922	(23.703)	25.269	(24.462)
SEI70	41.761	(23.809)*	25.936	(20.981)**
SEI71	42.745	(24.251)	26.456	(21.699)
SEI73	44.197	(24.296)	27.513	(20.615)
Hourly wages 67[d]	309.646	(130.382)	237.323	(104.079)
Hourly wages 68	350.983	(130.365)*	261.206	(94.308)**
Hourly wages 69	383.706	(143.084)	280.417	(106.137)
Hourly wages 70	413.183	(187.841)*	294.291	(117.433)**
Hourly wages 71	427.973	(174.190)	299.987	(128.898)
Hourly wages 73	487.452	(225.578)	331.671	(170.496)
* Based on N = 699.				
** Based on N = 155.				
B. *Young women employed 1969–75:*	(N = 304)		(N = 86)	
Age	19.799	(2.816)	20.698	(2.657)
Mother's education	10.921	(2.646)	8.919	(3.073)
Father (or household head's) SEI when R was 14	35.299	(23.518)	19.756	(16.684)
Number of siblings	3.000	(2.217)	4.756	(2.886)

Table 1. (Continued)

Variables	Whites Mean	Whites (Standard Deviation)	Nonwhites Mean	Nonwhites (Standard Deviation)
Highest grade completed, 1969	12.309	(1.837)	12.279	(1.643)
Change in highest grade, 1969–71	.378	(.712)	.221	(.562)
Highest grade completed, 1971	12.688	(1.803)	12.500	(1.679)
Change in highest grade, 1971–73	.158	(.482)	.093	(.395)
Highest grade completed, 1973	12.845	(1.886)	12.593	(1.778)
Change in highest grade, 1973–75	.227	(.537)	.279	(.607)
White-collar training as of 1969[a]	.319	(.467)	.337	(.467)
Blue-collar training as of 1969	.026	(.160)	.023	(.152)
Additional white-collar training, 1969–71	.270	(.445)	.233	(.425)
Additional blue-collar training, 1969–71	.040	(.195)	.081	(.275)
White-collar training as of 1971	.467	(.500)	.488	(.503)
Blue-collar training as of 1971	.059	(.236)	.105	(.308)
Additional white-collar training, 1971–73	.336	(.473)	.256	(.439)
Additional blue-collar training, 1971–73	.030	(.170)	.000	(.000)
White-collar training as of 1973	.599	(.491)	.547	(.501)
Blue-collar training as of 1973	.082	(.275)	.105	(.308)
Additional white-collar training, 1973–75	.237	(.426)	.209	(.409)
Additional blue-collar training, 1973–75	.010	(.099)	.000	(.000)
White-collar training as of 1975	.655	(.476)	.605	(.492)
Blue-collar training as of 1975	.089	(.285)	.105	(.308)
Health limitation, 1968[b]	.049	(.217)	.012	(.108)
Health limitation, 1971	.066	(.248)	.023	(.152)
Health limitation, 1973	.056	(.230)	.047	(.212)
Marital status, 1969[c]	.322	(.468)	.267	(.445)
Marital status, 1971	.457	(.499)	.395	(.492)
Marital status, 1973	.572	(.496)	.430	(.498)
Marital status, 1975	.674	(.469)	.454	(.501)
Number of children, 1969	.250	(.600)	.651	(.955)
Change in number of children, 1969–71	.089	(.401)	.174	(.439)
Number of children, 1971	.339	(.690)	.826	(1.098)
Change in number of children, 1971–73	.115	(.320)	.151	(.392)
Number of children, 1973	.454	(.756)	.977	(1.095)
Change in number of children, 1973–75	.188	(.502)	.140	(.489)
SEI69	44.211	(18.481)	36.454	(21.922)
SEI70	46.296	(18.773)*	36.541	(19.718)**
SEI71	47.980	(18.263)	38.279	(21.695)
SEI72	48.272	(17.864)*	39.977	(20.942)**
SEI73	49.355	(18.286)	38.616	(20.967)
SEI75	49.993	(19.506)	40.942	(23.347)
Hourly wages 69[d]	232.066	(10.706)	211.339	(81.705)
Hourly wages 70	256.573	(92.630)*	244.769	(86.625)*
Hourly wages 71	278.986	(114.227)	262.759	(97.623)
Hourly wages 72	296.619	(100.995)*	276.165	(111.541)**
Hourly wages 73	311.147	(110.464)	284.955	(100.685)
Hourly wages 75	315.990	(111.904)	296.415	(111.054)

* Based on N = 294.
** Based on N = 85.

SOURCE: National Longitudinal Surveys of Labor Market Experience of Young Men (1966–1973) and Women (1968–1975).

[a] Training. White-collar training = 1 if professional, technical, clerical, sales training or general courses not leading to a degree have been taken by the date specified, 0 otherwise (see note 4 for further explanation). Additional white-collar training = 1 if any such training had been taken in a given two-year interval beyond that reported by the beginning of the interval, 0 otherwise. Blue-collar training = 1 if any skilled or other manual training has been taken by the specified date, 0 otherwise. Additional blue-collar training = 1 if such training has been taken over a two-year interval in addition to that reported at the beginning of the interval, 0 otherwise.

[b] Health limitation = 1 if respondent reported health as limiting extent or kind of work, 0 otherwise (see note 5 for further explanation).

[c] Marital status = 1 if presently married, 0 otherwise.

[d] Hourly wages: in constant 1972 cents.

viduals, such as the effects of the economy on socioeconomic achievement. With more than one observation per individual over more than one time, there is the possibility of estimating the various components of variance and controlling for them in estimating the other coefficients of the model.

An approach to estimation that takes the error structure into account is that of modified generalized least square (MGLS). This class of estimators is consistent and asymptotically efficient.[11] The small sample properties of GLS and MGLS have been examined by Hannan and Young (1977) (see also Hannan and Tuma, 1979). The particular MGLS estimator used in this paper is the one developed by Fuller and Battese (1974), which uses the fitting-of-constants method to estimate the covariance matrix (Henderson, 1952; Searle, 1971). This method is appropriate for this situation, in which some of the exogenous variables do not change over time. This algorithm has been included as part of the SAS computing package (Drummond and Gallant, 1977).[12]

RESULTS

Observed Career Profiles

Figures 1 and 2 show the observed career profiles (in solid lines) for white and nonwhite women and men. The data from which these profiles were drawn are in Table 1. The first time-point (1967 for young men, 1969 for young women) is the time of first job for many of these people and close to it for others, and will be discussed as approximately Y_0. Starting wages in 1972 cents vary as predicted: white men have the highest hourly wages, followed by nonwhite men, white women, and nonwhite women. The ordering of wages is first by sex and then, within sex, by race. The relative positions with respect to income remain the same six years later. Further, over the period observed, white men gain more in wages than nonwhite men, who in turn gain more than white women, who gain more than nonwhite women. The career profiles in Figure 1 show, however, that women and nonwhite men have very similar career profiles, while white men belong to a distinctly different pattern. Differences between white men and the other groups increase over the time observed. At the same time, the careers of men by race are parallel, and those of women by race are parallel.[13]

The story changes with respect to status. Here status is determined first by race and then, within race, by sex—but with women having the advantage. White women have the highest status profiles, nonwhite men the lowest; and white men and nonwhite women fall in between. Over the period observed, though, white men gain more in status than any other group, and nonwhite women gain the least (4.5 points), in line with what was expected.

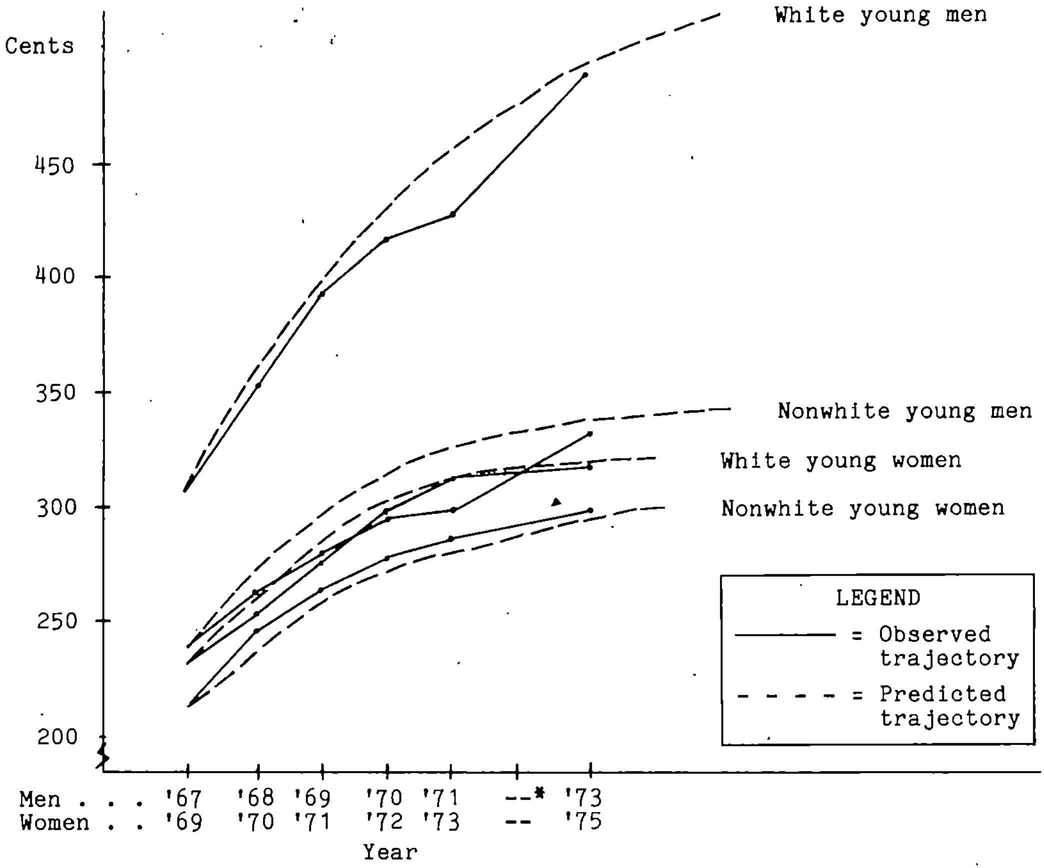
The differences in wage and status curves cannot be explained completely by sex and race differences in the levels of resources and constraints. Nonwhite men do have: a noticeably lower level of education and training (especially white-collar training); mothers with less education and fathers with lower status jobs; more siblings and dependents; and somewhat less stability as indicated by marriage. If all these variables affect achievement—and if these effects do not vary by race—we would expect lower achievement for nonwhite men on the basis of their personal characteristics. The case is not so clear for women, however. The women, here, are approximately as well educated as the men. They have higher rates of white-collar training, although less blue-collar training. White women, on the average, come from socioeconomic backgrounds

---

[11] Unfortunately, unlike maximum likelihood estimators, the parameter estimates obtained by MGLS do not necessarily retain their desirable properties under nonmonotonic transformations, such as those by which one calculates the parameters of the career mode.

[12] I wish to thank Andrew Kolsted for bringing this program to my attention.

[13] The abrupt rise between 1971 and 1973 for young men may reflect measurement error. The 1973 wage for young men was not calculated by CHRR, but had to be constructed by me for this analysis, using the reported CHRR procedures. Thus, this measure may be less clean than theirs.



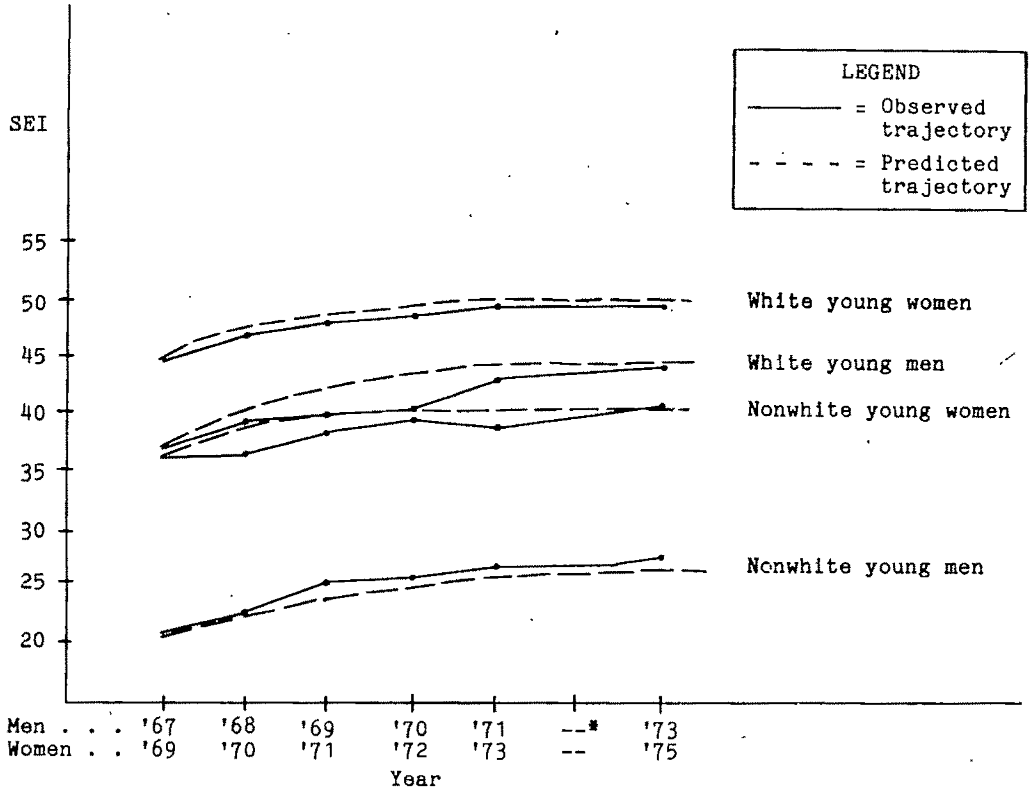
*No observation made this year.

Figure 1. Career Profiles by Sex and Race: Hourly Wages (in 1972 Cents) over Time for Those Employed 1967–1973 (Young Men) or 1969–1975 (Young Women)

similar to those of white men, while nonwhite women, like nonwhite men, come from families with lower levels of parental education and status and larger number of siblings (which, perhaps, makes their educational comparability with whites even more remarkable). The women, here, who have been employed fairly consistently over the six years observed, have lower rates of marriage than the men and have fewer children than do men, on the average, have dependents. This is true despite the tendency of women, in general, to marry at lower ages than men. They may, indeed, have chosen to commit themselves to work rather than to raising families, at least for a while. And, if anything, these women are healthier than the men, although only a very small proportion of any group reports health limitations.

Effects of Individual Characteristics: Initial Job

Returns to various resources, and constraints in early wages and status, are shown in Table 2. For all groups, education is important for early attainment. White men and women get approximately the same wage and status returns to their education at this stage of their work lives. Nonwhite women also receive returns comparable to those of white men—and much higher than those of nonwhite men. The location of these white and nonwhite women in lower white-collar jobs and of white men in positions where education, perhaps, is used to screen applicants, would seem to contrast with the position of nonwhite men who may be in exactly those positions for which education credentials are less important, as speculated



*No observation made this year.

Figure 2. Career Profiles by Sex and Race: SEI over Time for Those Employed 1967–1973 (Young Men) or 1969–1975 (Young Women)

before. Formal training is also of importance for the rewards received in early jobs. White-collar training increases the status of men and, to a lesser extent, of white women (some of whom have taken clerical courses within regular school). Blue-collar training also increases wages, significantly for all groups except nonwhite women. The effect is greatest for nonwhite men, for whom formal white- or blue-collar training perhaps is seen as a more reliable credential than education received in public schools of perceived dubious quality.

The returns to resources can be summarized by calculating returns to education and training using each group's coefficients and white men's average resources. If all groups had the average characteristics of white men, white men would receive the highest status returns to their resources, with white women and nonwhite women close behind. White men, with average white male education and training, would receive 65 status points on the basis of those resources. On the basis of their coefficients in Table 2 and the white men's means, white women would receive 61 points; nonwhite women, 57. Nonwhite men would receive the lowest returns to all their resources: only 35 status points.

With respect to wages, nonwhite men with average white male education and training are, still, the group with the lowest return to their resources: \$1.31/hour on the basis of education and training alone. White women actually receive the greatest return to their human capital at this career stage: \$3.02, compared with \$2.72 for nonwhite women and \$2.47 for white men.

Health limitations, which might be viewed as negative human capital, have no effects for men, but significant negative effects for white women. (See Corcoran

Table 2. Prediction of "First" (1967 for Young Men, 1969 for Young Women) SEI and Hourly Wage by Sex and Race[a]

	A. Young Men Employed 1967–1973							
	SEI 1967				Hourly wage 1967			
	Whites		Nonwhites		Whites		Nonwhites	
Dependent Variable Independent Variable	Metric coefficient	s.e.	Metric coefficient	s.e.	Metric coefficient	s.e.	Metric coefficient	s.e.
Mother's education	−.031	.265	.650	.388	.320	1.601	4.539	2.477
Father's SEI	.003	.029	.044	.049	−.022	.174	.752*	.310
Siblings	−.187	.344	−.381	.372	−1.550	2.079	−3.607	2.377
Highest grade, 1967	5.200**	.504	2.712**	.498	19.481**	2.147	8.870**	3.183
White-collar training	10.172**	1.758	8.762*	4.112	21.068*	10.617	90.965**	26.288
Blue-collar training	−2.024	2.047	4.265	3.887	48.119**	12.367	31.587	24.850
Health limitations, 1966	−1.676	1.996	4.794	4.272	−4.594	12.054	−22.003	27.311
Marital status, 1967	6.251**	1.729	7.252*	2.925	81.941**	10.446	67.248**	18.699
Number of dependents, 1967	−1.440	.988	−1.752	1.236	8.472	5.968	−3.556	7.902
Constant	−29.457		−15.463		16.027		70.244	
R^2	.377		.387		.298		.336	
N	707		160		707		160	

	B. Young Women Employed 1969–1975							
	SEI 1969				Hourly wage 1969			
	Whites		Nonwhites		Whites		Nonwhites	
Dependent Variable Independent Variable	Metric coefficient	s.e.	Metric coefficient	s.e.	Metric coefficient	s.e.	Metric coefficient	s.e.
Mother's education	−.348	.307	.349	.715	−2.171	2.467	−.749	2.972
Father's SEI	.074	.042	.346*	.122	.071	.270	.281	.508
Siblings	−.428	.426	−.089	.782	2.914	2.718	−.834	3.254
Highest grade, 1969	4.940**	.536	4.937**	1.484	23.849**	3.422	21.562**	6.173
White-collar training	4.481*	1.994	−.302	4.230	45.017**	12.731	30.385	17.591
Blue-collar training	3.081	5.644	−21.210	13.463	22.261	36.026	27.638	55.994
Health limitations, 1968	−9.535*	4.170	46.326*	18.769	−30.705	26.619	−68.915	78.061
Marital status, 1969	−.828	2.315	2.864	5.333	8.529	14.775	10.551	22.180
Number of children, 1969	−1.758	1.794	−4.270	2.594	23.370*	11.452	4.791	10.789
Constant	−12.269		−31.627		−71.166		−64.359	
R^2	.317		.389		.224		.239	
N	304		86		304		86	

[a] These are ordinary least squares estimates, since there is no lagged dependent variable.

* $.01 \leqslant p \leqslant .05$.

** $p < .01$.

and Duncan, 1979, for a more detailed comparison of the human capital returns to men and women.) Nonwhite women have an unexpectedly positive return to health limitations. Since only 1 of the 86 nonwhite women has any health limitations, this probably reflects an idiosyncratic case.

As expected, effects of family of origin are largely mediated by education (as demonstrated also by regressions, not shown), with some small effects for nonwhite women and men. Family of procreation variables have effects primarily for men. For them, being married has a positive effect on both status and wages. For women, there is essentially no effect on marital status. For men at this career stage, it is possible that marriage represents either actual or perceived stability, while for women, as speculated before, the stereotype may be such that they cannot win. However, white women do receive some small positive returns to number of children, perhaps because this variable represents perceived stability and motivation to earn, as was hypothesized for men.

At this point in the work life, soon after entry into the work force, women and white men do at least equally well in returns to their human capital resources, while nonwhite men get the low returns predicted for those in secondary labor markets and in the periphery sector. Marriage is effective in demonstrating stability (or being associated with other positive traits) only for men.

The analysis so far has not used the dynamic career model to look at speed of achievement or to go beyond the observed data to the potential level of achievement. It only forms the basis for comparison with this potential and effects on it. We can now turn to results from the dynamic career model.

Estimated Potential Achievement

Table 3 presents the estimates for the difference equation in Equation 6, with error structure such as in Equation 7. In all cases, the individual specific component of the variance is relatively large. Ignoring this would lead to biased coefficients and would have resulted in misleading inferences, with a confounding of the b, an indicator of opportunity structure, with the effects of individual-level characteristics, such as motivation to search for a "good" job. The time-specific component, however, is relatively small. From these estimates, we can calculate estimates of the parameters of the differential equation model. The results of doing this are shown in Table 4.[14]

We can now go beyond the observed data to comparison of potential reward levels. Potential is calculated for each group as

$$a_0 + a_1 (\overline{X}_1) + a_2 (\overline{X}_2) + \ldots a_K (\overline{X}_K) \quad (8)$$

using estimated parameters a_i in Table 4 (derived from estimates in Table 3) and the last observed means for each group (generally from 1973 for the men and from 1975 for the women).[15]

At their potential, white women would, still, be expected to have the highest average status (50.3, compared with 44.8 for white men, 26.7 for nonwhite men, and

---

[14] With information on change in some of the exogenous variables, there are two ways in which coefficients for those variables could be calculated. One is the way used here. The second way is to use the more complicated formula in Coleman (1968), which utilizes information from the change variables as well. If the assumption of linear change in the exogenous variable is reasonable and if there is not too much measurement error, the estimates calculated in these two ways should give approximately the same result. As might be expected, although education seemed to vary linearly according to the tests in Coleman (1968), other variables did not. Since there is no compelling reason to seek another solution to the differential equation—because the overall result of including the possibility of exogenous change in X_i "leaves the original relationships between observations and coefficients of the model undisturbed" (Coleman, 1968:443)—the simpler formulation is used in Table 4.

[15] These estimates, as explained earlier, will change as members of the different groups increase their resources and liabilities. The qualitative implications will probably not change too much over time, however, as changes in education and training—two very important factors which determine potential—should occur at a decreasing rate, according to predictions of human capital theory and to empirical results. Marital status and number of children (dependents) may continue to change over a longer period of time. However, these factors are not large contributors to the potential level.

Table 3. Models of SEI and Wage Change by Sex and Race: Modified Generalized Least Squares Estimates

Dependent Variable	SEI at t				Hourly wages at t			
	Whites		Nonwhites		Whites		Nonwhites	
Independent Variable	Metric coefficient	s.e.	Metric coefficient	s.e.	Metric coefficient	s.e.	Metric coefficient	s.e.
A. *Young men employed 1967–1973:*								
SEI or wage at t–2	.299**	(.020)	.395**	(.045)	.638**	(.024)	.463**	(.053)
Mother's education	.061	(.182)	−.200	(.305)	2.190	(1.429)	3.038	(2.030)
Father's SEI	−.006	(.020)	.007	(.038)	.030	(.155)	.629*	(.252)
Siblings	−.514*	(.236)	−.723*	(.285)	−.182	(1.846)	−.710	(1.876)
Highest grade at t–2	4.053**	(.250)	2.256**	(.421)	10.720**	(1.894)	7.584**	(2.740)
Change in highest grade t–2 to t	2.327**	(.554)	3.796**	(1.106)	9.628*	(4.832)	.121	(8.336)
White-collar training at t–2	2.427**	(.875)	1.330	(2.027)	10.979	(7.338)	52.244**	(14.452)
Blue-collar training at t–2	−2.206*	(1.012)	−1.999	(2.036)	30.587**	(8.435)	10.278	(14.316)
Additional white-collar training	4.008**	(.805)	5.329**	(1.967)	11.481	(6.873)	48.347**	(13.958)
Additional blue-collar training	−.128	(1.125)	.836	(2.307)	5.367	(9.660)	−2.090	(16.720)
Health limitations	−2.349	(1.301)	−4.922	(2.939)	−6.117	(11.124)	−17.641	(21.007)
Marital status at t–2	1.998*	(.934)	−1.189	(1.652)	10.261	(8.046)	11.538	(11.801)
Number of dependents t–2	.174	(.460)	.131	(.699)	.424	(3.862)	3.003	(4.884)
Change in number of dependents	.732	(.493)	.234	(.679)	−.763	(4.265)	.718	(4.950)
Constant	−22.764**	(3.384)	−2.953	(4.496)	14.390	(32.381)	30.333	(31.813)
Variance components:								
individual specific	85.738		62.047		4732.790		2253.860	
time specific	.096		0.		879.142		302.287	
random error	136.320		116.907		11433.134		7011.838	
Regression MSE	161.342		133.458		12760.666		7605.960	

Whites: 707 cross-sections, 3 time series.
Nonwhites: 160 cross-sections, 3 times series.

Table 3. (Continued)

Dependent Variable	SEI at t				Hourly wages at t			
	Whites		Nonwhites		Whites		Nonwhites	
Independent Variable	Metric coefficient	s.e.	Metric coefficient	s.e.	Metric coefficient	s.e.	Metric coefficient	s.e.
B. *Young Women employed 1969–1975:*								
SEI or wage at t–2	.295**	(.032)	.117*	(.053)	.387**	(.031)	.556**	(.061)
Mother's education	.298	(.285)	−.352	(.543)	−1.520	(1.696)	−1.384	(2.131)
Father's SEI	−.014	(.030)	.083	(.095)	.371*	(.180)	.370	(.373)
Siblings	−.729*	(.303)	−.495	(.599)	−1.528	(1.797)	−1.756	(2.363)
Highest grade at t–2	2.587**	(.401)	7.533**	(.983)	11.866**	(2.336)	12.101**	(4.288)
Change in highest grade t–2 to t	.678	(.806)	4.041*	(1.739)	1.034	(4.875)	11.121	(9.125)
White-collar training at t–2	1.186	(1.113)	.749	(2.327)	2.712	(6.781)	−1.935	(11.004)
Blue-collar training at t–2	−4.671*	(2.373)	1.011	(5.153)	5.225	(14.182)	4.366	(20.819)
Additional white-collar training	1.873	(1.006)	2.331	(2.070)	18.502**	(6.068)	25.349*	(10.722)
Additional blue-collar training	−6.362*	(2.738)	4.005	(6.129)	22.698	(16.463)	38.476	(28.135)
Health limitations	−1.176	(2.024)	2.754	(4.961)	−13.203	(12.103)	−21.189	(26.561)
Marital status at t–2	−.426	(1.145)	−2.458	(2.304)	2.219	(6.923)	.275	(11.373)
Number of children t–2	−1.190	(.948)	2.050	(1.482)	5.498	(5.691)	.272	(6.422)
Change in number of children	−.033	(1.035)	3.419	(1.848)	2.933	(6.239)	−6.474	(9.740)
Constant	1.849	(5.002)	−58.383	(13.651)	−44.663	(31.314)	−3.973	(58.567)
Variance components:								
individual specific	71.532		129.066		2460.448		1539.760	
time specific	.463		0		211.844		123.718	
random error	98.895		86.344		3566.962		2876.207	
Regression MSE	118.646		106.706		4326.184		3393.626	

Whites: 304 cross-sections, 3 time series.
Nonwhites: 86 cross-sections, 3 times series.
* .01 ≤ p < .05.
** p < .01.

Table 4. Estimated Parameters for Differential Equation Models[a]

Variable	SEI		Hourly wage	
	Whites	Nonwhites	Whites	Nonwhites
A. Young men employed 1967–1973				
SEI or wage at t–2 dynamic coefficient	−.604	−.464	−.225	−.385
Potential level coefficient for:				
Mother's education	.087	−.331	6.050	5.657
Father's SEI	−.009	.012	.083	1.171
Siblings	−.733	−1.195	−.503	−1.322
Highest grade completed	5.782	3.729	29.613	14.123
White-collar training	3.462	2.198	30.329	97.289
Blue-collar training	−3.147	−3.304	84.494	19.140
Health limitations	−3.351	−8.136	−16.898	−32.851
Marital status	2.850	−1.965	28.345	21.486
Number of dependents	.248	.217	1.171	5.592
Intercept	−32.474	−4.881	39.751	56.486
Potential predicted using means from 1971–1973	44.781	26.676	555.561	347.277
B. Young women employed 1969–1975				
SEI or wage at t–2 dynamic coefficient	−.610	−1.073	−.475	−.293
Potential level coefficient for:				
Mother's education	.423	−.399	−2.480	−3.117
Father's SEI	−.020	.094	.605	.833
Siblings	−1.034	−.561	−2.493	−3.955
Highest grade completed	3.670	8.531	19.357	27.255
White-collar training	1.682	.848	4.424	−4.358
Blue-collar training	−6.626	1.145	8.524	9.833
Health limitations	−1.668	3.119	−21.538	−47.723
Marital status	−.604	−2.784	3.620	.619
Number of children	−1.688	2.322	8.969	.613
Intercept	2.623	−66.119	72.860	−8.948
Potential predicted using means from 1973–1975	50.337	41.432	323.336	308.843

[a] Formulae used are: dynamic parameter = ln lag coefficient/Δt (where $\Delta t = 2$); potential coefficients = regression coefficient/(1 − lag coefficient). Lag and other regression coefficients are shown in Table 3.

41.4 for nonwhite women), having gained on the average a bit over 6 status points since their "first" jobs. This gain in status is in contrast to the lack of status gain found in data for women, generally (Rosenfeld, 1978; Wolf, 1976). This provides further evidence that to some extent women, in general, fail to gain in status over time because of discontinuities in their employment. When employed as are men, they do experience some gains. The white men have, however, gained slightly more than have the women (and the nonwhite men). Although their expected potential status is, still, lower than that of white women, it is now higher than that of nonwhite women. This probably reflects the greater heterogeneity in the jobs held by men over their work lives. As a group, they would show more mobility, as some white men get sorted into higher-level jobs over time in the work force. Nonwhite men, while experiencing a somewhat greater gain in status than nonwhite women, remain at the lowest status level.

The situation with respect to wages shows much greater contrast between white males and other groups than is true of status potential. At their potential level, white men would be expected to earn over $5.50 an hour, having experienced an increase in wages of over $2.50. Women and nonwhite men have gained, on the average, less than $1.00 in hourly earnings, receiving under $3.50 an hour in 1972 dollars. Nonwhite men continue to have average earnings somewhat higher than white women, who earn about $.15 more an hour than nonwhite women. This result suggests that lack of wage increase for women is *not* related to employment continuity.[16]

---

[16] Sandell and Shapiro, 1978, and Corcoran and Duncan, 1979, estimate that only 25% to 30% of the

Effects of Individual Characteristics: Potential Achievement

The coefficients (after the first line) in Table 4 represent estimates of the a_i, the effects of individual characteristics on potential achievement. At their potential, white men receive greater returns to their human capital than white women. They receive higher returns in terms of both status and wages to their years of formal schooling; get greater returns, in terms of status, to their white-collar training; suffer less of a status disadvantage from having some formal preparation for a blue-collar job; and receive considerably greater wage returns from both types of training but, especially, blue-collar training. The difference in returns to white-collar vs. blue-collar training is consistent with the idea of the rich plumber—that is, that many craftsmen and skilled manual laborers receive rather high pay for their valuable skills. While nonwhite men receive lower human capital returns than white men, they actually do better than white women, with the exception of somewhat lower wage returns to education. Nonwhite men, as compared with white men, continue to benefit in wages much more from white-collar training and less from blue-collar training and schooling, perhaps because of difficulty in having their educational certification (at least for secondary school) recognized as having any value, and because of difficulty in gaining access to jobs on which to use formal blue-collar training (which is perhaps the result of union control). Nonwhite women, the "double minority," receive higher status returns to their education than does any other group, and wage returns almost as high as those of white men. The returns they receive for formal training,.however, are as low as for white women. Poorly educated nonwhite women receive very low status, while well-educated nonwhite women (and, on the average, these women *do* have education beyond high school) have their credentials recognized. For them, education seems to be the key to high attainment. (The status returns on education for nonwhite women compared with other groups are consistent with Treiman and Terrell, 1975.)

Overall, if all groups had the 1973 resources of white men, white men and nonwhite women would receive the highest status returns (76.1 and 111.8) and white women and nonwhite men the lowest (46.7 and 48.7). With respect to wage returns, white men would gain the most ($4.28/hour) for having average education and training, followed by nonwhite women ($3.33), and then white women and nonwhite men ($2.57 and $2.44). In general, white men, who might be expected to have the greatest access to the better labor markets, receive the greatest returns from their educational and training credentials.

Family of origin effects are small but still evident at potential reward levels. If anything, they are somewhat stronger, here, when occupational differentiation is greater and sorting on the basis of family background might be more apparent, in contrast to the early career stage, when there is less differentiation. Having a large number of siblings, perhaps causing competition for attention and material support, negatively affects status and wage returns, significantly so in the status difference equation for both groups of men and white women. Having a higher-status father significantly, although very slightly, increases wages of white women and nonwhite men. Family of procreation variables, however, have almost no effect here, in contrast with the situation earlier in the career. Only in the status difference equation for white men does marital status reach significance.

This further confirms the idea that characteristics of family of procreation act primarily as screening signals for employers and that these are most important early in the careers of men, when on-the-job behavior is most in question.

In contrast with a time early in the work life, both white women and nonwhite men do worse than white men in the evaluations of their human capital. The change in the position of white women may be due to their more static occupational position. They remain in lower white-collar jobs that do not lead to positions giving greater

---

wage gap between white men and women can be accounted for by work history.

rewards to education and training credentials. As one can see from comparing Tables 2 and 4, the value of their credentials actually deteriorates slightly after they have entered the labor force. Nonwhite men, in contrast, do get increased returns to their education as compared with their early careers, as well as greater wage returns to their training, although the returns to their education remain relatively low. Nonwhite women, while not receiving returns to their formal training, get greater benefit from education at their potential than they did earlier in their careers, returns in some cases greater than those for white men. Of course, it must be kept in mind that the nonwhite women here are exceptions, in that they had jobs on each survey date. The small N for nonwhites, compared with those for whites, indicates the extent to which this is true. Beck et al. (1978a) found that nonwhite women received lower returns to human capital compared with white men, using an income indicator that took into account extent of unemployment. As emphasized before, this paper does not take race and sex as absolute indicators of labor market location. Some groups of women and nonwhite men will be more likely than others to be in the more desirable opportunity structures. Indeed, steady employment might be a sign of being in other than the secondary market. At the same time, one needs to keep in mind the values of the other components of the career model for nonwhite women. If nothing else, levels of wages for nonwhite women are below those of even nonwhite men.

With respect to the other types of variables included in the model, the findings are that family of origin has small effects that are, nonetheless, larger than those earlier in the career, perhaps because sorting into occupational levels was too far from complete for such effects to become apparent earlier. Family of procreation effects are smaller then earlier, perhaps because these variables are no longer needed to screen job applicants.

Speed of Achievement

Potential reward level, without changing resources and liabilities, is reached only as time reaches infinity—which is a longer time than anyone, to date, has spent in the labor force. The speed at which the potential is reached is, therefore, as important as the eventual level and is an indicator of the structure of opportunity. The first line for each sex in Table 4 gives estimates of the speed of mobility. Nonwhite women reach their status potential most quickly ($|b|$ of 1.073 is the greatest). White men and women attain their status potential at about the same speed ($|b| = .610$ and .604). Nonwhite men reach potential status most slowly, perhaps because of a possibility of advancement that is not always realized.[17] With respect to wages, white men, as predicted, reach their high potential most slowly, while white women reach their highest wages most quickly of all race/sex groups. For nonwhites, the ordering by sex is reversed.

The results concerning the speed with which the different groups reach their potentials in the two job rewards are consistent with what has been found by examining age differences in cross-section. In such examination, status potential is reached more quickly than wage potential. Occupation may stop changing while wages continue to change, even within the same occupation or general type of occupation. Otto and Haller (1979), although lacking good longitudinal data, show results suggesting that maximum status differentiation for men has been reached by age 35, while maximum income differentiation is not reached until about age 45.

In Figures 1 and 2, the estimated model is used to predict career trajectories, showing, in general, close fit to observed trajectories (the curves for nonwhite men's wages being an exception). The predicted values over time were estimated using

---

[17] Sørensen, 1979, argues that SEI is not a good metric with which to test such hypotheses because its distribution does not make sense as a distribution of socioeconomic reward. Transforming SEI in such a manner that he obtains an exponential distribution of attainment, he finds orderings, on b, of blacks and whites and of men and women that do not correspond exactly to the orderings obtained by using the usual SEI metric. I have not yet tried these models with his transformation, although I have such work in progress. One should keep in mind, however, that Sørensen used OLS, which means that his estimates are biased. As Table 3 shows, the extent of bias will differ across groups, making OLS comparisons suspect.

$$\hat{Y}_t = (e^{\widehat{bt}})\,\overline{Y}_0 + (1-e^{\widehat{bt}})\,\hat{Y}^*, \qquad (9)$$

where Y_0 = 1967 (for men) or 1969 (for women) status or wages, Y^* is the predicted potential shown in Table 4 and b is the "dynamic" coefficient in Table 4.

CONCLUSIONS

This paper analyzes career differences by race and sex, with careers defined as trajectories of socioeconomic status and income over the work life. It was assumed that the different groups tend to be in different labor markets and economic sectors and to experience different opportunities even within labor market segments, and this assumption guided predictions for (and interpretation of) results. The simple differential equation model describing careers allowed examination of various elements of inequality within the work force: entry level of status and wages; potential status and wage levels; effects of human capital, family of origin, and family of procreation variables on initial and potential reward levels; and the speed at which potential status and wages are approached. The dynamic nature of this model required longitudinal data and an estimation technique that could avoid the confounding of state dependence with unmeasured individual or time effects. The data from the National Longitudinal Survey of Labor Market Experience of Young Men and Women provided six years of work history for the analysis. These data were analyzed using a variance components model that controlled for unobserved variables constant over time for individuals and constant over individuals for each time. The results of this analysis showed the general advantage of white men in many aspects of careers, including wage levels and returns to human capital. Further, it showed the differences in effects of education and training, marital status, and parental socioeconomic status early in the career versus at the career stage when change has ceased to occur. The differences in speed with which equilibrium is reached for the two socioeconomic position variables and for the four groups give clues to the nature of the job structure through which men and women and blacks and whites move, over the course of their work lives.

The career model estimated here involves time, yet assumes some constancy. Basically, it assumes that once the trajectory begins it continues smoothly along as described by the equation. This seems not unreasonable. It may be that the economic conditions under which, and the particular position in which, one begins a career determines to a large extent where one goes from there. The small magnitude of the time-variance component is consistent with this speculation. With the longitudinal data currently and potentially available, however, one can look for period, cohort, and age effects. Using later panels from the group whose work histories were analyzed here, one could go on to see whether their careers in the next decade are as predicted by the model. Comparisons with the careers of young men and women beginning their work lives in the early 1980s (data on which will be gathered by a new National Longitudinal Survey of Labor Market Experience, and by High School and Beyond, 1980) might help answer the question of how the threat of the draft (as compared with the threat of a recession) affects career beginnings and trajectories. Comparisons of the effects of various economic shocks on different cohorts might suggest the nature of period effects vs. age effects. In general, then, the use of a simple career model such as the one estimated here enables one to use effectively the longitudinal data on labor market histories that have been and are being gathered to investigate change over and within lifetimes.

A model such as this provides an overview of careers: where a group came from, where it is going, and how fast it is getting there. As such, it is useful in giving us a variety of social indicators for the position of a given group. Understanding more fully what is behind the group differences, however, requires a closer examination of the nature of jobs held over time and of the types of job shifts occurring at the individual level. Further, to capture the extent of inequality in economic position, movement into and out of the work force must be analyzed. Although some of this work is in progress (see, for example, Winship and Rosenfeld, 1979), the interest in understanding the workings of labor markets and their effects on inequality has opened large areas of research problems that have not yet been addressed. This paper shows one—but only one—model and technique for analyzing the process of inequality.

REFERENCES

Allen, Walter R.
1979 "Family roles, occupational statuses, and achievement orientations among black women in the United States." Signs 4:670–856.

Beck, E. M., Patrick M. Horan, and Charles M. Tolbert II
1978a "Labor market discrimination against minorities: a dual economy approach." Revision of paper presented at the annual meetings of the American Sociological Association, San Francisco.
1978b "Stratification in a dual economy: a sectoral model of earnings determination." American Sociological Review 43:704–20.

Bibb, Robert, and William H. Form
1977 "The effects of industrial, occupational, and sex stratification on wages in blue-collar markets." Social Forces 55:974–96.

Blalock, Hubert M.
1969 Theory Construction: From Verbal to Mathematical Formulations. Englewood Cliffs: Prentice-Hall.

Blau, Francine
1977 Equal Pay in the Office. Lexington, Mass.: Lexington Books.

Blau, Peter M., and Otis Dudley Duncan
1967 The American Occupational Structure. New York: Wiley.

Bluestone, Barry
1970 "The tripartite economy: labor markets and the working poor." Poverty and Human Resources Abstracts 5:15–35.

Bose, Christine E.
1973 Jobs and Gender: Sex and Occupational Prestige. Baltimore: Center for Metropolitan Planning and Research, The Johns Hopkins University.

Boyd, Monica
1977 "Occupational attainment of native born Canadian women." University of Wisconsin-Madison: Center for Demography and Ecology Working Paper 77–26.

Boyd, Monica, and Elizabeth Humphreys
1979 "Sex differences in Canada: incomes and labour markets." Paper presented at the Conference on Canadian Incomes, Winnepeg, Manitoba, May 9–12, sponsored by the Economic Council of Canada.

Center for Human Resources Research
1979 The National Longitudinal Surveys Handbook. Columbus, Ohio.

Coleman, James S.
1968 "The mathematical study of change." Pp. 428–78 in H. M. Blalock (ed.), Methodology in Social Research. New York: McGraw-Hill.

Corcoran, Mary, and Greg J. Duncan
1979 "Work history, labor force attachment, and earnings differences between races and sexes." Journal of Human Resources 14:3–20.

Doreian, Patrick and Norman P. Hummon
1974 "Models of stratification processes." Quality and Quantity 8:327–45.
1976 Modeling Social Processes. New York: Elsevier.

Doeringer, Peter, and Michael Piore
1971 Internal Labor Markets and Manpower Analysis. Lexington, Mass.: D. C. Heath and Co.

Drummond, Douglas and A. Ronald Gallant
1977 "The TSCSREG procedure." Pp. 155–67 in Jane T. Helwig (ed.), SAS Supplemental Library User's Guide. Raleigh, N.C.: SAS Institute Inc.

Duncan, Otis Dudley
1961 "A socioeconomic index for all occupations." Pp. 109–38 in Albert J. Reiss, Jr., Occupations and Social Status. New York: Free Press of Glencoe.

Duncan, Otis Dudley, David L. Featherman, and Beverly Duncan
1972 Socioeconomic Background and Achievement. New York: Seminar Press.

Dussault, Ginette, and Ruth Rose-Lizée
1979 "Discrimination against women and labour market segregation: the case of office workers in Montreal." Paper presented at the Conference on Canadian Incomes, Winnipeg, Manitoba, May, sponsored by the Economic Council of Canada.

Economic Report of the President
1977 Washington, D.C.: U.S. Government Printing Office.

Featherman, David L., and Robert M. Hauser
1976 "Sexual inequalities and socioeconomic achievement in the U.S., 1962–73." American Sociological Review 41:462–83.

Fligstein, Neil, and Wendy Wolf
1978 "Sex similarities in occupational status attainment: are the results due to the restriction of the sample to employed women?" Social Science Research 7:197–212.

Form, William H., and Delbert C. Miller
1949 "Occupational career pattern as a sociological instrument." American Journal of Sociology 54:317–29.

Fuller, Wayne, and George Battese
1974 "Estimation of linear models with crossed error structure." Journal of Econometrics 2:67–78.

Goldner, Fred
1965 "Demotion in industrial management." American Sociological Review 30:714–24.

Gordon, David
1972 Theories of Poverty and Underemployment. Lexington, Mass.: Lexington Books, D. C. Heath and Co.

Halaby, Charles
1979 "Sexual inequality in the workplace: an employer specific analysis of pay differences." Social Science Research 8:79–104.

Hannan, Michael, and Nancy Tuma
1979 Methods of Dynamic Analysis. Unpublished manuscript, Stanford University.

Hannan, Michael T., and Alice Young
1977 "Estimation in panel models: results on pooling cross-sections and time series." Pp. 52–83 in David Heise (ed.), Sociological Methodology 1977. San Francisco: Jossey-Bass.

Hauser, Robert M., and David L. Featherman
1977 The Process of Stratification: Trends and Analyses. New York: Academic Press.

Henderson, Charles R., Sr.
1952 "Specific and general combining ability." Pp. 352–70 in John W. Gowen (ed.), Heterosis. Ames: Iowa State College Press.

Hodson, Randy
1978 "Labor in the monopoly, competitive, and state sectors of production." University of Wisconsin-Madison: Center for Demography and Ecology Working Paper 78–2.

Hoffman, Saul
1978 "Black-white earnings differences over the life cycle." Chap. 7, pp. 247–71 in Greg J. Duncan and James N. Morgan (eds.), Five Thousand American Families—Patterns of Economic Progress, Vol. VI. Ann Arbor, Mich.: Survey Research Center, Institute for Social Research, University of Michigan.

Jusenius, Carol
1975 "Occupational change, 1967–1971." Pp. 21–35 in Herbert Parnes, Carol Jusenius, and Richard Shortlidge, Jr., Dual Careers, Vol. 3. Manpower Research and Development Monograph 21. Washington, D.C.: U.S. Government Printing Office.

Kalleberg, Arne, and Aage B. Sørensen
1979 "The sociology of labor markets." Annual Review of Sociology 5:351–79.

Kelley, Jonathan
1973 "Causal chain models for the socioeconomic career." American Sociological Review 38:481–93.

Lipset, Seymour M. and Reinhard Bendix
1952 "Social mobility and occupational career patterns: I. stability of job holding." American Journal of Sociology 57:366–74.

Nilson, Linda
1976 "The occupational and sex related components of social standing." Sociology and Social Research 60:328–36.

Oppenheimer, Valerie
1970 The Female Labor Force in the United States: Demographic and Economic Factors Governing its Growth and Changing Composition. Population Monograph Series, No. 5. Berkeley: University of California.
1974 "The life-cycle squeeze: the interaction of men's occupational and family life cycles." Demography 11:227–45.

Ornstein, Michael and Peter H. Rossi
1970 "Going to work: an analysis of the determinants and consequences of entry into the labor force." Report No. 75. Baltimore: The Center for the Study of the Social Organization of Schools, Johns Hopkins University.

Otto, Luther and Archibald Haller
1979 "Evidence for a social psychological view of the status attainment process: four studies compared." Social Forces 57:887–914.

Phelps, Edmund
1972 "The statistical theory of racism and sexism." American Economic Review 62:659–61.

Rosenbaum, James
1979 "Organizational career mobility: promotion chances in a corporation during periods of growth and contraction." American Journal of Sociology 85:21–49.

Rosenfeld, Rachel A.
1977 "Breaks in women's employment and labor market re-entry." Paper presented at the annual meetings of the Canadian Sociology and Anthropology Association, Fredericton, New Brunswick.
1978 "Women's employment patterns and occupational achievements." Social Science Research 7:61–80.
1979 "Dynamic models of socioeconomic achievement." Unpublished manuscript, National Opinion Research Center, University of Chicago.

Rosenfeld, Rachel, and François Nielsen
1978 "Differential equations models of structural change." Paper presented at the 9th World Congress of Sociology, Uppsala, Sweden, August 14.

Sandell, Steven, and David Shapiro
1978 "The theory of human capital and the earnings of women: a re-examination of the evidence." Journal of Human Resources 13:103–17.

Searle, S. R.
1971 "Topics in variance component estimation." Biometrics 27:1–76.

Sewell, William, and Robert Hauser
1975 Education, Occupation and Earnings. New York: Academic Press.

Sewell, William, Robert Hauser, and Wendy C. Wolf
1977 "Occupational achievement to mid-life of male and female high school graduates." Paper presented at the annual meeting of the American Sociological Association, September 6, Chicago.

Shields, Patricia
1977 "The determinants of service in the armed forces during the Vietnam era." Columbus,

Ohio: Center for Human Resource Research, Ohio State University.

Sørensen, Aage B.
1975 "The structure of intragenerational mobility." American Sociological Review 40:456–71.
1977 "The structure of inequality and the process of attainment." American Sociological Review 42:965–78.
1979 "A model and a metric for the analysis of the intragenerational status attainment process." American Journal of Sociology 85:361–84.

Sørensen, Aage B., and Nancy B. Tuma
1978 "Labor market structure and job mobility." Paper presented at the 9th World Congress of Sociology, Uppsala, Sweden, August. Also University of Wisconsin-Madison: Institute for Research on Poverty Discussion Paper 505–78.

Spence, A. Michael
1974 Market Signaling: Information Transfer in Hiring and Related Screening Processes. Cambridge, Mass.: Harvard University Press.

Spilerman, Seymour
1977 "Careers, labor market structures and socioeconomic achievement." American Journal of Sociology 83:551–93.

Tachibanaki, Toshiak
1979 "Models for educational and occupational achievement over time." Sociology of Education 52:156–62.

Thurow, Lester
1975 Generating Inequality. New York: Basic Books.

Thurow, Lester C., and Robert E. B. Lucas
1972 The American Distribution of Income: A Structural Problem. U.S. Government Printing Office: Washington, DC.

Treiman, Donald J., and Kermit Terrell
1975 "Sex and the process of status attainment: a comparison of working women and men." American Sociological Review 40:174–200.

U.S. Bureau of the Census
1970 Census of the Population: 1970. Volume I, Characteristics of the Population; Part 1: U.S. Summary, Section 2. Washington, D.C.: U.S. Government Printing Office.

Wilensky, Harold
1960 "Work, careers, and social integration." International Social Science Journal 12: 543–60.

Winship, Christopher, and Rachel Rosenfeld
1979 "Youth unemployment and job mobility." Proposal submitted to the National Science Foundation, Applied Research Division. University of Chicago: National Opinion Research Center.

Wolf, Wendy C.
1976 "Occupational attainment of married women: do career contingencies matter?" Madison, Wisconsin: Center for Demography and Ecology Working Paper 76–3.

CLASS AND INCOME INEQUALITY IN KOREA*

HAGEN KOO
Memphis State University

DOO-SEUNG HONG
Seoul National University

American Sociological Review 1980, Vol. 45 (August):610–626

This paper seeks to contribute to recent efforts to improve status attainment models by introducing class categories or labor market segmentation. The research reported here replicates and extends Wright and Perrone's (1977) analysis of class categories and income inequality in the United States in the context of a rapidly industrializing society. The results confirm that Marxist class categories are as consequential as occupational status in explaining income inequality in both advanced and developing capitalist societies. Yet, there are several difficulties in applying this class model to developing economies, so that a substantially modified class model is developed taking the manual-nonmanual distinction and structural marginality as additional major axes of class determination. This new model is superior to Wright's (1976) model in explaining income inequality in Korea. In the final part of the paper, a tentative test of a dependency model of inequality is attempted, with negative results for the predicted hypothesis of the center-periphery sectoral inequality.

Class inequality is generally understood to be more crude and ominous in the early stage of capitalism than in its mature stage. With little organized power of labor and with little institutional protection, the exploitation of wage laborers by capitalists can assume its most naked character. At the same time it is also widely held that crystallization of class interests and clear demarcation of class boundaries in developing capitalism are severely limited by the remnants of precapitalistic elements and various distorting influences of the metropolitan economies in the world capitalist system. But beyond these broad expectations, little systematic knowledge has been advanced regarding the structure and processes of class inequality in peripheral capitalist societies. This paper seeks to fill a part of this vacuum with an empirical analysis of class inequality in one developing capitalist nation, South Korea.

In the present analysis, we take a point of departure from a recent study by Wright and Perrone (1977) of the relationships between class categories and income inequality in the United States. Wright and Perrone developed a reasonable operationalization of broadly Marxian class categories and demonstrated with multivariate quantitative analysis the significance of class positions in determining individuals' incomes in the States. We thought it would be useful to replicate their analysis with Korean sample data to see the similarities and divergencies in the class structures of the two societies. This analysis is presented in the first part of the paper.

But, as will be explained later, Wright and Perrone's model of class structure is not wholly satisfactory in the context of most peripheral capitalist societies. Thus, in the second part we develop a substantially modified class model and subject it to the same analysis. The results of both analyses are consistent with the American findings—that class positions are more consequential for income inequality than occupational status, and that class positions also have effects on the ways in which the individual's education influences his/her income. In the final section, we go beyond this status attainment framework and discuss some important issues raised by our data for the theory of class structure in peripheral capitalist societies.

---

* Direct all communications to: Hagen Koo; Department of Sociology; Memphis State University; Memphis, TN 38152. This research was supported in part by a grant from National Institute of Education (grant NE-G-00-3-0119) to William Cummings. Preparation of this article was supported in part by Faculty Summer Grant (1978) from Memphis State University to the first author. We would like to thank Edna Bonacich, Erik Wright, Dennis Hogan, Amy Ong Tsui, and an anonymous reviewer of *ASR* for their helpful comments. An earlier version of this article was presented at the 1979 annual meeting of the American Sociological Association, Boston.

Wright's Class Model

Prior to his empirical analysis of class and income inequality in a coauthored article with Perrone (Wright and Perrone, 1977), Wright (1976) had advanced an operationalization of what he regarded as Marxian class categories. Following the unquestionably Marxian tradition, he first defined classes in terms of common structural positions within the social organization of production. He then proposed to define these structural positions in terms of three basic criteria: (1) ownership or nonownership of the means of production, (2) purchase or sale of the labor power, and (3) control or noncontrol of the labor power of other workers.

A combination of the first two criteria produces three major class categories which were identified by Marx himself: the capitalists, who own the means of production and purchase the labor power of others; the workers, who do not own the means of production and sell their labor power; and the petite bourgeoisie, who own the means of production but neither buy nor sell labor power.

Wright argues, however, that this orthodox conceptualization is not adequate in the advanced capitalist societies and requires some important extension. Most contemporary Marxist scholars agree on this point, although there is less agreement on how to modify it. Wright's claim is that in the contemporary capitalistic organization of production, control over the labor process constitutes a significant axis of the social division of labor. He insists that the managers who do not own the means of production but exercise control over the labor process occupy the "contradictory locations" torn between the bourgeois and the proletariat positions. It is more legitimate, he argues, to treat such contradictory positions as such, that is, as a distinct class position, rather than to include them arbitrarily in either the bourgeoisie or the proletariat. Thus, in effect Wright subdivides the larger worker class into the manager and the worker classes, according to the third criterion of control of the labor power of others. Wright's model can be schematically presented as shown in Table 1.

The Korean Economy

Before describing our data and variables for analysis, a few comments on the Korean economy may be useful. South Korea is widely known as an exemplary case of remarkable economic success through export-led industrialization. During 1963 to 1974, the real gross national product (GNP) more than trebled, growing at 10% per annum, and the per capita GNP reached an excess of $500 in current United States dollars in 1975 (Hasan, 1976; Westphal, 1978). Most observers agree that manufactured exports have been the engine of Korean economic growth. Statistics show that within a decade, from 1965 to 1975, the ratio of total exports to GNP more than trebled and the share of GNP originating in the manufacturing sector more than doubled. Manufactured products constituted 42% of total exports in 1965 and 75% in 1975 and the contribution of total exports to GNP rose from 12.9% in 1965 to 26.3% in 1975 (Westphal, 1978).

These spectacular economic changes have transformed Korea from a largely agricultural society into a semiindustrialized country in a relatively short time. Compared with other countries at similar levels of income and population, Korea maintains relatively high shares of manufacturing products to GNP and manufacturing employment, and its export

Table 1. Wright's Class Model

Class Categories	Ownership of the Means of Production	Purchase or Sale of the Labor Power	Control of the Labor Power of Others
Capitalists	Yes	Purchase	Yes
Managers	No	Sale	Yes
Workers	No	Sale	No
Petite Bourgeoisie	Yes	Self-Employed	No

shows the heavily industrial content. In 1975 the percentage share of manufactured products in GNP was 31.9%, while that of primary products was 23.0%; social overhead accounted for 13.7% and other services contributed 31.4%. In employment, 21.9% of the active labor force in 1976 was engaged in the secondary industry, 44.6% in the primary sector, and 33.5% in the service and commercial sector (Economic Planning Board, 1979). The official unemployment rate in 1976 was only 5.0%.

At the same time, Korea, like many other Third World countries, is heavily dependent upon foreign capital. Westphal (1978) notes that between 1960 and 1975 roughly 40% of total investment was financed from abroad. In the period from 1962 to 1966, Korea accepted 308 million dollars worth of foreign capital, primarily from Japan and the United States, of which 37.5% was in the form of public loans, 58.2% commercial loans, and 4.3% direct investment (Economic Planning Board, 1979). In the period from 1972 to 1976, this figure jumped to 5,989 million dollars with a noticeable increase in the proportion of direct investment (9.4%). According to the World Bank's report (1978), Korea's external public debt in 1976 was about 27.6% of GNP and the debt service ratio to GNP was 3.8%. According to these measures, external dependence of the Korean economy is higher than that of many other developing economies. But for a dependent economy, its performance in the past decade and a half have been really remarkable.

Data and Variables

The data analyzed in this paper were collected in the capital city of Seoul in the summer of 1976. Seoul is a typical example of a primate city; more than one-fifth of the nation's population (7.2 million residents in 1975) as well as the major political, economic, and educational institutions are concentrated in this city. If Seoul does not represent the whole nation, it certainly does constitute the most vital part of it. Its economy is fully industrial and capitalistic and it is in this economy that the profound economic changes in the nation have expressed themselves with least inhibition.

The survey data were gathered from a probability sample of male household heads, aged 21 to 65.[1] A sample of 900 household heads was originally drawn through the multistate random sampling method from the official Register of Residents, which provides a fairly complete list of residents. Due to recent moves and extended trips as well as some refusals, 795 interviews (88.3%) were completed by trained university students majoring in sociology and related social sciences. Of the 795 completed interviews, 21 were from respondents aged under 20 or over 65, and thus deviated from our sampling frame. Excluding these 21 cases, our usable sample includes 774 cases.

Variables

The main purpose of this analysis is to determine the effect of class categories on individual income, while controlling for the effects of education, occupational status, age and family background. Class categories are derived from the classification scheme described above. Ownership of the means of production is operationally defined by ownership of an independent enterprise. Accordingly, all the self-employed are treated as property owners.[2] Control of the labor power of other workers is measured on the basis of detailed job descriptions obtained through interviews and the number of employees in the work place. As in Wright's classification, teachers are excluded from the manager category and both manual and nonmanual workers are included in it. Purchase of labor power is a critical criterion that distinguishes capitalists from the petite bourgeoisie. In Korea, many small shopkeepers hire one or two assistants. Though nominally property owners and employers, their relations with hired workers are not typically capital-labor relations since these shopkeepers are immediate producers themselves, and their employees are usually their relatives. Thus, we decided to classify the employers of one or two workers in the category of the petite bourgeoisie.

---

[1] Women were not included in our sample because of budgetary constraint. This is unfortunate, because women's economic role and their class situations in industrializing societies are becoming more problematic from both a theoretical and an empirical point of view. Recently, Korean women have begun to participate in increasing numbers in marginal service occupations as well as in export industries as largely unskilled labor. Since women's wages are typically lower than men's, we conjecture that the inclusion of women in our sample would accentuate class differences between the capitalists and white-collar workers on the one hand, and blue-collar and marginal workers on the other.

[2] We will later point out that this is not a very satisfactory operationalization in less developed societies. Our interest in the present analysis is to follow Wright's operationalization as closely as possible for comparison of two societies at different stages of capitalistic development.

Education is measured by a 7-point credential scale rather than by years of education: (1) no or some elementary school; (2) completed elementary school (grade 6); (3) some secondary school; (4) completed secondary school (grade 12); (5) some college; (6) college degree; (7) post-college. Strictly speaking this is not an interval variable, although treating it as such would not violate common research practice in sociology nor does it affect any of our substantive conclusions. In the following analysis, we will use it primarily as an interval variable; but, when using discrete educational levels that may add new interpretations (particularly for examining class differentials in educational income returns), we use education as both a continuous and a categorical variable.

Income data were originally reported categorically (12 categories) and refer to the average monthly income derived from the respondent's occupation and occupation-related activities. For the purpose of this analysis we took the midpoint of the response interval as a point estimate of the respondent's income.[3] The Korean monetary unit is *won*, 485 *won* being equivalent to one U.S. dollar in 1976.

Occupational status is measured by applying Treiman's Standard International Occupational Prestige Scale (SIOPS) to 77 occupational categories (Treiman, 1977). Although the validity of this scale in the context of Korean society should be determined by a specifically designed nationwide survey, we attempted our own measure of validation. Specifically, we asked the respondent to compare the "social prestige" of 31 familiar occupational categories with his own occupation and rate them as "much higher," "similar," "lower," or "much lower." Later we assigned scores (5 to 1) to these categories and calculated the average score attributed to each occupation by these pair-wise comparisons. The product-moment correlation between Treiman's scores and our own scores is .75. Given this fairly high correlation, it should not matter too much which measure we use. However, there is no doubt that Treiman's international measure has more merits for a comparative study.

In the case of father's occupational status, we used only the first digit of Treiman's two-digit scores (e.g., 10 through 19 = 1) because more than a half of our respondents' fathers had farm occupations which show little occupational differentiation. In addition, farm-nonfarm categories of father's occupation are included in the analysis, partly to compensate for gross differentiation in father's status and partly to see the effects of this new sectoral dimension of social background (which overlaps closely with rural-urban background).

Age is also assumed to be related to income, not only as a proxy for years of work experience in the labor market but also as an important basis of reward allocation in this still heavily age-oriented Asian society.[4]

---

[3] It is notoriously difficult to obtain valid income data in most developing societies. We took special care to overcome this problem, by assuring our respondents that the information was confidential and stressing the importance of income information for this "worthy academic" research. We believe that our respondents gave fairly accurate information about their incomes received for their main or "formal" occupational activities but probably not about other incomes derived from their "informal" activities (such as money lending, property renting, commissions, or bribery). For this reason, we suspect that incomes of property owners and managers might have been underreported and, therefore, there is no compelling reason to employ log transformation of income data. Besides, using actual earnings makes interpretation more straightforward. In any event, our trial analysis with logged income variable produced essentially the same results as far as our hypotheses are concerned.

[4] Age shows a mild curvilinear relationship with income, as linear income increase tails off (but not

Table 2. Means and Standard Deviations (in Parenthesis) of Major Variables by Wright and Perrone's Class Categories

Class Categories	Income[a]	Education[b]	Age	Occupational Status	Father's Occupation	Farm Background	N
Capitalists	171,200 (87,940)	4.44 (1.54)	44.5 (9.5)	51.73 (10.60)	4.10 (1.02)	58%	104
Managers	168,090 (75,250)	5.16 (1.17)	40.8 (7.6)	57.38 (9.04)	4.45 (1.13)	48%	117
Petite Bourgeoisie	95,390 (60,350)	3.58 (1.35)	42.1 (9.6)	41.48 (10.78)	3.77 (.93)	63%	222
Workers	94,740 (55,330)	4.01 (1.57)	38.0 (8.2)	39.71 (15.43)	3.88 (1.04)	60%	236

[a] Income is expressed in Korean monetary unit, *won;* 485 *won* was equivalent to one U.S. dollar in 1976.
[b] Education is measured by a 7-point credential scale. See text for details.

Data Analysis

We will first compare the distribution of class categories in our sample with that in the American sample before examining the effect of class on income inequality. Table 2 shows the frequency distribution of four classes and the means and standard deviations of the variables for each class category.

The most remarkable difference between Wright and Perrone's samples and ours is the size of the petite bourgeoisie. In their American national nonfarm representative sample, the petite bourgeoisie constitutes only 4.3%, whereas in our sample it makes up 32.7%. Due to extremely small numbers, Wright and Perrone justifiably excluded the petit bourgeois category from their analysis. Obviously we cannot do the same, so our analysis will deal with all four categories.

The data also indicate that in spite of rapid industralization the proletarianization of the Korean urban labor force has not developed very far; only a little more than one-third (34.8%) of our sample belongs in the worker category. Interestingly, this is the case in the most industrailized part of Korea. In Wright's nonfarm sample, the workers constitute 49.2%.

About one-third of our sample is split between the capitalist and the manager classes. As in Wright and Perrone's sample, the capitalists in our sample are mostly small capitalists (71 of 104 employ fewer than 10 workers and another 22 employ 10 to 29 workers). About 15.3% of the sample falls in the capitalist category and 17.2% in the manager category; the comparable figures in the American sample are 7.4% and 37.4%. That the manager class is much smaller in Korea is easily understandable in view of a much smaller proportion of workers and the relatively smaller sizes of Korean industrial and commercial enterprises.

The income data in Table 2 reveal that the average monthly incomes of small capitalists and managers are almost identical (171,200 won or about $353 and 168,100 won or $347), while workers and the petite bourgeoisie are also quite similar in their incomes (94,700 won or $195 and 95,400 won or $197). To the extent that we can assume that income is a crucial component of class situations, this pattern of income distribution is a strong indication of a polar structure of Korean stratification. In regard to education, the managers seem to be the most highly educated (5.16 in our scale, which corresponds to some college education), while small capitalists are only slightly more educated (4.44) than workers (4.01). Educational attainment is lowest among the petite bourgeoisie (3.58). If we juxtapose the polar structure of income distribution with a smooth gradation of educational resources, we can find a clue to the independent role of the social organization of production in creating what we call class inequality beyond a simple stratification.

In order to test the first hypothesis concerning the independent effects of class

---

dramatically) after age 55. But since this curvilinearity is not strong and since only a very small proportion of our sample falls over age 55, we decided to use raw ages in all regression equations.

categories, we will employ dummy variable regression analysis, using the following equation:

$$\begin{aligned} \text{Income} = {} & a + b_1\text{Education} + b_2\text{Age} \\ & + b_3\text{Father's Occ.} \\ & + b_4\text{Occ. Status} \\ & + b_5\text{Capitalist} + b_6\text{Manager} \\ & + b_7\text{Petite Bourg.} \end{aligned} \tag{1}$$

The results of the analysis presented in Table 3 show that even if we hold education and other causal components of income constant, employers are likely to receive about 47,000 won more, and managers 31,000 won more, than workers. These differences are statistically significant (regression coefficients being greater than twice their standard errors). The difference between workers and the petite bourgeoisie, however, is both statistically and substantively insignificant. A test for the overall significance of the class dummy variables considered as a whole (by comparing the explained variance before and after the dummies are added, and computing an F-test with 3 and 676 degrees of freedom) reveals that class effect on income is significant at .001 level of significance.

But a better way to weigh the effect of class variable is to compare it with the effect of occupational status. Wright and Perrone's analysis showed that class has a stronger effect than occupational status. For this comparison, we try to determine whether the regression equation which includes class variables (Equation 2) explains a larger proportion of variance in income than does Equation 3 which includes occupational status.

$$\begin{aligned} \text{Income} = {} & a + b_1\text{Education} + b_2\text{Age} \\ & + b_3\text{Father's Occ.} \\ & + b_4\text{Capitalist} + b_5\text{Manager} \\ & + b_6\text{Petite Bourg.} \end{aligned} \tag{2}$$

$$\begin{aligned} \text{Income} = {} & a + b_1\text{Education} + b_2\text{Age} \\ & + b_3\text{Father's Occ.} \\ & + b_4\text{Occ. Status} \end{aligned} \tag{3}$$

Table 3 shows that Equation 2 explains 46.9% of the variance in income, while Equation 3 explains 43.3%. From a different view, when class is added to the equation containing occupational status, the total explained variance increases by 5.0% (48.3 − 43.3), whereas when status is added to the equation containing class, the increase is only 1.4% (48.3 − 46.9). An obvious conclusion asserts itself: class has a greater explanatory power than occupational status in predicting income. Thus, the first hypothesis is confirmed by our data.

The second hypothesis suggests that class position affects the relationship between education and income, producing different income returns to education for different social classes. In statistical terms, this hypothesis predicts that education slopes are not constant but vary significantly according to class categories. In order to test this hypothesis, we will compare four class categories for the following two regression equations.

$$\text{Income} = a + b_1\text{Education} \tag{4}$$

$$\begin{aligned} \text{Income} = {} & a + b_1\text{Education} + b_2\text{Age} \\ & + b_3\text{Father's Occ.} \end{aligned} \tag{5}$$

The findings presented in Table 4 show that income returns to education are high-

Table 3. Regressions of Income on Wright and Perrone's Class Categories and Other Causal Factors of Income (Standardized Regression Coefficients Are in Parentheses)

	Dependent Variable = Income							
Constant	Education	Age	Father's Occupation	Occupational Status	Capitalist Dummy	Manager Dummy	Petit Bourg. Dummy	R^2
(1) −94,370	23,330	1,650	3,780*	780	45,890	31,010	100*	.483
	(.42)	(.20)	(.05)	(.16)	(.23)	(.16)	(.00)	
(2) −84,480	24,310	1,770	3,870*	—	53,560	39,590	2,870*	.469
	(.50)	(.21)	(.05)	—	(.26)	(.20)	(.02)	
(3) −121,857	21,400	2,010	4,330*	1,200	—	—	—	.433
	(.44)	(.24)	(.06)	(.24)	—	—	—	

* Coefficient less than twice its standard error.

Table 4. Comparison of Income Returns to Education for Four Class Categories

A. Regression Coefficients and Standard Errors (in Parentheses)

	Small Capitalists		Managers		Petite Bourgeoisie		Workers	
	EQ. (4)	Eq. (5)	Eq. (4)	Eq. (5)	Eq. (4)	Eq. (5)	Eq. (4)	Eq. (5)
Education	26,480	27,970	27,800	32,170	18,700	17,430	22,560	24,060
	(5,210)	(5,460)	(5,660)	(6,070)	(3,000)	(3,330)	(1,620)	(1,730)
Age	—	3,370	—	4,090	—	860	—	1,310
		(810)		(870)		(430)		(320)
Father's Occ.	—	−1,070	—	4,430	—	10,100	—	1,090
		(8,450)		(6,180)		(4,590)		(2,570)
Constant	56,580	−96,580	26,490	−181,000	26,840	−43,070	3,140	−56,670
R^2	.23	.35	.20	.36	.18	.22	.48	.52

B. Selective Comparison of Returns to Education

	Workers Compared to Small Capitalists		Workers Compared to Managers		Workers Compared to the Petite Bourgeoisie		Small Capitalist Compared to the Petite Bourgeoisie	
	Eq. (4)	Eq. (5)	Eq. (4)	Eq. (5)	Eq. (4)	Eq. (5)	Eq. (4)	Eq. (5)
Difference in Education Coefficients	3,920	3,910	5,240	8,110	3,860	6,630	7,790	10,540
t-Value Difference	1.2	1.2	1.5	2.2*	1.6	2.6**	2.0*	2.5**

* Significant at .05 level with one-tail significance test.

** Significant at .01 level with one-tail significance test.

est among managers and lowest among the petite bourgeoisie. Considering the simple regression of income on education (Eq. 4), managers as a whole earn 27,800 won for each increment in education, compared with 18,700 for the petite bourgeoisie; comparable income returns for small capitalists are 26,480 won and 22,560 won for workers. According to t-test results, the differences between the petite bourgeoisie and managers or small capitalists are statistically significant at the .05 significance level, whereas the difference between workers and any other class category is not significant.

The pattern of differentials between classes remains the same when we hold age and family background constant, but the gaps tend to widen. Again income returns to education are highest among managers; for each increment in education, they tend to receive 4,200 won more than small capitalists, 8,110 won more than workers, and as much as 14,740 won more than the petite bourgeoisie. The latter two differences are statistically significant at the .05 level. While small capitalists receive 3,910 won more than workers for each level of education, the difference is not statistically significant, whereas the difference of 6,630 won between workers and the petite bourgeoisie is statistically significant. There is also a marked difference (10,540 won) between small capitalists and shopkeepers, which is statistically significant.[5]

That managers rather than capitalists receive the highest income returns to education in Seoul disagrees with the reverse pattern found in Wright and Perrone's American sample. But our finding makes more sense because employers are not paid wages in relation to education but, rather, pay themselves out of their profits. The same logic applies to the higher return among workers than among the petite bourgeoisie.

---

[5] The significance of the education slope differences between class categories is assessed by the following t-test:

$$t = \frac{b_{11} - b_{12}}{\sqrt{\dfrac{(N_1 - 1)S^2b_{11} + (N_2 - 1)S^2b_{12}}{(N_1 + N_2 - 2)}}}$$

where b_{11} is the education coefficient for group 1, b_{12} is the education coefficient for group 2, S^2b_{11} and S^2b_{12} are the squared standard errors of the coefficients for groups 1 and 2, respectively, and $(N_1 - 1)$ and $(N_2 - 1)$ are the degrees of freedom for group 1 and 2, respectively.

Overall, we may conclude that our data provide a modest, limited support to the second hypothesis. Our data do suggest some variability in the relationship between education and income according to class positions, but it seems that the real significance of the class variable in explaining income inequality is not so much in its interaction with education as in its independent causal influence on the individual's income.

A Modified Class Model

Although the previous analysis has indicated the utility of Wright's classification of "Marxist" class categories in examining income inequality in Korea, there are several problems in his conceptualization, particularly when applied to a developing nation. In what follows, we will discuss some of the limitations of Wright's model and will propose a substantially modified class model which we believe is more appropriate to the study of peripheral capitalist societies.

Probably the most questionable element in Wright's classification is the way he separates managers and supervisors as a distinct class from the working class. His justification is that those who do not legally own the means of production but exercise control over the labor process occupy a "contradictory location" between capitalists and workers, and thus must be treated as a separate class. In effect, this approach amounts to recognizing authority in work organizations as an essential basis of class division, and, as such, this is, as Robinson and Kelley (1979) have already pointed out, a Dahrendorfian rather than a Marxian view.[6]

Wright's scheme, therefore, involves the difficulties of Dahrendorf's approach, which was sharply criticized by Giddens (1973). Giddens argued that (1) authority divisions are basically not dichotomous relations but very often involve a graded hierarchy of relations; (2) "there is no reason to assume . . . that exercise of authority presupposes a (latent) opposition of interest between those in authority and those who are subject to that authority;" and, most crucially, (3) this view "logically implies the recognition of an indeterminate plurality of classes" emerging in every "imperatively coordinated association" ranging from cricket clubs and schools to factories and the state bureaucracy (Giddens, 1973:72–3). The authority division may describe the cleavages within formal or bureaucratic organizations but not cleavages that occur at the societal level because, as Parkin (1971:46) points out, "the lines of authority which cut across each formal organization do not interlock, as it were, to form a major cleavage cutting across society."

Apart from these general difficulties, the utility of the Dahrendorfian scheme is particularly questionable in developing economies where legal ownership and actual control of the means of production are usually in the same hands. In these economies, industrial organizations are typically very small and, consequently, the labor process itself is often controlled by the owners of the enterprises themselves. Elaborate authority hierarchies that do exist in these societies are more likely to be found in state bureaucracies, where graded hierarchies are more likely to exist than dichotomous authority groupings. In fact, Dahrendorf himself did not argue that authority was a major class division in societies that were not industrialized.

Another related problem with Wright's model is his formal and legalistic definition of ownership of the means of production. In that model only legal owners constitute the capitalist class. This approach is contrary to the predominant view of contemporary Marxist scholars, who believe that ownership must be understood substantively rather than juridically. If we take ownership substantively as actual control, then top managers who have the power to assign the means of production to given uses and to dispose of the products thus obtained, must be understood as occupying the capitalist class position (see Poulantzas, 1975). It is difficult to believe that these top managers occupy "contradictory" locations, as may minor supervisors who have control over the labor power of others but who do not have control over the means of production.

---

[6] Wright and Perrone (1977:84ff.) tried to distinguish their approach from Dahrendorf's by arguing that "authority is defined specifically in terms of the social relations of production, not the social relations of any organization whatsoever." But this does not take us very far and offers no practical guideline for actual classification, unless they want to restrict authority relations to particular types of organizations such as "productive" as opposed to "unproductive" ones in the traditional Marxian sense. Wright (1976), however, emphatically denied the importance of this distinction in the earlier article.

A third problematic element in Wright's classification is his attempt to infer ownership of the means of production from self-employment. Although this may be a reasonable assumption in most highly industrialized societies, the same is not true in developing societies where self-employment often conceals a variety of underemployment such as selling homemade juice on the street, shoeshining, and sharpening knives in back alleys. These "marginal" forms of self-employment are, of course, very different from regular shopkeeping or wholesaling. Although the latter activities involve at least a modest amount of capital investment and a stable source of income, the former have none of these. There is a growing recognition that the expansion of this sort of marginal activity (i.e., "marginalization") in large urban centers is a pervasive phenomenon in the less developed countries and may be an unavoidable consequence of the capitalist development in the periphery of the world capitalist system.[7]

The new class model we propose here shares with Wright's model the Marxian premise that classes designate structural positions in the social division of labor. But when it comes to the question of what defines the social division of labor, our conception deviates substantially from the orthodox Marxian notion. We assume that the three most important axes of the social division of labor in the contemporary capitalist economy are (1) ownership or nonownership of the means of production (understood as actual control rather than legal ownership of it), (2) purchase or sale of labor power, and (3) the manual and nonmanual, or white-collar and blue-collar, social division of labor.

The first two are well-known Marxian criteria and therefore require no special explanation. The manual-nonmanual distinction is, however, not a Marxian idea. Yet, probably no other class division is regarded as more important than this one by most non-Marxian scholars. First, it is a significant form of social division of labor; manual and nonmanual workers differ in their relations to the means of production (manual work being more subordinated to the exigencies of machines), to the owners of the means of production (manual workers being more directly exploited by producing surplus value of labor), and to one another (office workers being socially isolated from manual workers on the shop floor). Second, the manual-nonmanual distinction is associated with two distinct levels of "market capacities." In most societies, educational requirement for white-collar jobs is substantially higher than what is required for blue-collar jobs, in that those without secondary or postsecondary educational credentials cannot even compete for white-collar jobs. Third, owing to the above two factors, there usually exists a marked mobility barrier between manual and nonmanual occupations. In most societies, social mobility, both intra- and intergenerational, tends to occur within manual and nonmanual occupations. Unlike Marxist theorists, neo-Weberian scholars such as Parkin (1971) and Giddens (1973) stress the latter two factors, differential market capacities and "mobility closure," as the major bases of class determination (or "class structuration" in Giddens's terms), and here we side with them. Thus, in effect, we propose to substitute a Weberian conception for Dahrendorf's in Wright's model.

There is convincing evidence of considerable meshing of the labor process at the boundary of manual and nonmanual jobs in the United States (Braverman, 1975;

---

[7] "Marginalization" refers to many phenomena in the literature, but in "dependency" literature it usually means "increasing underemployment and poverty" in the urban centers of peripheral nations (Amin, 1976:357). Similarly, "marginality" is often defined as "the lack of difficulty of access to a reasonable and stable income" (Sunkel, 1973:142). See also Johnson (1972) and Peattie (1974) for slightly different definitions.

also Kalleberg and Griffin, 1978). This is partly due to the increasingly mechanized and routinized nature of lower clerical jobs and "feminization" of clerical work, and partly due to the growing power and material rewards of the unionized skilled workers. If this labor process continues, the manual-nonmanual distinction may eventually lose much of its social significance in industrial societies. But such is not the case in developing societies. In them, manual jobs are mostly unskilled work and lower white-collar jobs are not so routinized or feminized as in industrial societies. In most developing societies, the colonial legacy of high authority and prestige associated with office work still persists. Furthermore, access to higher education is much more limited in these societies and, consequently, white-collar occupations that require this scarce qualification enjoy greater material and symbolic advantages. It is thus reasonable to assume that a greater mobility barrier exists across manual and nonmanual positions in developing societies. In short, we believe the manual-nonmanual division to be more important in developing societies than in industrialized societies.

Now that we have isolated three primary axes of the social division of labor, we can combine them to identify five major class categories[8]: (1) the *capitalist class,* which includes legal owners of the means of production who purchase the labor power of others and top managers who have actual control of the means of production—albeit without legal ownership;[9] (2) the *new middle class,* which is composed of the nonmanual salaried employees; (3) the *working class,* which consists of manual workers who are hired by capitalists; (4) the *petite bourgeoisie,* which includes small property owners, mostly shopkeepers, who are either self-employed or employers of one or two helpers; and (5) the *marginal class,* which includes the essentially propertyless self-employed (such as peddlers, street vendors and itinerant repairmen) who are engaged in marginal-scale trade and personal services. Included also in this category are casual laborers and domestic service employees (e.g., barbers, taxi drivers and restaurant employees). Table 5 presents this classification scheme.

Analysis Based on New Class Categories

It is apparent from Table 6 that the marginal class and the working class are the two most underprivileged classes in Seoul. On the average, marginal workers earn less than two-thirds of manual wage workers' income (which itself is very small, about $155 per month) and a little more than a quarter of the shopkeepers' (about $248). Of greater importance may be the enormous gap in income between the white-collar and the blue-collar workers; the latter receive on the average only about one-half of the former's income (about $293). Notice that the income differential between the white- and blue-collar workers (142,240/75,100 = 1.89) is slightly larger than the differential between managers and workers in Wright's model (168,090/94,740 = 1.77).

The first hypothesis concerning the independent effect of class positions on the individual's income is summarized in the regression analysis in Table 7 and reveals that class dummy variables, jointly with education, age, and father's occupation, explain 53.7% of variance of income (see Equation 2). This is substantially larger than the proportion of variance explained by occupational status conjointly with other independent variables (49.9% in Equation 3). If we add class variables to Equation 3 containing occupational status and other causal variables, we can increase the explained variance by 5.8%,

---

[8] Although this class model has been developed primarily with the urban class structure in mind, it may also be applicable to the rural structure. Thus, small independent farmers who constitute a majority of Korean rural population belong to the petite bourgeoisie and tenant farmers or farm laborers to the marginal class, while large landlords may belong to the capitalist class.

[9] Included also in the capitalist class are top-level governmental officials. Our justification for this classification is based on the well-known fact that in Korea, as in many other developing nations, the state exercises an enormous amount of power over the market economy and thus the managers of the state machineries often control the flow and actual disposition of the large amount of capital available to private industrial enterprises.

Table 5. Our New Class Model

Class Categories	Ownership/Control of the Means of Production	Purchase or Sale of the Labor Power	Manual-Nonmanual Distinction
Capitalists	Yes	Purchase[1]	Nonmanual
New Middle Class	No	Sale	Nonmanual
Working Class	No	Sale	Manual
Petite Bourgeoisie	Yes	Self-Employed[2]	Nonmanual[3]
Marginal Class	No	Mostly Self-Employed[4]	Manual

[1] The exceptions are top managers in large corporations and the heads of the state.

[2] The employers of no more than two workers are classified in the petit bourgeois category.

[3] Small independent business is commonly regarded as a nonmanual occupation, but actually the manual-nonmanual distinction is not very relevant to this type of work.

[4] Although the marginal class consists mainly of workers on own account without viable capital (indicated by the absence of any permanent shop for trade), it also includes casual laborers and domestic service employees whose employment is basically temporary and unstable in nature.

whereas if we add occupational status to Equation 2, the increase is only 2.0%. It seems clear that the class variable has a greater explanatory power than occupational prestige has.

Looking at the content of Table 7 more closely, we note that the capitalist class clearly occupies the most privileged position in Seoul. Employers and top managers in this class earn as a whole 61,504 won more than manual wage workers, even after adjusting for the effects of education, age, and family background (see Equation 2); holding the same factors constant they are making 42,386 won (61,504 – 19,118) more than the white-collar workers, and 38,385 won (61,504 – 23,119) more than shopkeepers.

On the opposite end of the class structure, we notice that the marginal class is clearly the most disadvantaged one in Seoul. We have noted already that marginal workers' average income is markedly lower than that of the wage-earning workers. Table 7, additionally, suggests that income difference between these two classes is not solely due to the individual characteristics of their members but, more importantly, due to their differential structural positions in the total organization of urban economy; thus, even after holding other causal factors constant, we find an income difference of 30,523 won between blue-collar and marginal workers.

In contrast, the petite bourgeoisie seem to make higher incomes (after education and other variables are controlled), than not only the blue-collar or marginal workers but also the white-collar workers. This income advantage of the petite bourgeoisie is most likely to be derived from property ownership which characterizes its members, a variable which is not included in the regression equations. In any event, the finding is unambiguous in showing that the social classes that are based on ownership or control of the means of production, large or small, enjoy definite advantages over other classes in the economic reward system of Korean society.

Table 6. Means and Standard Deviations (in Parentheses) of Major Variables by New Class Categories

Class Categories	Income	Education	Age	Occupational Status	Father's Occupation	Percent Farm	N
Capitalist Class	182,050	4.6	45.0	54.1	4.2	57%	128
	(89,670)	(1.5)	(9.5)	(10.8)	(1.0)		
New Middle Class	142,240	5.3	38.3	54.0	4.3	48%	193
	(62,820)	(1.1)	(7.0)	(10.8)	(1.1)		
Working Class	75,100	3.3	36.4	33.7	3.5	67%	100
	(28,780)	(1.0)	(7.5)	(8.6)	(1.0)		
Petite Bourgeoisie	120,480	3.8	41.4	43.5	3.8	60%	151
	(59,520)	(1.4)	(8.8)	(10.6)	(0.9)		
Marginal Class	47,380	2.8	43.4	31.5	3.6	68%	107
	(23,270)	(1.2)	(10.0)	(11.6)	(1.0)		

Table 7. Regression of Income on New Class Categories and Other Causal Factors of Income (Standard Regression Coefficients Are in Parentheses)

				Dependent Variable = Income						
Constant	Education	Age	Father's Occupation	Farm-Nonfarm	Occupational Status	Capitalist Dummy	New Middle Dummy	Petit Bourg. Dummy	Marginal Dummy	R²
(1) −100,085	15,263	1,566	3,346*	−1,573*	1,646	38,957	246*	12,775*	−27,127	.557
	(.32)	(.19)	(.05)	(−.01)	(.28)	(.21)	(.00)	(.07)	(−.14)	
(2) −72,881	21,063	1,798	3,988*	−1,072*	—	61,504	19,118	23,119	−30,523	.537
	(.44)	(.22)	(.06)	(−.01)	—	(.33)	(.12)	(.13)	(−.15)	
(3) −133,606	14,286	1,722	3,134*	−1,432*	2,475	—	—	—	—	.499
	(.30)	(.21)	(.04)	(−.01)	(.43)	—	—	—	—	

* Coefficient less than twice its standard error.

In Equation 1, which includes occupational prestige, the coefficients of the new middle class and the petit bourgeois dummies become statistically nonsignificant, which means that income differences between these two classes and the manual working class are not significant when statistical adjustment is made for the effects of occupational status and other variables. This is to be expected since the white-collar and blue-collar distinction contains a heavy status component, while small business owners are usually accorded higher occupational prestige than the blue-collar workers. In general, our class model has greater overlap than Wright's model with the occupational prestige hierarchy. However, an F-test of the overall effect of the class variable as a whole, as was conducted with Table 3 before, shows that the class effect, independent of occupational prestige and other causal variables, is statistically significant at .001 level ($F=19.4$ with 4 and 675 degrees of freedom).

Now let us turn to the question of whether class position influences the way in which education affects income. As before, we conducted a separate regression analysis for each class category to compare regression coefficients of education across class categories. In the following equations only one control variable of age is introduced because father's occupational status and farm-nonfarm background were shown to be mostly insignificant, both statistically and substantively, in predicting income within class categories. The figures in parentheses indicate standard errors.

$$\begin{aligned}\text{Capitalist}\\ \text{Income} &= -117{,}280\\ &\quad + 31{,}620\ \text{Educ.}\\ &\qquad (4{,}270)\\ &\quad + 3{,}470\ \text{Age}\\ &\qquad (682)\\ &\quad (R^2 = .39)\end{aligned}$$

$$\begin{aligned}\text{White-Collar}\\ \text{Income} &= -97{,}700\\ &\quad + 30{,}110\ \text{Educ.}\\ &\qquad (3{,}460)\\ &\quad + 2{,}140\ \text{Age}\\ &\qquad (530)\\ &\quad (R^2 = .31)\end{aligned}$$

$$\text{Blue-Collar Income} = 2{,}420 + \underset{(2{,}760)}{14{,}200\ \text{Educ.}} + \underset{(360)}{720\ \text{Age}} \quad (R^2 = .22)$$

$$\text{Petit Bourgeois Income} = -44{,}110 + \underset{(3{,}540)}{21{,}750\ \text{Educ.}} + \underset{(530)}{1{,}950\ \text{Age}} \quad (R^2 = .24)$$

$$\text{Marginal Worker Income} = 54{,}920 + \underset{(267)}{1{,}350\ \text{Educ.}} + \underset{(200)}{280\ \text{Age}} \quad (R^2 = .20)$$

It is shown that income return to education is highest for capitalists and white-collar workers and lowest for marginal workers. Among two intermediate groups, shopkeepers are likely to receive significantly larger income returns than blue-collar workers. Table 8 displays differences of regression slopes (i.e., returns to education) between working-class and other class categories and the corresponding t-values. All these differences turned out to be significant at the .01 level.

Table 8. Selective Comparison of Class-Specific Regression Coefficients of Education and t-Values of Differences

Comparison	Difference in Education Coefficient	t-Value
Capitalist vs. Working Class	17,430	4.7*
New Middle vs. Working Class	15,910	4.9*
Petite Bourg. vs. Working Class	7,550	2.3*
Working Class vs. Marginal	9,540	4.1*
Capitalist vs. New Middle	1,520	.4
New Middle vs. Petite Bourg.	8,360	2.4*
Petite Bourg. vs. Marginal	17,090	5.8*

* Significant at .01 level with one-tail significance test.

Special attention needs to be paid to differences between white-collar and blue-collar workers. In spite of similarity in their labor market situations (i.e., both sell their labor power and are most likely to be employed in bureaucratic organizational settings), education seems to have differential impact for the two categories of workers. For each additional educational level, the members of the new middle class receive more than twice the amount of income received by the members of the working class. This is a strong indication of the privilege which the former enjoys over the latter in the reward structure of Korea. The blue-collar workers fare badly even in comparison with small shopkeepers; for each increment in education, the latter receive 7,550 won more than the former. As expected, education makes little difference for marginal workers. In both independent and interactive effects, their class position is characterized by true "marginality."

A question can be raised as to whether the smaller education effects found for the blue-collar and the marginal workers are an artificial result of treating education as an interval datum. It is true that education is not precisely an interval variable because two years of elementary school education would not be the same as two years of college education in affecting social rewards. Since manual workers are clustered at the lower end of the education scale where the income effect is relatively small, it is possible that lower income returns to education for these two class categories are a result of these scale-specific qualities rather than class interaction.

To check against this possibility, we calculated separate regression equations using educational credential dummy variables. (For simplicity of comparison, we grouped educational levels into three categories producing two dummy variables: high school—completion or noncompletion of high school; college—having or not having a college diploma.) The results shown in Appendix I, however, reveal no important discrepancies from the above findings, except that educational income returns for the capitalists turned out to be greater than shown above. It remains unchanged that blue-collar and marginal workers receive considerably lower incomes than white-collar workers or the petite bourgeoisie for each educational level increment.

In all, the class categories we have developed prove to have greater explanatory power than Wright's categories in both their independent and interactive effects on the individual's income in Seoul. This improvement seems to be primarily due to our recognition of the significant internal division among those who exist outside the organized labor market and secondarily to the more realistic perception of class boundaries in the dominant capitalistic sector.

Conclusion and Discussion

The dominant conclusion of the present analysis is that class categories, conceived principally in terms of the social division of labor, are more consequential than occupational prestige in determining individuals' incomes, in both advanced and developing capitalist societies. This conclusion seems to hold regardless of specific class models adopted in the present analysis, although our model seems to have a greater explanatory power than Wright's in the context of the Korean metropolitan center.

It must be admitted that the real value of this kind of analysis pertains not so much to the framework of Marxian class relations as to that of status attainment. It would be wrong if we argue that this study has established the overall validity of Marxian class theory. For class in Marxian theory means a much broader and more complicated relational process, and our data analysis has not really addressed the nature and quality of relations between social classes, let alone the consciousness of the individuals in these class positions.

Granting these limitations, we believe our study, nevertheless, sheds light on some important features of the Korean class structure which might be generalizable in other peripheral capitalist societies. First of all, our data revealed the quantitative significance of the self-employed—the labor force which is untouched by the wage labor system. The mode of production represented by this labor force is regarded as "simple commodity production" or "precapitalistic." Marx treated this element as "transitional" and based his theory largely on the relations of production within the predominantly capitalist mode of production. As such, the labor force that is not absorbed into the wage system occupies only a residual category in Marx's class model. As shown by our sample data, this element of the Marxian theory of class causes serious limitations in grasping the total reality of the class system in Korea; and these limitations apply to the majority of less developed nations where the self-employed represent 30% to 60% of the urban labor force.

Thus, an adequate approach to the class system in peripheral societies must pay close attention not only to the relations of production within the predominantly capitalistic mode of production but also to the relations within the simple mode of commodity production and, further, to the nature of relations between these two modes of production. There is increasing awareness of these problems among contemporary Marxist scholars, particularly concerning the question of "articulation" of the modes of production, but no satisfactory approach has been developed in the literature (see Foster-Carter, 1978).

In this connection we must consider the "center-periphery" model, which has received much attention recently in dependency literature. Center-periphery relations may be conceptualized in two ways: At the level of international relations, they refer to the relationships between the advanced capitalist countries and the economically underdeveloped dependent countries; at the domestic or national level, they involve the relationships between the industrial, modernized sectors or regions, and the backward, poor sectors or regions. It is central to dependency theories that these external and internal dimensions of the center-periphery relations are structurally similar, interlocked, and mutually reinforcing, for both are viewed as the consequences and manifestations of one single process, the evolution of the world capitalist system.

From this perspective, dependency writers advanced an important proposition that in the peripheral economies the economic sectors or groups that are closely linked to metropolitan capital (i.e., the "internationalized sector") prosper, while those that are segregated from it suffer growing material deprivation and unemployment. It is thus hypothesized, as Portes (1976:76) notes, that "the basic cleavage in these new quasi-industrial societies does not occur between traditional peasant and landowning classes or even between domestic bourgeoisie and urban proletariat but between groups linked with the foreign-owned sector (workers and employers alike) and the rest of the population." Carried a little further, this hypothesis implies "the replacement of class struggle by a sectoral struggle between those connected with the internationalized sector and those marginal to it" (O'Brien, 1975:19).

Unfortunately this hypothesis cannot be directly tested with our data because we are unable to distinguish the internationalized sector from the domestic or marginal sector. But there are some indirect ways in which we can at least partially assess the validity of this center-periphery hypothesis. One approach is by using the size of work organization (number of employees) as a rough measure of sectoral division, assuming that the internationalized sector is capital- and technology-intensive and highly bureaucratized, whereas the domestic sector enterprises are, for the most part, small-scale, utilizing little capital and simple technologies. We chose an arbitrary cutoff point at 30 employees and created a new dummy variable in which the enterprises employing 30 or more workers were regarded as belonging to the center sector, while those with fewer than 30 employees belonged to the periphery. A regression analysis was then conducted in a similar manner to the previous analysis. The results, displayed in Appendix II, show that the sectoral division has no significant effect on income, controlling for education, age, and occupational status. Inclusion of this dummy variable in the equation containing education, age, and occupational status adds nothing to the explained variance. The substitution of this sectoral variable with other sectoral dummies using different cutoff points (100 employees or 300 employees) produces virtually the same results.

In connection with this analysis, we should note that our previous findings have a definite bearing on the sectoral hypothesis at hand. Of the five social classes differentiated in our model, the petite bourgeoisie and the marginal class belong to the domestic or periphery sector, while the majority of white-collar and blue-collar workers in our sample participates in what can be regarded as the internationalized sector in the predominantly export-oriented economy of Seoul. Now, how consistent are our findings with the prediction of the center-periphery polarization hypothesis? There is no question that marginality, in its quantitative and qualitative dimensions, is an economic reality of this society. It may also be true that in this society the most serious economic deprivation does not occur in the form of typical capitalistic exploitation but rather in the failure of the people to enter into even such exploitative relations in the modern industrial sectors. But it is also true that the overall economic conditions of the industrial workers (71% of whom belong to the center sector by the above definition) are not tremendously better than those of the marginal workers. Our data revealed that the deprivation of proletarians seems equally critical (only about half of the average income of the new middle class), and the slight advantages they enjoy over the "subproletariat" may mean very little to their life chances. In comparison, many small property owners in the peripheral sector seem to be doing remarkably well economically. With the effect of education controlled, the petite bourgeoisie appear, in fact, to be earning more money than white-collar workers. There is little sign that all the workers who are excluded from the internationalized sector suffer growing material deprivation and marginality. Nor is there any convincing ground to assume that the workers participating in the internationalized sector benefit disproportionately from "dependent development" (Cardoso, 1972).

Although our data are very tentative in nature, we side with the view that the peripheral social formation consists of more than one mode of production. Here we identified two distinct modes of production, the capitalistic mode of production and simple commodity production, and we regard their interrelationships as problematic rather than as that of a simple "chain of exploitation."[10] It is our viewpoint that in developing societies the most crucial class relations are also defined by the capitalist relations of production, that is, by capital-labor relations, but actual political and ideological manifestations of these class relations are defined not simply by the relations of production within the capitalistic mode of production, but also by the ways this mode is articulated with other modes of production, and, consequently, by the nature of alliances between classes across different modes of production. In short, class analysis in peripheral capitalism must tackle immensely more multiplicative processes of class relations than envisioned by a traditional Marxist class model.

---

[10] See Laclau (1971) for an excellent synthesis of arguments on this issue.

APPENDIX I

Capitalist Income = −27,746 + 54,744 High*
(18,534)
+ 119,681 Coll*
(18,556)
+ 3,132 Age (R^2 = .358)
(716)**

White-Collar
Income = −18,184 + 41,931 High
(18,094)
+ 102,316 Coll
(17,732)
+ 2,216 Age (R^2 = .301)
(540)

Blue-Collar
Income = 48,956 + 21,265 High
(5,391)
+ 62,754 Coll
(18,760)
+ 445 Age (R^2 = .205)
(353)

Petit Bourgeois
Income = 16,731 + 31,910 High
(10,040)
+ 88,955 Coll
(14,542)
+ 1,786 Age (R^2 = .241)
(528)

Marginal Worker
Income = 48,338 + 15,337 High
(4,708)
+ 5,210 Coll
(13,266)
− 137 Age (R^2 = .101)
(220)

---

* The dummy variables High and Coll are measured as follows: High: (1) graduation from high school, (0) nongraduation from high school; Coll: (1) graduation from college, (0) nongraduation from college.

** The figures in parentheses indicate standard errors of regression coefficients.

APPENDIX II

(1) Income = −138,557 + 16,283 Educ
(.32)
+ 1,735 Age + 2,627 Occ
(.21) (.40)
(R^2 = .470)

(2) Income = −138,463 + 16,329 Educ
(.32)
+ 1,728 Age + 2,634 Occ
(.21) (.40)
− 796 Centr30* (R^2 = .470)
(−.01)

(3) Income = −138,424 + 16,369 Educ
(.32)
+ 1,727 Age + 2,631 Occ
(.21) (.40)
− 1.360 Centr100* (R^2 = .470)
(−.01)

---

* The dummy variables Centr30 and Centr100 are measured as follows: Centr30: (1) enterprises with 30 or more employees, (0) enterprises with fewer than 30 employees; Centr100: (1) enterprises with 100 or more workers, (0) enterprises with fewer than 100 workers. The regression coefficients for both variables are not significant at the .05 level of significance.

REFERENCES

Amin, Samir
1976 Unequal Development: An Essay on the Social Formation of Peripheral Capitalism. New York: Monthly Review Press.

Braverman, Harry
1975 Labor and Monopoly Capital: The Degradation of Work in the Twentieth Century. New York: Monthly Review Press.

Cardoso, Fernando Henrique
1972 "Dependency and development in Latin America." New Left Review 74:83–95.

Economic Planning Board
1979 Major Statistics of Korean Economy. Seoul, Korea: Economic Planning Board.

Foster-Carter, Aidan
1978 "The modes of production controversy." New Left Review 107:47–77.

Johnson, Dale
1972 "On oppressed classes." Pp. 269–301 in J. D. Cockcroft, A. G. Frank, and D. L. Johnson (eds.), Dependence and Underdevelopment. New York: Doubleday.

Giddens, Anthony
1973 The Class Structure of the Advanced Societies. New York: Barnes and Noble.

Hasan Parvez
1976 Korea: Problems and Issues in a Rapidly Growing Economy. Baltimore: Johns Hopkins University Press.

Kalleberg, Arne and Larry Griffin
1978 "Positional sources of inequality in job satisfaction." Sociology of Work and Occupation 5:371–400.

Laclau, Ernesto
1971 "Feudalism and capitalism in Latin America." New Left Review 67:19–38.

O'Brien, Philip J.
1975 "A critique of Latin American theories of dependency." Pp. 7–27 in Ivar Oxxal, T. Barnett and D. Booth (eds.). Beyond the Sociology of Development: Economy and Society in Latin America and Africa. London: Routledge and Kegan Paul.

Parkin, Frank
1971 Class Inequality and Political Order: Social Stratification in Capitalist and Communist Societies. London: MacGibbon and Kee.

Peattie, Lisa R.
1974 "The concept of marginality." Pp. 101–9 in W. A. Cornelius and F. M. Trueblood (eds.), Latin American Urban Research 4. Beverly Hills: Sage.

Portes, Alejandro
1976 "The sociology of national development." American Journal of Sociology 82:55–85.

Poulantzas, Nicos
1975 Classes in Contemporary Capitalism. London: New Left Books.

Robinson, Robert V. and Jonathan Kelley
1979 "Class as conceived by Marx and Dahrendorf: effects on income inequality, class consciousness, and class conflict in the United States and Great Britain." American Sociological Review 44:38–58.

Sunkel, Osvaldo
1973 "Transnational capitalism and national disintegration in Latin America." Social and Economic Studies 22:132–76.

Treiman, Donald
1977 Occupational Prestige in Comparative Perspective. New York: Academic Press.

Westphal, Larry E.
1978 "The Republic of Korea's experience with export-led industrial development." World Development 6:347–82.

World Bank
1978 World Development Report, 1978. Washington, D.C.: World Bank.

Wright, Erik Olin
1976 "Class boundaries in advanced capitalist societies." New Left Review 98:3–41.

Wright, Erik Olin and Luca Perrone
1977 "Marxist class categories and income inequality." American Sociological Review 42:32–55.

THE STRUCTURE OF ATTITUDE SYSTEMS IN THE GENERAL PUBLIC: COMPARISONS OF A STRUCTURAL EQUATION MODEL*

CHARLES M. JUDD
Harvard University

MICHAEL A. MILBURN
University of Massachusetts-Boston

American Sociological Review 1980, Vol. 45 (August):627–643

Philip Converse and other theorists have argued that the public at large does not have meaningful and stable attitudes. Specifically, they have argued that the public's attitudes, as compared to those of the political "elite," show less stability over time and less consistency between issues and are less likely to be based upon an underlying ideological predisposition. These hypotheses have been supported, generally, by computing correlations between attitude questions and comparing them between public and elite samples. The present paper examines all three hypotheses using a structural equation approach. This allows us to: (a) test for a single common underlying ideological construct; (b) separate ideological stability from issue-specific stability; and (c) use unstandardized structural coefficients to make between-sample comparisons. Using panel data from 1972–1974–1976 national surveys, we find that both the highly educated and the uneducated public show evidence of an underlying ideological predisposition, show remarkable stability in their attitudes, and show equal consistency or constraint between different attitude issues.

A great deal of research in sociology is predicated on the notion that the public's attitudes on social issues, as measured by surveys and opinion polls, are relatively meaningful and stable. Various sociologists (Alwin, 1973; 1976; Wheaton et al., 1977) have examined attitude stability by suggesting models for estimating simultaneously the reliability and stability of attitudes over time. While this research certainly recognizes that survey measures of attitudes are affected by measurement error, it is generally assumed here, and elsewhere, that there is, in fact, some relatively stable underlying attitude, possessed by the respondent, which contributes to responses to attitude questions. Opinion polls are meaningful for researchers and for policy makers only if such an assumption is made.

While the assumption may seem to many to be trivial and obvious, there exists in the social sciences a substantial body of literature which has called it into question. In particular, Philip Converse (1964; 1970; 1975) has repeatedly questioned whether attitude responses from members of the public reflect anything more than spur-of-the-moment, random responses. Converse (1964:245) has compared the attitude responses of the mass public to responses from politically involved "elites" in making the argument that "large portions of an electorate do not have meaningful beliefs, even on issues that have formed the basis for intense political controversy among elites for substantial periods of time."

The Characterization of Attitude Systems

The comparison of attitude systems between the public at large and elites has been primarily along two related dimensions: (1) the degree of constraint among various political attitudes, and (2) the degree to which those specific attitudes are derived from and reflect a more general ideological position on an underlying liberal-conservative continuum.

Constraint among attitudes. Attitude constraint has been defined by Converse (1964:207) as "the success we would have in predicting, given initial knowledge that an individual holds a specified attitude, that he holds certain further ideas and attitudes." Traditionally, it has referred to two types of predictability between attitudes: between attitudes on different is-

---

* Direct all communications to: Charles M. Judd; Harvard University; 33 Kirkland Street; Cambridge, Mass. 02138.

This research was supported by N.I.M.H. grant MH 30509 to the first author. We also thank the Government Data Center, Harvard University, for access to the data, and Don Ferree, David Kenny, Carol Mueller, and Duane Alwin for comments on an earlier draft.

sues at the same point in time, and between attitudes on the same issue at different points in time. To discriminate between these two types of attitude predictability, we refer to the first as attitude constraint and to the second as attitude stability.

Differences in attitude constraint between the public at large and elites usually have been assessed by intercorrelating attitude measures at one point in time, separately for each of the two groups. The averages of these two sets of correlations are then used to compare the attitude constraint of elites and nonelites. Comparing 1958 congressional candidates to a cross-section of the national electorate, Converse (1964:229) found that the average association[1] among four domestic attitude issues was .53 for elites and .23 for the general population. Thus, Converse concludes that the attitudes of elites are much more constrained than those of nonelites.

A similar procedure has been used to assess attitude stability, although, in his original presentation, Converse (1964) reports on stability only for his nonelite sample. Using data collected at two-year intervals from 1956 to 1960, Converse reports that the average within-item, over-time correlation is approximately .36. While comparable data are not available on the elite sample, Converse clearly concludes that the nonelites show a remarkable lack of stability in their attitudes.

General ideological basis of attitudes. As the second dimension for comparison between elites and nonelites, Converse focuses upon the degree to which specific attitudes are organized or are consistent with a general ideological liberal-conservative continuum. Studies have traditionally measured the degree of such ideological organization by coding responses to open-ended questions on political candidates for the extent to which they refer to ideological principles (Campbell et al., 1960; Converse, 1964; Field and Anderson, 1969; Nie et al., 1976). Using such a coding procedure, Converse (1964) reports that only 2.5% of his American electorate sample consistently use a general ideological continuum in evaluating candidates. Another 9% peripherally refer to a general ideological continuum, and the remaining 88.5% of the sample fail to use any ideological dimension in answering the questions. Converse concludes from this analysis that most of the electorate tend not to refer to general ideological principles in evaluating issues or political candidates.

The relationship between these two dimensions—which Converse (1964) has used to characterize differences in attitude systems between elites and nonelites—is probably apparent to the reader. If a general ideological principle is used to some extent for evaluating issues, then attitudes on those issues should tend to be intercorrelated to at least that extent. In other words, specific attitude items should correlate with each other if they are derived from the same general underlying ideological predisposition.

Research on the attitude systems of elite and nonelite samples has been fairly prolific since Converse's (1964) original presentation. Some of this research has been critical of the methods employed by Converse to support his conclusions. A number of researchers have suggested that measurement error, vagueness in the wording of questions, or response variation without "true" attitude change may cause low interitem correlations without these necessarily reflecting random or meaningless attitudes (Achen, 1975; Pierce and Rose, 1974). Weissberg (1976) and Barton and Parsons (1977) have argued that differences in correlations between samples may be, in part, due to differences in response variance within samples. They suggest that Converse's conclusions may be biased because of differing variances in the two samples.

Some, including Converse himself (Converse and Markus, 1979), have used more recent data to amend, criticize, or support the original conclusions. Nie and his colleagues (Nie and Anderson, 1974; Nie et al., 1976) examined the constraint of elites and nonelites using national political surveys conducted during the sixties and early seventies. They note that a sharp increase in attitude constraint for the American electorate occurred in 1964 and continued through 1972, setting attitude constraint at a level roughly comparable to that of the elites in the late 1950s. Some increase in the use of ideological principles also occurred at the same time, although the shift was considerably less dramatic (Field and Anderson, 1969).

---

[1] These statistics are tau gammas rather than the more familiar product moment correlations.

Luttbeg (1968) collected cross-sectional data from samples of political leaders in Oregon, and of the public at large in that same state, in 1959. Instead of comparing average correlations between attitude items from the two samples, Luttbeg separately factor analyzed the two correlation matrices. Converse's (1964) research suggests that a "simpler" factor structure should be found for the elites than for the nonelites. In other words, in the analysis of the elite sample, one or a very few factors should explain most of the common or shared variance in the attitude items, while in the nonelite sample many more factors should be required to explain an equal amount of variance. In fact, if (as Converse argues) there tends to be a single abstract ideological continuum that is the basis for attitude positions among elites, then we should expect that most of the common variance in responses should be explained by a single factor. Luttbeg (1968) finds that this is far from the case. While the factor structure for the elite's correlation matrix was somewhat simpler than for the matrix from the nonelite sample, there emerged five meaningful and substantial factors in both samples. Thus, Luttbeg's study supports the notion of slightly higher constraint among elites than among nonelites; however, it suggests that, even for elites, there is no single ideological continuum that serves as a base for all of the local attitude issues examined in the study.

Bishop et al., (1980) have examined constraint and stability in two small samples which differ in educational background. The attitudes of both samples on seven national issues were measured at two points in time, separated by an average of ten months. While the two samples differed in the degree of constraint between items, thus supporting Converse's (1964) conclusion, both samples showed quite high over-time stability in their attitudes. Like most other studies, correlations between items and over time were examined. The abstract ideological base of the attitudes was not examined, either via coding open-ended questions or by factor analyzing the correlation matrices.

Most recently, Converse and Markus (1979) have argued that panel data collected during the years 1972–1974–1976 by the University of Michigan show almost identical stability correlations as the original 1956–1960 panel survey. They make no mention of attitude constraint, however, in these data; nor do they engage in elite vs. nonelite comparisons.

As is apparent from this brief review, Converse's (1964) original hypotheses have been supported by some studies and disconfirmed by others. There has been surprisingly little consensus among those who have sought to replicate or criticize Converse's work.

Problems in the Traditional Assessment of Constraint and Ideological Basis of Attitudes

In the research on attitude structure in elite and nonelite samples there are three primary analytic problems which may be responsible, in part, for the inconclusive results. Our discussion of these problems borrows notions from sociologists who have been concerned with models of reliability and stability in panel data (e.g., Wheaton et al., 1977).

First, the extent to which attitudes reflect a single underlying ideological base has been ambiguously assessed in the past. Converse (1964) and others originally coded open-ended responses for ideological content. It is never clear what their criteria for coding were nor with what reliability the coding was conducted. Further, it is questionable whether the responses to open-ended questions concerning political candidates adequately assess whether responses to close-ended attitude measures are ideologically derived. Certainly it may be possible to register attitudes that are idelogically derived and yet evaluate a political candidate solely on the basis of idiosyncratic, nonideological reasons (for example, I dislike him because he's fat and smokes cigars).

As has been suggested, the assessment of an ideological base in attitude responses is probably best conducted by examining the structure of matrices of correlations between attitude items through factor analysis. If there is a single liberal-conservative ideological dimension that is reflected in all specific attitudes for some sample, then we would expect correlations among those attitudes to exhibit a single common-factor structure. Happily, tests for the number of common factors in a correlation matrix have recently been developed which permit us to examine the common ideological base of a set of attitude responses. These procedures use maximum likelihood factor analysis models in a confirmatory, rather than exploratory, manner (Jöreskog and Sörbom, 1978; Kenny, 1979).

The second major problem in the assessment of attitude structure and its comparison between elite and nonelite samples concerns the well-known fact that correlations between variables are affected by the variances of the correlated variables (Blalock, 1967). Therefore, comparisons between samples of correlations may well be affected by differences within the samples in the variances of the variable. Barton and Parsons (1977) and Mueller and Judd (1979) have discussed this problem in the comparison of attitude structures at some length. The problem arises from the fact that a correlation between two variables is equivalent to the covariance between the two variables in standardized form (transformed so their means equal zero with unit standard deviations). If the standard deviations in two groups on the same variables differ, then the correlations may differ even if unstandardized coefficients between the measures are equal. The solution to this dilemma, if all items are measured on the same scales—and, hence, have the same metric—is to compare unstandardized or structural coefficients rather than correlations (Duncan, 1975; Kenny, 1979). Unfortunately, this solution is more difficult when dealing with factor analysis where the variances of underlying factors are unobserved. However, assumptions can be made about the metrics of factors which permit the estimation of unstandardized coefficients, as we will explain.

The final problem in the assessment of attitude structure concerns the fact that at least two sources of attitude stability are confounded in its traditional test-retest correlation. We might conceive of variation in attitude responses to any item, at any point in time, being caused by at least three different constructs or factors. First, to some extent, variance in responses to a given attitude question may be due to variation in the underlying ideological continuum which explains variance in all attitude responses at any one point in time. In factor analytic terms, there exist one or more common factors which explain item variance at any one point in time. In addition, there may exist variation in a given attitude at one point in time that is *not* associated with variation in other attitudes at that point, but that *is* associated with variation in the same attitude at another point in time. In other words, there may exist variation in response to an item that is not caused by any underlying general ideological factor but that, nevertheless, shows over-time stability. Finally, variation in response to an item may be, in part, made up of random error and other components that are idiosyncratic to that particular attitude at that point in time. This portion of variation is correlated neither with other attitude items at the same time nor with the same attitude item at different points in time.

These three components of variance in an attitude item can be expressed in the equation,

$$X_{ij} = \sum_{k} \alpha_{ijk} T_{jk} + \beta_{ij} I_i + U_{ij}, \qquad (1)$$

where X_{ij} refers to a response on the i^{th} attitude item at time j; T_{jk} is the set of k common factors which explains covariance between items at any one point in time (in the case of a single underlying ideological construct, k = 1); I_i is an item-specific factor which explains covariance between the same items at various points in time; U_{ij} is disturbance or residual variation in X_{ij} not associated with T_{jk} or I_i; and α_{ijk} and β_{ij} are structural coefficients (or factor loadings in the language of factor analysis). In this model we are assuming that T_{jk}, I_i, and U_{ij} are all uncorrelated with each other and that the expected values of X_{ij}, T_{jk}, I_i, and U_{ij} are all zero. It is further assumed that the item-specific factor, I_i, for any given attitude question is uncorrelated with the item-specific factors of other attitude questions.

This equation, which specifies the components of variation in any observed attitude response (X_{ij}), gives rise to two sorts of attitude stability. First of all, there may be more or less stability in the underlying time-specific common factors (T_{jk}). Second, there may also be stability in the item-specific components of variance (I_i). Converse (1964) and others have recognized that teasing apart these two bases of stability is important, although they have been unable to do so adequately. The former represents stability in general ideological position, regardless of issue; the latter is issue-specific stability, independent of general ideological stance.

Stability in the common time-specific factors can be determined by examining either the correlations or the structural coefficients between the T_{jk}'s at various points in time (j = 1, 2, and 3). As we have already seen, comparisons between populations are more properly conducted using structural coefficients than using correlations. Thus, to assess stability in the time-specific common factors, we need to posit a causal or structural model among them and estimate the coefficients of that model. The structural model we assume among the T_{jk} is the fully recursive model described by the following structural equations:

$$T_{2k} = \gamma_{21} T_{1k} + V_{2k}, \text{ and} \quad (2)$$

$$T_{3k} = \gamma_{31} T_{1k} + \gamma_{32} T_{2k} = V_{2k}, \quad (3)$$

where the T_{jk}'s are as previously defined, γ_{ij} is a set of structural coefficients, and V_{jk} is the disturbance or residual variation in the endogenous construct of interest. Thus, we assume that the common underlying ideology at any one point in time is caused by that same ideology or factor at prior points in time.

The second sort of stability, that concerning the stability of the item-specific component of variance in any X_{ij}, is examined by inspection of the I_i structural coefficients (β_{ij}) in Equation 1.

An example of the combined measurement model (Equation 1) and the structural model among the latent constructs or factors (Equations 2 and 3) is depicted in Figure 1. In this figure, we assume that the two attitudes, X_{ij}, are measured at three points in time. We also assume that there is only one common ideological factor at any point in time (k = 1).

The Present Study

Using national survey data from the 1972–1974–1976 panel study conducted by the Institute for Social Research at the University of Michigan, we attempt in the present study to examine attitude constraint, stability, and ideological base in highly educated and less educated populations, while overcoming the three analytic problems discussed above. We use the same data as those reported by Converse and Markus (1979). At each of the three years, all respondents answered a set of five attitude questions. A model similar to that presented in Figure 1 (with i=5, j=3, and k=1) is developed for each sample, and comparisons are made between the samples.

To estimate the coefficients of the model and to test formally differences between the populations in the coefficients of the model, the computer program LISREL (Jöreskog and Sörbom, 1978: Version 4) is employed. LISREL is a general procedure for estimating the unknown parameters of structural equation models. It permits us to constrain certain parameters (both within and between samples), allows others to freely converge to maximum likelihood estimates, and provides a goodness-of-fit test between the model and the sample variance/covariance matrices of the variables (X_{ij}'s).

METHODS

Samples

The data we report on were gathered by the Center for Political Studies at the University of Michigan. They were collected from a national panel sample interviewed

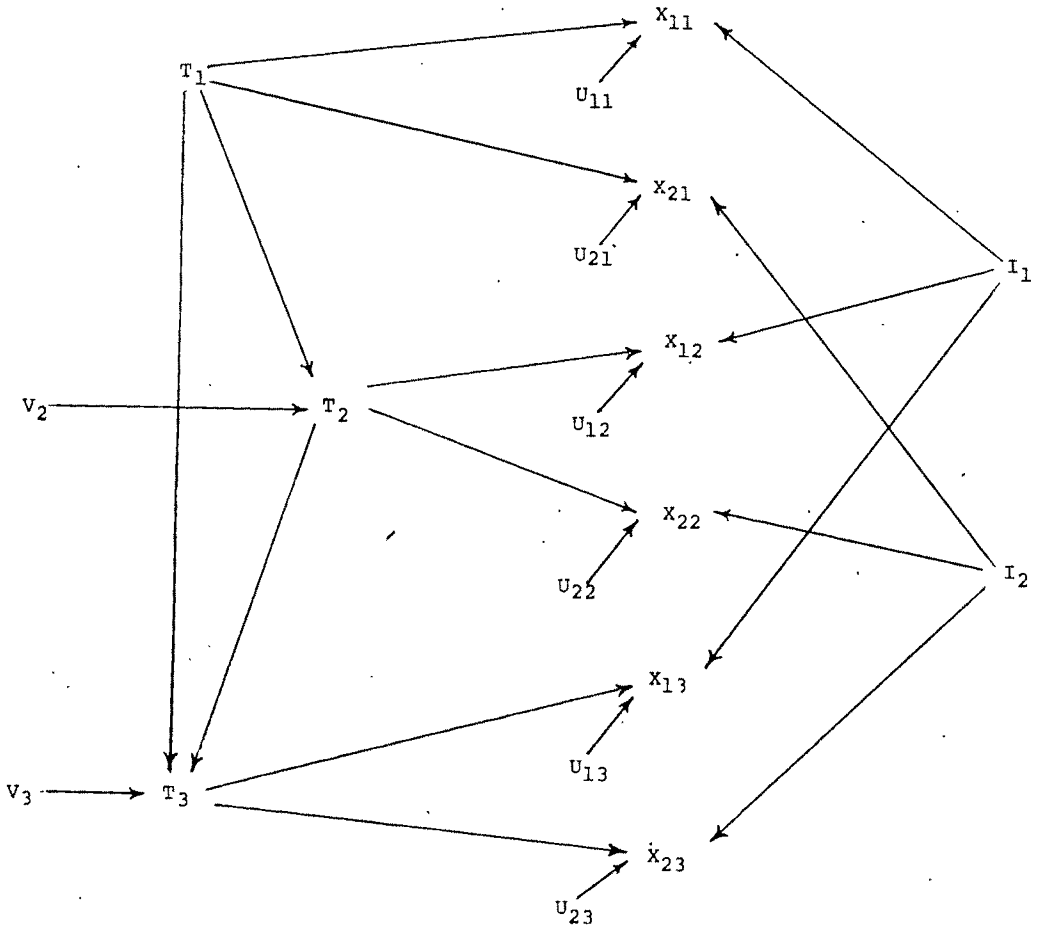


Figure 1. Structural Model of Two Attitudes (X_{1i}, X_{2i}) Measured at Three Points in Time

during the election years 1972, 1974, and 1976. Respondents were interviewed before and after the elections in each of these years.

The full sample in the three-wave panel survey consisted of 1,320 respondents. Of these, 464 respondents had complete data on all attitude questions which were used for our analysis. Because of the substantial number of cases (856) which were omitted from the analysis due to missing data, generalization from our results should be tentative.

In order to form elite and nonelite comparison groups, the sample of 464 respondents was broken down by a number of education and political-involvement variables. In the literature that we have reviewed on attitude belief-systems, there seems to be some confusion over the variables which should be used to discriminate between elites and nonelites. Conceptually, there is agreement that elites are relatively well educated and politically articulate; operationally, many different variables have been used to identify them. For our purposes, it seems reasonable to use educational level as a way of differentiating between elites and nonelites. Emphasizing education as the discriminating variable is consistent with both prior theory (Converse, 1964) and research (Bishop et al., 1980; Stimson, 1975). As Bishop et al. point out, it seems reasonable to believe that education should affect attitude constraint and stability because higher education is commonly seen as encouraging people to relate issues to each other and to be consistent in their thinking.

Of our 464 respondents, 203 (44%) had not attended college, 118 (25%) had attended some college but less than four years, and 143 (31%) respondents had attended four years of college or more. For purposes of analysis, the last group was defined as our elite sample, the first as our nonelite sample. Respondents who had attended some college but less than four years were not included in either group in order to clearly discriminate between our elite and nonelite samples.

Measures

Respondents' attitudes on five different issues were collected at each of the three waves of the panel survey and were used in our analysis. The five questions, all answered on seven-point self-rating scales, are as follows:

(1) Some people think achieving racial integration of schools is so important that it justifies busing children to schools out of their own neighborhood. Others think letting children go to their neighborhood schools is so important that they oppose busing. Where would you place yourself on this scale (1 = Bus to achieve integration; 7 = Keep children in neighborhood schools)?

(2) Some people are primarily concerned with doing everything possible to protect the legal rights of those accused of committing crimes. Others feel that it is more important to stop criminal activity even at the risk of reducing the rights of the accused. Where would you place yourself on this scale (1 = Protect rights of accused; 7 = Stop crime regardless of rights of accused)?

(3) Some people feel that the government in Washington should make every possible effort to improve the social and economic position of blacks and other minority groups. Others feel that the government should not make any special effort to help minorities because they should help themselves. Where would you place yourself on this scale (1 = Government should help minority groups; 7 = Minority groups should help themselves)?

(4) Some people feel that the government in Washington should see to it that every person has a job and a good standard of living. Others think that the government should just let each person get ahead on his own. Where would you place yourself on this scale (1 = Government see to job and good standard of living; 7 = Government let each person get ahead on his own)?

(5) We hear a lot of talk these days about liberals and conservatives. Here is a seven-point scale on which the political views that people might hold are arranged from extremely liberal to extremely conservative. Where would you place yourself on this scale (1 = Extremely liberal; 7 = Extremely conservative)?

As is apparent, the last question measured attitudes on the general liberal-conservative continuum rather than on a specific issue. As will be discussed below, this measure was used to assign a metric to the common factor. Respondents also gave their attitudes on other issues which were not used in this analysis (for example, Vietnam war, marijuana, national health insurance). These were omitted because they were not asked at all three years of the full sample.[2]

The Model

The model we propose is quite similar to a class of stability models discussed by Wheaton et al. (1977). In this model, variation in attitude responses to each question is assumed to be due to three components:

$$X_{ij} = \sum_{k} \alpha_{ijk} T_{jk} + \beta_{ij} I_i + U_{ij}, \tag{4}$$

where X_{ij} is a response to the i^th^ question at time j; T_{jk} is a time-specific common factor or latent construct that explains covariance between items at time j; I_i is an item-specific factor or construct explaining covariance between the same items across time; and U_{ij} is disturbance or residual variation in X_{ij}. In order to test for a single common factor for the five items at any one point in time, we set k = 1. This model differs from those of Wheaton et al. (1977) in our specification of an item-specific factor for each item (I_i), as well as an error component (U_{ij}), instead of allowing errors of measurement to be correlated between the same measure at different time points.

---

[2] The issue of women's role in society was asked of all respondents and was included in preliminary analyses. It was omitted from the final model, however, because of very low correlations between it and the other issues, especially in the elite sample. Other studies have also found anomalous results when this issue was included (Bishop et al., 1980).

The above equation describes what is called the measurement model, the structural effects of unmeasured constructs upon the observed variables or indicators. In addition, we assume a structural model among our latent constructs. In this model, all I_i are assumed to be uncorrelated—as are all U_{ij}. Further, both of these sets of constructs are assumed to be uncorrelated either with each other or with T_j.[3] The structural model among the T_j is described by the following equations:

$$T_2 = \gamma_{21} T_1 + V_2, \text{ and} \qquad (5)$$
$$T_3 = \gamma_{31} T_1 + \gamma_{32} T_2 + V_3, \qquad (6)$$

where the gammas are structural coefficients and the V_j are disturbance or residual variances.

Attitude constraint in the two samples is assessed in this model by examining the α_{ij} structural coefficients. Two sorts of attitude stability are assessed by focusing on different structural coefficients: (a) stability in the general ideological position or in the common time-specific factors (T_j) is estimated by the γ_{ij} structural coefficients; and (b) stability in the item-specific components of variance is estimated by the β_{ij} structural coefficients. Finally, as we have said, the use of a single underlying ideological continuum by both groups is examined by allowing only a single time-specific common factor (T_j) at any point in time and testing the quality of fit of the model to the sample data.

Estimation

The unknown parameters of the model were estimated in the two samples simultaneously using the LISREL procedure (Jöreskog and Sörbom, 1978:Version 4). This procedure generates maximum-likelihood parameter estimates under the assumption of multivariate normality. The following unknown parameters were estimated:

(a) the structural coefficients of latent constructs upon the indicators (α_{ij} and β_{ij});

(b) the structural coefficients among the latent constructs (γ_{ij});

(c) the variances of disturbances to the indicators (U_{ij});

(d) the variances of disturbances to the latent endogenous constructs (V_j); and

(e) the variances of all latent constructs.

Estimation of the coefficients of the model is possible only if the model is identified. This means that there must exist sufficient information in the sample variance-covariance matrices to solve for the unknown coefficients. If there is more information in the matrices than needed, a set of overidentifying restrictions exist that can be used to test whether in fact the sample data are consistent with the model that has been hypothesized (Kenny, 1979). An overall goodness-of-fit test (χ^2) is performed by the LISREL procedure which amounts to a simultaneous test of all overidentifying restrictions. If the model is consistent with the sample data, this χ^2 should not reach traditional significance levels.

In the present case, we want to test the quality of fit of the model to two samples simultaneously. In such cases, the goodness-of-fit test (χ^2) generated by LISREL tests the hypothesis that the model holds in each population, simultaneously. In addition, it is possible to impose between-sample constraints on parameters of the model and, thereby, to test, for instance, whether structural coefficients are equal in two populations yet allow the variances of both constructs and indicators in the two populations to differ. For our purposes we can define a hierarchy of models that can be examined, these models differing in the extent to which between-sample constraints upon parameters are imposed. At the lowest level, we impose no between-sample constraints upon the model parameters. Thus the χ^2 test at this level tests whether the same structural model explains the observed data in both populations, allowing both the structural coefficients and the variances of unobserved constructs to differ. At the highest level, we might specify that all unknown parameters of the model be identical in the two samples. It is unlikely that such a constrained model would hold—since it is to be expected that the variances of the unmeasured constructs differ between populations, just as the variances of their indicators differ. The next highest level of the hierarchy might be one in which all structural coefficients of the model are constrained to be identical in the two samples. Since all of the estimated structural coefficients (α_{ij}, β_{ij}, and γ_{jj}) are relevant to the issues of either attitude stability or attitude constraint, if such a model fits the sample data the implication would be that both populations exhibit equal attitude stability and attitude constraint. At a somewhat lower level of this hierarchy, we might test models in which the three types of structural coefficients (α_{ij}, β_{ij}, and γ_{ij}) are constrained to be identical, one type at a time. For instance, a model in which all α_{ij} are constrained to be equal between the two samples tests whether the degree of attitude constraint is equal in the two populations. A model in which β_{ij} are constrained equal between samples, but in which other parameters differ, tests whether the issue-specific component of attitude stability is equal between populations. Likewise, a model in which γ_{ij} are constrained equal between samples tests for equality of ideological stability. There are many other levels in this hierarchy that we will not detail (for example, putting between-sample constraints upon some of the α_{ij} but not upon them all).

---

[3] While we must assume that U_{ij} is uncorrelated with T_j to estimate the coefficients of the model, we have constrained I_i to be uncorrelated with T_j on substantive grounds rather than because of requirements of the estimation procedure.

Differences in the quality of fit between two successive levels of this hierarchy can be tested by comparing the two χ^2 statistics that result from the estimated parameters. The difference between these two χ^2's is itself a χ^2 and can be used to determine whether the model in which fewer between-sample constraints are made fits the observed data significantly better than the more constrained solution.

We have earlier presented the rationale for avoiding between-sample comparisons of measures of association between standardized variables. Thus, we wish to estimate the structural coefficients of the model using the "raw" or untransformed metrics of the variables. To estimate these coefficients, we must fix the metrics of the unmeasured constructs. If we do not do so we will be unable to estimate their variances (Kenny, 1979). The metrics of latent constructs are fixed by constraining one structural coefficient between each construct and an observed variable at unity. Because we assume that the time-specific common factors (T_j) represent a general ideological predisposition, the structural coefficients or loadings (α_{ij}) of the three liberal-conservative questions on these constructs were constrained at unity for each year.[4] For the item-specific constructs (I_i) the structural coefficient of the 1972 question for each respective construct was constrained at unity.

RESULTS

Table 1 presents the means, standard deviations, and correlations among all fifteen variables for the elite group. Table 2 presents the same data from the nonelite sample. On all five issues, the nonelite group tends to have more conservative attitudes than the elite. Comparing the standard deviations across samples, the elites have more varied responses on the busing and general liberal/conservative attitudes, but less varied on the other three.

When we use the traditional assessments of constraint and stability to compare attitude structures in the two samples, results consistent with Converse's emerge. The average correlations between different items within the same year is .47 for the elite sample and .31 for the nonelite sample. Thus, the elites seem more constrained than the nonelites even though these latter show higher constraint than Converse's data from 1958, consistent with the findings of Nie et al. (1976). Assessing stability, the average within-item, between-years correlation for the elite sample is .64, while for the nonelites it is .51. Again, Converse's hypothesis

---

[4] In fact, the choice of which variable we use to set the metric of a latent construct is arbitrary so long as it is the same metric for all groups on all occasions.

Table 1. Elite Sample: Means, Standard Deviations, and Correlations (N = 143)

			Correlations																	
			$\bar{X}_{\cdot ij}$	σ	X_{11}	X_{12}	X_{13}	X_{21}	X_{22}	X_{23}	X_{31}	X_{32}	X_{33}	X_{41}	X_{42}	X_{43}	X_{51}	X_{52}
Busing	(X_{1j})	1972	5.49	2.03														
		1974	5.50	1.76	.79													
		1976	5.55	1.74	.71	.78												
Accused Rights	(X_{2j})	1972	3.44	1.84	.43	.43	.37											
		1974	3.76	1.68	.39	.46	.44	.54										
		1976	3.78	1.83	.27	.35	.34	.53	.60									
Minorities	(X_{3j})	1972	3.53	1.60	.54	.53	.51	.39	.36	.18								
		1974	3.87	1.46	.57	.61	.56	.45	.37	.31	.58							
		1976	3.79	1.57	.51	.52	.54	.40	.35	.30	.60	.64						
Guaranteed Job	(X_{4j})	1972	4.31	1.67	.47	.48	.49	.29	.38	.18	.56	.51	.51					
		1974	4.46	1.48	.50	.56	.59	.28	.35	.20	.58	.62	.52	.56				
		1976	4.47	1.54	.47	.48	.53	.29	.32	.28	.50	.57	.58	.49	.61			
Liberal/Conservative	(X_{5j})	1972	3.87	1.41	.52	.60	.49	.47	.36	.28	.54	.49	.45	.53	.51	.41		
		1974	3.97	1.38	.57	.63	.54	.41	.39	.26	.50	.54	.41	.50	.58	.49	.76	
		1976	4.16	1.30	.51	.56	.52	.32	.33	.21	.45	.49	.39	.49	.54	.55	.67	.75

seems to be supported, although more stability characterizes the nonelites' attitudes than in 1958. We turn now to our structural equation model to more properly test Converse's hypothesis.

Starting at the lowest level of the hierarchy of models discussed in the Methods section—the level at which no between-sample constraints upon the model's parameters are imposed—the obtained parameter estimates are presented in Figures 2 and 3 and Tables 3 and 4. The figures present the structural coefficients of variables on the time-specific common factors (α_{ij}), the structural coefficients between these factors or constructs (γ_{jj}), and their disturbances. Figure 2 applies to the highly educated or elite sample. Figure 3 presents the parameters for the nonelite sample.

Table 3 presents (for each sample) the loading coefficients of variables on the item-specific factors (β_{ij}) and the disturbance variance for each variable (U_{ij}).

Table 2. Nonelite Sample: Means, Standard Deviations, and Correlations (N = 203)

			Correlations																	
			$\bar{X}_{ij}$	σ	X_{11}	X_{12}	X_{13}	X_{21}	X_{22}	X_{23}	X_{31}	X_{32}	X_{33}	X_{41}	X_{42}	X_{43}	X_{51}	X_{52}
Busing	(X_{1j})	1972	6.46	1.25														
		1974	6.33	1.31	.44													
		1976	6.27	1.34	.54	.58												
Accused Rights	(X_{2j})	1972	4.18	2.11	.24	.22	.21											
		1974	4.29	1.97	.20	.25	.25	.53										
		1976	4.46	2.00	.14	.13	.17	.40	.44									
Minorities	(X_{3j})	1972	4.15	1.85	.37	.31	.32	.28	.24	.21								
		1974	4.67	1.79	.25	.34	.32	.32	.38	.29	.50							
		1976	4.71	1.64	.29	.43	.38	.27	.23	.21	.47	.56						
Guaranteed Job	(X_{4j})	1972	4.55	1.90	.39	.22	.22	.25	.16	.13	.44	.34	.29					
		1974	4.69	1.82	.31	.30	.21	.21	.21	.23	.29	.41	.39	.52				
		1976	4.61	1.79	.30	.33	.28	.25	.16	.14	.36	.38	.44	.48	.41			
Liberal/Conservative	(X_{5j})	1972	4.38	1.23	.30	.34	.32	.32	.19	.13	.34	.35	.34	.34	.36	.36		
		1974	4.18	1.25	.21	.33	.25	.25	.19	.12	.17	.31	.26	.31	.28	.38	.62	
		1976	4.39	1.40	.35	.35	.32	.30	.20	.25	.29	.28	.40	.45	.41	.49	.57	.55

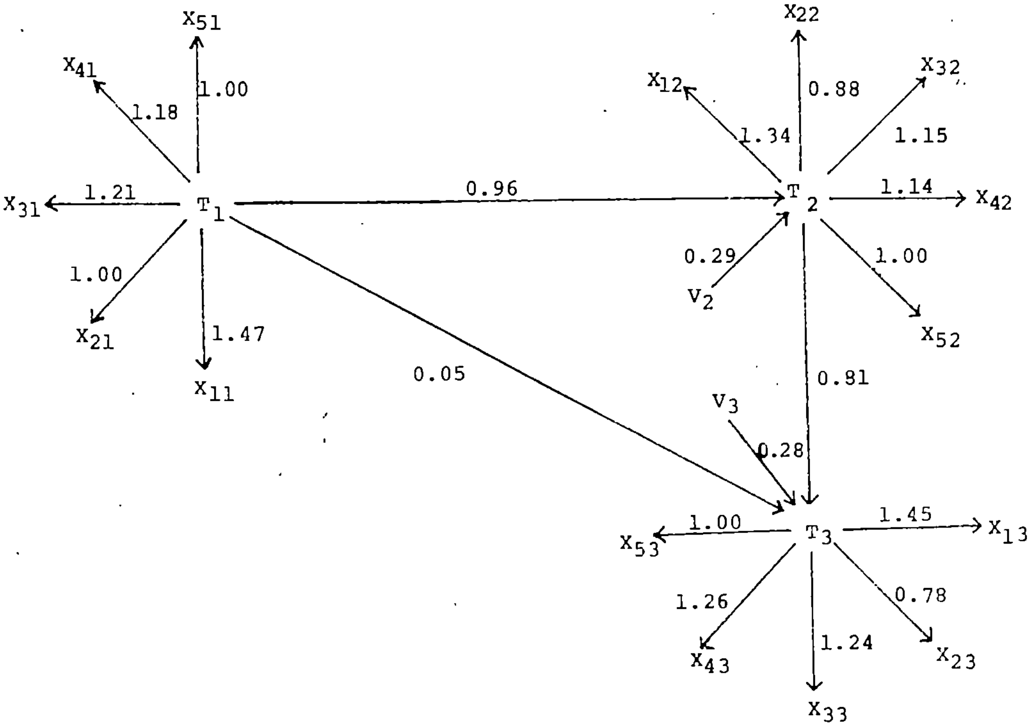


Figure 2. Maximum-Likelihood Parameter Estimates; Elite Sample; No Between-Sample Constraints

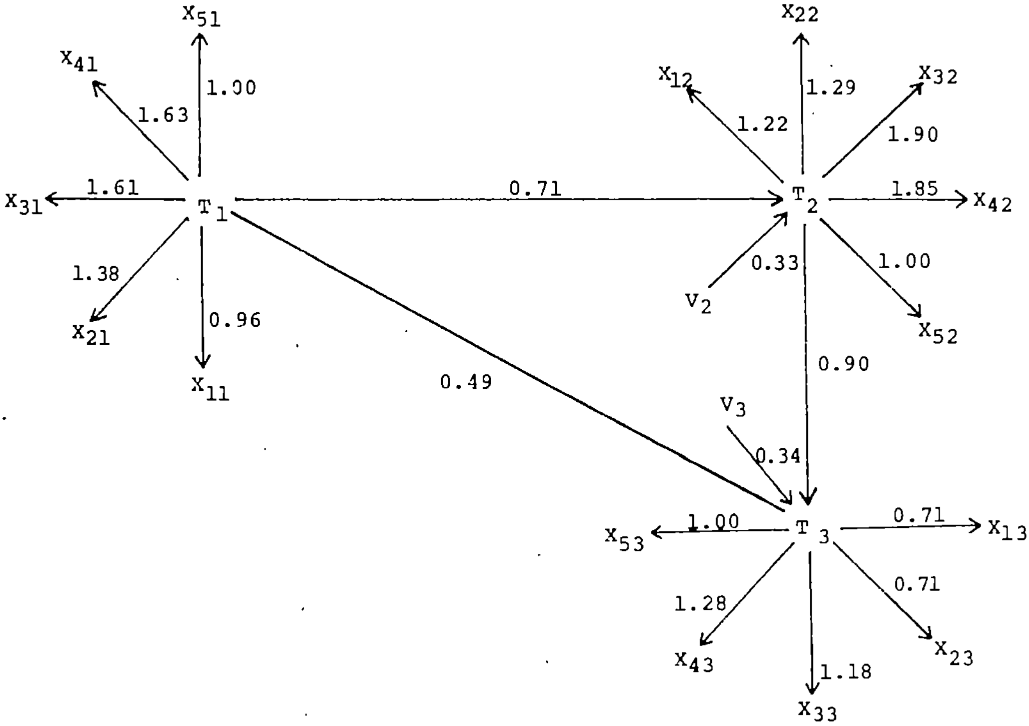


Figure 3. Maximum-Likelihood Parameter Estimates; Nonelite Sample; No Between-Sample Constraints

Table 3. Maximum-Likelihood Parameter Estimates; Elite and Nonelite Samples; No Between-Sample Constraints

		Coefficients (β_{ij}) of Item-Specific Factors (I_j) on Variables		Disturbance Variances (U_{ij})	
		Elite	Nonelite	Elite	Nonelite
1972	X_{11}	1.00	1.00	1.03	0.81
	X_{21}	1.00	1.00	1.59	2.10
	X_{31}	1.00	1.00	1.02	1.67
	X_{41}	1.00	1.00	1.37	−0.55*
	X_{51}	1.00	1.00	0.55	0.54
1974	X_{12}	0.98	1.13	0.40	0.82
	X_{22}	1.05	1.10	1.15	1.54
	X_{32}	1.00	1.39	0.78	1.07
	X_{42}	2.29	0.24	0.60	1.90
	X_{52}	1.22	1.28	0.26	0.45
1976	X_{13}	0.76	1.89	0.77	0.41
	X_{23}	1.48	0.81	1.05	2.65
	X_{33}	5.21	0.90	0.00	1.17
	X_{43}	1.36	0.16	0.96	1.73
	X_{53}	0.98	0.80	0.50	0.82

* This negative disturbance variance probably indicates some misspecification in the nonelite model. However, the quality of fit of the model as a whole leads us to believe that this misspecification is slight.

Table 4 presents the estimated variances of all factors or latent constructs for each sample.

The structural model we have hypothesized with no between-sample constraints simultaneously fits the observed variance-covariance matrices of the two samples quite well, χ^2 (144) = 165.61; p = .105. Separate tests of the model, on each sample alone, confirm that the observed matrix does not deviate significantly from the model's predictions in either case (for the elite sample: χ^2 (72) = 80.07; p = .241; for the nonelite sample: χ^2 (72) = 85.54; p = .150). Thus, it appears that, for both samples, the attitude items at any one point in time exhibit a single common factor structure. Regardless of the degree of attitude constraint or stability in the samples, a single underlying attitude construct or ideology does an adequate job in explaining all of the covariance between specific attitude items at any given point in time. The quality of fit of the model therefore leads us to reject Converse's hypothesis that there is no single underlying ideological continuum that is reflected in the attitude responses of nonelite members of the electorate.

The degree of attitude constraint in the two samples is assessed by the relative magnitude of the α_{ij} coefficients, the structural coefficients of the time-specific constructs on the variables. To test for equal constraint in the two populations, every α_{ij} in one sample was constrained to be identical to the corresponding α_{ij} in the other sample. Within any sample the different α_{ij}'s were allowed to vary. Under these between-sample equality constraints, the estimated model inadequately fits the two variance-covariance matrices, χ^2 (156) = 198.87; p = .012. In other words, the departures of the observed data from the estimated model are significant. This conclusion is confirmed by examining the difference between this χ^2 and the goodness of fit of the model with no between-sample constraints: Difference χ^2 (12) = 33.26; $p < .001$. Thus, the model with each α_{ij} constrained to be equal between samples fits significantly less well than the unconstrained model.

While this informs us that the α_{ij} coefficients in the two groups are not identical, it is still possible that the two populations exhibit equal levels of attitude constraint across the five issues. That is, even though we cannot say that all α_{ij} coefficients in one group are equal to their counterparts in the other, it still may be the case that they have α_{ij} coefficients of relatively equal magnitude across all variables. In fact, this turns out to be the case. Examining the α_{ij} coefficients in the two samples from the unconstrained model portrayed in Figures 2 and 3, we see that

Table 4. Maximum-Likelihood Parameter Estimates; Elite and Nonelite Samples; No Between-Sample Constraints

	Variances of Latent Constructs							
	T_1	T_2	T_3	I_1	I_2	I_3	I_4	I_5
Elite Sample	.997	.996	.815	.910	.836	.045	.058	.416
Nonelite Sample	.521	.367	.859	.266	1.411	.437	2.754	.430

in the cases of five of the fifteen variables the coefficients for the elite group are greater than the corresponding coefficients from the nonelite group. The nonelite sample shows larger α_{ij} coefficients in seven of the fifteen cases. In the remaining three cases, the coefficients in both samples have been fixed at unity. The average α_{ij} coefficient for the elite sample is 1.17; for the nonelite sample, the coefficients average 1.31. From these coefficients it is clear that the elite group does not show higher attitude constraint than the nonelite group, even though we are unable to conclude that the structural coefficients relevant to the constraint hypothesis are identical in the two. Rather than the coefficients being consistently larger in one sample than in the other, it appears that differences between the samples concern the extent to which the responses to individual attitude questions are reflective of the underlying ideological construct. Responses to the first question, that concerning the busing issue, are more reflective of an underlying ideology for the elite than for the nonelite sample. On the other hand, the fourth question, concerning a federally guaranteed job, seems to tap the underlying ideology more strongly for the nonelite sample than for the elite.

As we argued earlier, there are two types of attitude stability to be assessed: stability in the underlying ideological construct (T_j) and stability in item-specific variance. The latter sort of stability is assessed by the β_{ij} coefficients, the loading coefficients of variables on the item-specific factors, I_i. The first step in testing this item-specific stability is to test a model in which each β_{ij} in one sample is constrained to be identical to the corresponding coefficient in the other sample. The parameters of the resulting model are found not to fit the observed variance-covariance matrices, χ^2 (154) = 188.7; $p = .030$. This model, with between-sample equality constraints upon β_{ij}, accounts for the observed data significantly less well than the model containing no between-sample constraints, difference χ^2 (10) = 23.10; $p = .01$. Thus, we must conclude that the coefficients that indicate the level of stability in item-specific variance are not equivalent in the two populations.

As we argued when discussing the α_{ij} coefficients and the attitude constraint issue, it may be that the two groups exhibit roughly equal item-specific stability even though we know that individual β_{ij} coefficients are not identical. If we ignore those β_{ij} coefficients that are fixed at unity to set metrics for the I_i constructs (all of the 1972 coefficients), we find in the unconstrained model that in five of the ten cases the elite sample shows higher β_{ij} coefficients, and in the other five the nonelite sample shows higher coefficients. Thus, it appears, once again, that while we know that the individual β_{ij} coefficients in the two samples differ, it is not true that these indicators of issue-specific stability are higher in the elite group than in the nonelite group as Converse (1964) would lead us to expect. It is interesting to note that, once again, the busing and guaranteed job issues are the two that consistently show between-sample differences in the degree of item-specific stability. For the elite sample, responses to the guaranteed job question exhibit higher item-specific stability than for the nonelite, while for the busing issue this finding is reversed. This pattern complements that which we found for attitude constraint. Those issues that for one sample reflect ideology less well show more item-specific stability for that sample than for the other.

The other sort of stability to be assessed in the two groups is the stability of the latent time-specific constructs (T_j), the underlying ideology. This ideological stability is operationalized by the γ_{jj} structural coefficients in the model. Just as we did with the α_{ij} and β_{ij} coefficients, we reestimated the parameters of the model under the specification that individual γ_{jj} coefficients be equal across the two samples. Just as we found for the between-sample constraints on the other structural coefficients, the respecified model with between-sample constraints on the γ_{jj} coefficients does not adequately account for the observed data, χ^2 (147) = 178.13; $p = .041$. This respecified model does a significantly poorer job than the model with no between-sample constraints in explaining the observed data, difference χ^2 (3) = 12.52; $p < .01$.

Knowing that the individual γ_{ij} coefficients in the two populations are not identical, we can examine their magnitude in the original model to compare the stability of the latent ideological construct in the two groups. In Figures 2 and 3 it can be seen that γ_{21}, the effect of T_1 on T_2, is larger in the elite than in the nonelite sample. On the other hand, γ_{32}, the effect of T_2 on T_3, is larger in the nonelite than in the elite sample. Finally, γ_{31}, the direct or unmediated effect of T_1 on T_3, is substantially larger in the nonelite than in the elite sample. While we must conclude that the ideological stability in the two samples is not identical, it certainly is not the case, as Converse (1964) would have us believe, that substantially higher stability is obtained among elite than among nonelite respondents.

The major difference between the two samples in the magnitude of the γ_{ij} coefficients lies in the values of the γ_{31} coefficients. The other two coefficients, γ_{21} and γ_{32}, seem to be quite substantial in both samples. On the other hand, in the elite sample the effect of T_1 on T_3 seems to be nearly totally mediated by T_2, while in the nonelite sample there exists a substantial unmediated effect of T_1 on T_3. To test whether, in fact, the difference in γ_{31} is the only difference in these ideological stability coefficients, the model was respecified to include between-sample equality constraints on both γ_{21} and γ_{32} but not upon γ_{31}. The resulting model does nearly as good a job in accounting for the observed data as the original model in which there are no between-sample constraints, χ^2 $(146) = 168.76; p = .096$. In fact, the quality of fit of the respecified model is not significantly poorer than the unconstrained model, difference $\chi^2 (2) = 3.15; p > .20$. In this respecified model, γ_{21} in the two samples equals .88, γ_{32} equals .82 in both samples, and γ_{31} equals .06 in the elite sample and .53 in the nonelite sample. Given that this respecified model accounts for the observed data as well as the original model does, and given that the difference in the γ_{31} coefficients in the two samples is substantially more than twice their pooled standard error (.18), it would seem that the nonelite population shows more long-term stability in its underlying ideology than the elite population. Interestingly, this difference in stability, running in the opposite direction from Converse's hypothesis, is due to the presence of an unmediated long-term effect of 1972 attitudes on 1976 responses in the nonelite group. For the elite group, the long-term effects of prior attitudes are totally mediated by the intervening attitudes.

One of the strengths of the present structural equation model for the study of attitude stability is that we are able to assess coefficients that are not attenuated by irrelevant sources of variance in the observed variables. In other words, in assessing the stability of the underlying ideological construct we have been able to remove extraneous variance (both random and systematic) from the indicators of that construct. Even though between-sample comparisons of standardized measures of association should not be made, it is informative to present the correlations between the latent ideological constructs in the two samples to illustrate how our usual measures of attitude stability, based on single indicators, are seriously attenuated. Table 5 presents the estimated correlations between the latent ideological constructs (T_j) from the model in which no between-sample constraints are made. The remarkable thing about these correlations is their magnitude. Across both samples, the correlations between the underlying ideology average .914 for two-year intervals. For the four-year interval, 1972 to 1976, the average correlation between the underlying ideology is .894. As we have seen from the structural coefficients, it is not the case that the nonelite group shows less attitude stability than the elite group. What is remarkable in this model is the degree to which both groups show extremely high attitude stability.

Table 5. Correlations Among Latent Time-Specific Constructs (T_j)

	Elite Sample	Nonelite Sample
ρ_{12}	.956	.840
ρ_{23}	.951	.908
ρ_{13}	.914	.875

DISCUSSION

The conclusions we have reached differ from those of Converse and his colleagues when they have compared elite and nonelite attitude systems. In fact, they differ from what our own conclusions would have been had we used the traditional analytic procedures (comparing average correlations). On the other hand, some of our conclusions are consistent with those reached by researchers critical of the Converse hypothesis (most notably, Achen, 1975). From our structural equation model, the following conclusions emerge:

(a) The attitude systems of both groups show a single common-factor structure at any one point in time. Thus, the data from both groups are consistent with the notion that there exists a single underlying ideological predisposition that is reflected in these specific attitude responses. This single underlying predisposition adequately explains all of the covariance between responses at any one point in time.

(b) There is no evidence of greater attitude constraint in the elite than in the nonelite sample when the relevant structural coefficients are compared. While the average magnitude of these coefficients does not differ in the two groups, there are differences between them in the degree to which responses to specific issues are ideologically derived.

(c) When comparisons are made between samples in the stability of issue-specific components of variance, there is no evidence of greater stability in the elite than in the nonelite group. While there is no difference in the average magnitude of the structural coefficients that indicate issue-specific stability, there are differences between the groups in the extent to which responses to different issues show issue-specific variance.

(d) Both groups show remarkable stability in their underlying ideological predisposition. When we compare the relevant structural coefficients, there is no evidence that the elite sample shows higher ideological stability than the nonelite sample. In fact, the only significant difference between groups in these coefficients is the presence of an unmediated or direct effect of 1972 ideology on 1976 ideology for the nonelite. The elite group shows no such effect.

In the introduction to this paper we discussed three methodological shortcomings in the traditional comparisons of attitude systems between elites and nonelites. Our solutions to these three problems are responsible, we believe, for the differences between our conclusions and those arrived at by the more traditional comparison methods.

Our first conclusion, that a single underlying ideological predisposition explains attitude covariance in both samples, differs from earlier researchers because we have explicitly tested the single common-factor hypothesis in each population. Other researchers have examined ideological predispositions by coding responses to open-ended questions concerning political candidates. It is a long inferential leap from the results of such coding to the conclusion that nonelites show no underlying ideology in their answers to specific attitude questions. Testing for a single common-factor structure in the covariances between such questions permits a much more direct assessment of the ideological basis of attitudes.

As we mentioned previously, a few other researchers have examined the factor structure of a set of attitude items to determine whether an underlying ideological dimension is used. Luttbeg (1968), for instance, concludes that neither elites nor nonelites show an underlying ideological dimension in answering the ten attitude questions that he analyzed. Rather, both samples showed evidence of five underlying factors. We suspect that the difference between our single factor solution and Luttbeg's multiple factor solution is due to the specific attitude questions examined. Luttbeg's questions concerned a variety of local issues that might not be expected to tap an underlying liberal-conservative ideological dimension (for example, whether a metropolitan park should be built; whether new industry should be attracted). The questions we examined, on the other hand, might be expected to relate much more strongly to an underlying liberal-conservative stance. Thus, we reach the not altogether surprising conclusion that the presence of an underlying ideological predisposition in answering attitude questions depends upon the content of those questions more than it depends upon respondents. In fact, Luttbeg agrees with us that no greater ideological use is shown by elites than by nonelites.

The second conclusion we have reached, concerning the equality of attitude constraint in our two samples, differs from previous research because structural coefficients were compared between the samples, rather than measures of association between standardized variables. To assure ourselves of this fact, the structural model we hypothesized was estimated with the variances of variables and unmeasured constructs fixed at unity. In this standardized solution, the factor loadings of variables upon the time-specific ideological constructs were found, on average, to be larger in the elite sample than in the nonelite. Thus, inappropriate comparisons of standardized measures of association have led, we believe, to the erroneous conclusion of greater attitude constraint in elite samples.

While there is no evidence of greater attitude constraint in one group than the other, we were unable to conclude that the relevant structural coefficients were equal in the two groups. This led us to the conclusion that individual issues seem to be ideologically based to differing degrees in the two samples. Thus the issue of busing seems to reflect underlying ideology more for the elite than for the nonelite, while a federally guaranteed job is a more ideological issue for the nonelite sample.

The two conclusions just reviewed, concerning the presence and the use of an underlying ideology, are clearly interrelated. Both argue that there is little difference in the use of ideology, across issues, between relatively educated and relatively uneducated respondents. Thus, as we argued above, variation in the use of an underlying ideology does not seem to depend upon respondents. The difference between our single common factor structure and Luttbeg's (1968) more complex structure suggests that different issues may affect the use of an underlying ideology. In addition, our evidence supports the notion that respondents and issues may interact in explaining variation in the use of ideology. While some issues are more likely to reflect an ideological construct than others, regardless of respondents, it is also true that there are systematic differences between respondents in the extent to which an underlying ideology is used to respond to a question on a *specific* attitude issue. Across issues, we may all, on average, be equivalent in the use of ideology; for any specific issue, we may vary in the extent to which we see it ideologically.

Our conclusions concerning attitude stability in the two groups are discrepant with some earlier conclusions (e.g., Converse, 1964) and consistent with others (e.g., Bishop et al., 1980). The degree of stability, however, particularly in the underlying ideological predisposition, is markedly higher than the stability shown by other researchers. This difference is due, we believe, to our success in separating ideological stability from item-specific stability. The common ideological factors which show such high stability are measured free both from item-specific variance and from random errors of measurement.

In addition to the magnitude of our stability estimates in both groups, the other interesting conclusion concerning attitude stability is the difference between samples in the size of the γ_{31} coefficient. That is, for the nonelite sample we found a surprisingly large unmediated effect of the 1972 ideological construct on the 1976 construct, while for the elite sample, the effect of 1972 ideology was nearly totally mediated by the 1974 ideological construct. Although his stability coefficients were much lower than ours, Converse (1964) reports a similar unmediated effect for his nonelite sample. The presence of such an unmediated four-year effect in the nonelite sample is inconsistent with relatively simple Markov chain assumptions concerning change. Converse (1964) has put forward two alternative models to account for these unmediated effects. Both models are based on the assumption that the direct effects over two years are fairly low. Since these two-year stability coefficients are quite high in our data, neither of Converse's models seem appropriate here. We have no simple theory to explain the presence of this unmediated four-year effect in the nonelite sample, other than to suggest the possibility that nonelite members think back to the last presidential election year (1972) in deciding their positions in 1976, while elite members do not.

In conclusion, we believe that our results pose a substantial threat to Converse's original hypothesis that the attitude responses of the public at large are unstable, nearly random responses. We have shown for a set of five attitude items that both the relatively educated and relatively uneducated members of the public show an ideological orientation that is remarkably stable over time. Attitudes of the public at large are both ideologically derived, if the issue is one on which an ideological perspective is useful, and quite consistent. Attitude researchers and policy makers who use public opinion polls should find some comfort in these conclusions.

REFERENCES

Achen, Christopher, H.
1975 "Mass political attitudes and survey response." American Political Science Review 69:1218–31.

Alwin, Duane F.
1973 "Making inferences from attitude-behavior correlations." Sociometry 36:253–78.
1976 "Attitude scales as congeneric tests: a reexamination of an attitude-behavior model." Sociometry 39:377–83.

Barton, Allen H., and R. Wayne Parsons
1977 "Measuring belief system structure." Public Opinion Quarterly 41:159–80.

Bishop, George D., David L. Hamilton, and John B. McConahay
1980 "Attitudes and non-attitudes in the belief systems of mass publics: a field study." Journal of Social Psychology 110:53–64.

Blalock, H. M.
1967 "Path coefficients versus regression coefficients." American Journal of Sociology 72:675–6.

Campbell, Angus, Philip E. Converse, Warren E. Miller, and Donald E. Stokes
1960 The American Voter. New York: Wiley.

Converse, Philip E.
1964 "The nature of belief systems in mass publics." Pp. 75–169 in David E. Apter (ed.), Ideology and Discontent. New York: Free Press.
1970 "Attitudes and non-attitudes: continuation of a dialogue." Pp. 168–89 in E. R. Tufte (ed.), The Quantitative Analysis of Social Problems. Reading, Mass.: Addison-Wesley.
1975 "Public opinion and voting behavior." Pp. 75–169 in F. I. Greenstein and N. W. Polsby (eds.), Nongovernmental Politics, Vol. 4 of the Handbook of Political Science. Reading, Mass.: Addison-Wesley.

Converse, Philip E., and Gregory B. Markus
1979 "Plus ça change . . .: the new CPS election study panel." American Political Science Review 73:32–49.

Duncan, Otis Dudley
1975 Introduction to Structural Equation Models. New York: Academic Press.

Field, John Osgood, and Ronald E. Anderson
1969 "Ideology in the public's conceptualization of the 1964 election." Public Opinion Quarterly 33:380–98.

Jöreskog, Karl G., and Dag Sörbom
1978 LISREL: Analysis of Linear Structural Relationships by the Method of Maximum Likelihood. Chicago: National Educational Resources, Inc.

Kenny, David A.
1979 Correlation and Causality. New York: Wiley.

Luttbeg, Norman R.
1968 "The structure of beliefs among leaders and the public." Public Opinion Quarterly 32:398–409.

Mueller, Carol McClurg, and Charles M. Judd
1979 "Belief constraint and belief consensus: towards an analysis of social movement ideologies." American Sociological Association Annual Meeting, Boston.

Nie, Norman H., and Kristie Anderson
1975 "Mass belief systems revisited: political change and attitude structure." Journal of Politics 36:540–91.

Nie, Norman H., Sidney Verba, and John R. Petrocik
1976 The Changing American Voter. Cambridge: Harvard University Press.

Pierce, John C., and D. D. Rose
1974 "Non-attitudes and American public opinion.' American Political Science Review 68:626–49.

Stimson, James A.
1975 "Belief systems: constraint, complexity, and the 1972 election." American Journal of Political Science 19:393–417.

Weissberg, R.
1976 "Consensual attitudes and attitude structure." Public Opinion Quarterly 40:349–60.

Wheaton, Blair, Bengt Muthen, Duane F. Alwin, and Gene F. Summers
1977 "Assessing reliability and stability in panel models." Pp. 84–136 in D. R. Heise (ed.), Sociological Methodology 1977. San Francisco: Jossey-Bass.

COMMENT*

REJOINDER TO JUDD AND MILBURN

PHILIP E. CONVERSE
University of Michigan

Manuscripts reacting to the Belief Systems paper tackled by Judd and Milburn still cross my desk at the rate of better than a half-dozen a year. Only one in every three or four of these is published. However, whether published or unpublished, the verdict is certainly a mixed one, quite as these authors report; and my guess would be that if anything, disconfirmations are currently in the lead. In fact, I have seen so many disconfirmations in the past decade, and they form such repetitive patterns, that I consider myself something of an expert in the gambits necessary to produce disconfirmation. Invited to comment here, I thought it might be of some service to share these insights so that subsequent investigators can generate disconfirmations more surely and swiftly.

> (1) The first and foremost rule is to avoid data drawn from any genuine, national-level political elites, such as the American congressmen used in the original study. A chancy alternative is to substitute local elites, such as mayors and councilmen in small American cities, or members of Boards of Education almost anywhere. Safer yet is to carve a national cross-section sample into pieces, call the top end a political "elite," and use this different partition to disconfirm the original differences.
>
> (2) The preceding rule is, however, rarely sufficient in itself to ensure disconfirmation. A further precaution involves wise choice of the criterion variable separating a pseudo-elite from the non-elite, within a mass sample. We must remain credible, so, as Judd and Milburn point out, there are only two plausible contenders: education and political involvement. It has been known for many years (cf. Converse, 1975) that, as an empirical matter, education is a poor cousin in making this discrimination, compared with political involvement. Indeed, in the original text at this particular point (Converse, 1964: 229), even without the benefit of relevant data, the prediction was made in terms of "political interest," and not education. Hence, it is a good idea to avoid political interest, and hew to education as a discriminandum instead.

The questing reader has a right to complain at this point. After all, Judd and Milburn have observed not one but both of the above precautions, yet their raw data still confirm the original predictions rather than disconfirming them. How can this be? I myself am somewhat surprised, since I have seen data reported from several studies in which these two precautions alone are quite enough, especially with small N's. But the Pilgrim need not despair, since many other paths to disconfirmation have been mapped. The only problem is that these further paths require more extended explanation.

First, is a historical note. Upon my initial glimpse twenty years ago at the raw correlations between responses to the same political issue items in a national sample after a two-year interval, I was quite devastated. The possibility that all or even much of the observed turnover was true change seemed dim, and my psychometric training led me to regard these continuity correlations much as test/retest reliability coefficients. So construed, they indicated our measurements were extremely vague and unreliable. As I became more familiar with the data, however, a rather different message emerged. There was internal evidence that some respondents had answered some of our questions with very high reliability, while others had responded to the same items with very little reliability. These marked differentials fit very poorly with the conventional view of measurement reliability then prevailing, which was instrument-specific and assumed to be invariant in level over objects measured. I became convinced that these reliability differentials had to do with item-specific differences between respondents in cognitive states that were of high *substantive* interest.

I first described these differentials in item reliability in 1963, making the primary point that reliability had to be seen as a product of an interaction between measuring instrument and certain properties of the object measured (Converse, 1970:177). The paper contains my first "shot at" answering the key question as to the reasons for these reliability differentials. I opined that they stemmed from differences in what different respondents brought to the topic of each item in the way of information and attitude crystallization. When respondents brought a lot to a given subject, reliabilities would be high; for other respondents bringing much less to the subject, reliabilities would be lower.

After the 1963 paper I wanted further tests of the adequacy of these diagnoses. In particular,

---

* Direct all communications to: Philip E. Converse, Institute for Social Research, University of Michigan, Ann Arbor, MI 48104.

I wanted to see if some population that was unquestionably long on information and commitment about such political issues would not respond to these items with much higher reliability than was found in the mass electorate, thereby certifying that the problem lay more with respondent confusion than with measurements that were intrinsically unreliable. The only data set filling this bill that was available to me was the Miller-Stokes interviews with American congressmen. These data were not ideal: the questions were not absolutely identical in wording for the two populations, and there was no panel or longitudinal component permitting a glimpse at anything resembling a test/retest coefficient, so that heightened reliability had to be deduced from the comparative behavior of internal consistency data (both of these practical deficiencies have been remedied in later mass-elite data collections with very gratifying results). Nonetheless, the differences were not only in the expected direction, but quite markedly so. Hence the 1964 paper addressed by Judd and Milburn was born.

I am no more prepared now than I ever have been to opine whether or not the cognitive differences I was discussing have effects beyond those that fall out when differences in actual reliabilities are appropriately isolated. It seems likely that there are further effects, although they are probably complex, and I have never had much doubt that the largest and most robust registration of these cognitive differences is in the differential reliabilities. This being so, the agile reader can spot another popular route to a disconfirmation:

> (3) Take the contrasting samples and correct the data from each for attenuation due to measurement unreliability, either by time-honored means or by the kind of "shucking off" into the measurement model that newer methods like LISREL invite. This almost certainly means that you clean out more measurement noise from the less informed sample than from the more informed one, but there is no need to make any point of that. What is important is that the adjusted data with the reliability differentials corrected away can be used to disconfirm the Converse expectation that such differentials between the samples will be found.

We lack space to develop at any length some of the other paths to disconfirmation that are invented and reinvented in these manuscripts. What many of them have in common is a certain confusion between aspects of the measurement operations I happened to use, and the actual underlying theory I was eager to evaluate. I apologized at the time (Converse, 1964: 238–9) for the marginal appropriateness of the correlational indicators I felt I must resort to. I was painfully aware of their sensitivity to variance differences, and assumed that in analytic situations where variance difficulties arose, such as Barton and Parsons (1977) encountered, investigators would hew to the theory and take intelligent evasive action by shifting to other indicators, just as Barton and Parsons did. But this is not usually so. For many (and particularly the critics) the theory is irretrievably embodied in this particular set of operations. Thus:

> (4) A variety of excuses can be found for tinkering with the variances between the comparative samples, and a shrewd choice of these adjustments can be very helpful in achieving disconfirmation. The most general-purpose excuse is that variance differences are measurement artifacts and substantively irrelevant. Few readers will think about the matter long enough to realize that in various measurement situations the theory itself predicts variance differences that are pregnant with substantive meaning which is destroyed when they are tampered with.

Similarly, it is easy to assume that the theory is nothing more than one monotonous prediction, to wit, that correlational indicators of constraint and stability will be high in well-informed populations and low in poorly informed ones. Actually, while this is indeed the customary expectation, I spent five pages in the original (1964) text discussing one large class of cases where the theory would predict an inversion of such an ordering. I have since thought of another large class of cases where an inversion would be expected. Hence:

> (5) Use the theory to locate an inversion of the normally expected order. Collect or find the relevant data. Assuming that they do indeed show the predicted inversion, publish them as a disconfirmation, since almost nobody thinks of the theory as more than the single prediction.

Perhaps this is enough of an arsenal to keep the disconfirmation mills busy for some time to come, although the reader who prefers to be original should remember that all of these have been used before in manuscript, if not always in print, and most of them a great many times.

By now it should be clear that I do not see the Judd-Milburn paper as a very challenging disconfirmation. Actually, I rather like the paper, once I read beyond the disconfirmation claims and a few of the amazing statements attributed to me. I am unruffled by the alleged disconfirmation, in part, because I know that far more incisive replications than this one can claim to be (see points [1] and [2] above), now completed in countries on three continents, show the predicted differences in a magnitude which completely eclipses the restricted ones appearing here.

I am also unruffled because I do not believe that this paper is a disconfirmation even in its own terms. That is, my minimal prediction calls for differences in measurement reliabilities between the two samples. Once a LISREL routine has crunched these data, where will I find the relevant evidence? Since reliabilities are indexed by proportions of error variance, I want to look mainly at the relative sizes of the disturbance terms estimated by the model. These are presented in the final columns of the Judd-Milburn Table 3. Given the natural metric used, the numerical magnitudes are meaningless to the casual reader. We should remain aware for what follows, however, that these error variances represent an embarrassing fraction of the total observed variances in these items.

It is also possible, given the natural metric, that we cannot make intersample comparisons without more information from the respective variance/covariance matrices. However, assuming the numbers would not be printed unless some sense can be made of them, I naturally note that, on the average, the absolute size of the error variances runs 20% greater in the noncollege sample than among the college graduates. These differences are not striking compared to real contrasts with genuine political elites, where figures run more like 200% than 20%. But they are in the right direction and should not be called a disconfirmation.

Actually, the authors do not call these vectors of disturbance terms a disconfirmation. In fact, they do not direct the reader's attention to them at all. Their discussions hinge on the rest of the estimates, which are, in effect, what remains with the differential unreliabilities analytically removed. This is the essence of the third disconfirmation strategy above.

In view of the rest of the authors' discussion, it is possible that we disagree profoundly as to the nature of reality out there. That is, I long have taken the low reliabilities which everybody agrees surround these attitude measures in the mass public to reflect a certain confusion that intuitively would seem warranted given the low levels of information about most of these subjects that everybody also agrees pertain in the public. To my eye, the authors obliterate these signs of confusion analytically and, then, turn around to proclaim proudly that no such confusion exists, at least not "really," for example, in the estimates from the structural model. I am not unaware of what this kind of "real" means. At the same time, I cannot forget how the data looked before the authors began to process them, and I am convinced that those raw data say something about reality out there, too.

REFERENCES

Barton, Allen H., and R. Wayne Parsons
1977 "Measuring belief systems structure." Public Opinion Quarterly 41:159–80.
Converse, Philip E.
1964 "The nature of belief systems in mass publics." Pp. 75–169 in David E. Apter (ed.), Ideology and Discontent. New York: Free Press.
1970 "Attitudes and non-attitudes: continuation of a dialogue." Pp. 168–89 in E. R. Tufte (ed.), the Quantitative Analysis of Social Problems. Reading, Mass.: Addison-Wesley.
1975 "Public opinion and voting behavior." Pp. 75–169 in F.I. Greenstein and N. W. Polsby (eds.), Nongovernmental Politics, Vol. 4 of the Handbook of Political Science. Reading, Mass.: Addison-Wesley.
Judd, Charles M., and Michael A. Milburn
1980 "The structure of attitude systems in the general public: comparisons of a structural equation model." American Sociological Review 45:627–43.

HOW JEWS AVOID ALCOHOL PROBLEMS*

BARRY GLASSNER AND BRUCE BERG
Syracuse University

American Sociological Review 1980, Vol. 45 (August):647–664

Twenty-five years have passed since the last major study of Jewish drinking patterns. During that time Jews have drifted away from the Orthodox religious affiliations which the earlier studies (Snyder, 1958; 1978) found to be important in maintaining low alcohol problem rates, and yet these rates remain low. Data from a detailed study of Jews in an American community suggest a revised explanation that focuses upon four protective social processes: (1) association of alcohol abuse with non-Jews; (2) integration of moderate drinking norms, practices and symbolism during childhood by means of religious and secular ritual; (3) restriction of most adult primary relationships to other moderate drinkers; and (4) a repertoire of techniques to avoid excess drinking under social pressure. The results are discussed from the perspective of informal social controls.

If there has been a single well-documented and replicated finding in the sociology of deviance, it is the low rate of alcohol problems among Jews in comparison to other ethnic groups. Jews present high percentages of adult drinkers (Riley and Marder, 1947; Mulford, 1964; Cahalan and Cisin, 1968; HEW, 1972; Levy, 1973), but low alcoholism and alcohol problem rates (Snyder, 1958; 1978; King, 1961; Chafetz and Demone, 1962; Roberts and Myers, 1967; Room, 1968; HEW, 1972; Lowenthal et al., 1975; Schmidt and Popham, 1976; Greeley and McCready, 1978). The usual statistic given in recent years is that more than 7% of the adult United States population are alcoholics, but less than 1% of the adult Jewish population is so classified (Unkovic et al., 1975; 1977). Some researchers argue for a similarly low rate of alcohol abuse in Israel (King, 1961), and historical analysis suggests that alcohol problems have been rare in Jewish communities for the last 2,500 years (Keller, 1970).

However, two and one-half decades have passed since the last major sociological study of Jewish drinking patterns, and during that time an important change has occurred. Jews have drifted away from the Orthodox religious affiliations which the earlier studies (Snyder, 1958; 1978) found to be so important in producing low alcohol problem rates. "The noteworthy sobriety of the Jews appears to be primarily associated with the culture of Orthodox Judaism—a religious culture with a ritualistic emphasis, prescribing frequent drinking which is integrated with familial religious practices," Snyder (1978:96) contended. He joined Bales (1944) in predicting that the decline of Orthodoxy would bring the end of low Jewish alcoholism rates.

Since Jewish alcohol problem rates remain low, as suggested in the studies cited above, one turns to additional explanations of such rates amid the well-recognized (Lazerwitz, 1979; Heilman, 1977) diminution of Orthodoxy in contemporary American Jewish communities. Suggestions for such explanations have been available in the literature for many years.

Some theoretical constructions point, for instance, to special positions of the ethnic group in society. Glazer (1952) built upon Kant's (1789) argument that Jews are in vulnerable positions and that they respond with an emphasis upon self-control and propriety. "It is the consequences of the siege, passed down from generation to generation, and including such elements as the desire to hold on to one's senses and a distaste for the irra-

---

* Direct all communications to: Barry Glassner; Department of Sociology; Syracuse University; Syracuse, NY 13210.

For helpful comments the authors wish to thank Howard S. Becker, Louis Kriesberg, Ephraim H. Mizruchi, James Orcutt, David J. Pittman, Charles Snyder, Gary Spencer, Manfred Stanley, and anonymous reviewers for the *ASR*. For partial funding, we thank the Scientific Advisory Council to the Distilled Spirits Council of the United States, and the Syracuse University Senate Research Committee.

tional, that sets a limit to Jewish drinking," Glazer (1952:185) concluded. Similar arguments hold that Jews try to avoid the public censure that might arise from "affective" drinking (Glad, 1947), or that Jews have come to emphasize education and rational self-control (Snyder, 1978). Although this sort of discussion is descriptive of some Jewish value styles, it is by itself insufficient to explain low alcohol problem rates. Even if one is willing to grant causal power to values, the problem arises that these values somehow become substance specific. Jews do exhibit high rates of obesity and overeating (Patai, 1977: 447–53), high rates for neuroses and some psychoses (Wilkinson, 1970; Patai, 1977:447–53), and, within subpopulations, high rates of drug usage (Robbins et al., 1970; Patai, 1977:453–5).

What seem to be operative, then, are alcohol-specific protective characteristics in Jewish communities. Among those frequently cited are sacramental and family drinking (King, 1961; Snyder, 1978), which have depended, of course, upon traditional religious practices. (One must wonder about their continuation and vitality amid declines in traditional religiosity.)

A contrasting approach to the question of relatively low Jewish rates of alcohol problems seeks characteristics usually found among substance-abusing groups but absent from nonabusing groups.[1] Perhaps the best confirmed sociological generalization on this issue is that involvement with marijuana and alcohol is highly contingent upon usage of these substances by peers (Orcutt, 1978). If an explanation of Jewish drinking based upon this generalization is not to be "question-begging," however, it must address important issues. Do Jews, in fact, view members of their ethnic group as their principal drinking peers, rather than turning to peers from other walks of life, such as neighborhood, work or school friends? How could Jews maintain such peer relationships when other ethnic groups do not? Ample evidence suggests, for instance, that after the first generation in America, Italians (Lolli et al., 1958; Blane, 1977) and the Irish (Glad, 1947; Greeley and McCready, 1978) do accommodate to the drinking patterns of additional peers outside their own groups, even where ethnic group members cannot be properly termed assimilated at the structural or primary group levels. Indeed, Bales (1944), Snyder (1958) and Glatt (1973) all predicted both increasing acculturation of Jews and the expected concurrent increases in alcohol abuse.

In short, the development of an adequate explanation for the infrequency of alcohol problems among Jews requires a great deal of "unpacking" of social processes. The control of drinking needs to be approached directly—at one level to uncover within peer groups the actual means and content of socialization, inclusion and exclusion rules, relations with other groups, methods of boundary maintenance, and the day-to-day repertoire of substance-relevant behaviors. Similar questions might be asked at the level of the family as well, and here one might also identify the existence, or current content, of longstanding rituals, beliefs and symbolic associations. Such an analysis can help us to understand two phenomena: the group's actual drinking patterns, and the means by which the group aids its members in avoiding other drinking patterns available in the larger society. While some processes are productive of both phenomena, this article focuses primarily on avoidance.[2]

A recently emerging theoretical perspective that might be called the informal social controls approach entails this sort of analysis and incorporates well-confirmed hypotheses from the other perspectives. The general program is to identify two generic sets: (1) social controls that tend to prevent persons from passing through (2) a series of steps necessary in changing from one status to another. This approach is applicable to the variety of situations in which persons who are in many ways good candidates for a new status are prevented, at the primary groups level, from acquiring that status. Such situations include movement into educational, occupational, and deviant statuses. In the case of substance abuse, for the first generic set one seeks the general forms of regulating mechanisms available to groups in their dealings with substance usage, then tries to specify the sources and specific varieties of controls operative in a particular group relative to a particular substance. Similarly, to specify the latter set, one seeks the series of steps through which persons must pass in becoming substance abusers, together with any special content of these steps for a particular substance or group.

---

[1] Among those often listed are high levels of inhibited impulses and anxiety (Horton, 1943; Bales, 1944; Marshall, 1979), employment in jobs involving stress, power or challenges (McClelland et al., 1972), and anomie (Merton, 1957; Cahalan, 1970). However, even a casual look at Jewish communities would suggest the presence of these phenomena.

[2] Other reports in preparation deal with the following topics: Jewish drinking patterns, including amounts of alcohol consumed, attitudes toward drinking, and "social" vs. "need" drinking; a detailed discussion of methodology; and responses of Jews to heavy drinkers and alcoholics.

Relevant to the first generic set, a report from the National Research Council (Maloff et al., 1979) identifies, on the basis of a great deal of cross-cultural data, five types of informal controls on consumption: cultural recipes that describe which substances to use in which amounts to achieve which effects; other persons who teach when, where, how, why and with whom to use substances; rules specifying eligibility requirements for use (e.g., age, gender); sanctions that reinforce usage norms; and social relations that make it convenient or inconvenient to use substances in certain ways. Maloff et al. show that these controls function in varied ways among many groups and with regard to substances ranging from food to heroin. For instance, sanctioning may be accomplished by elders, by public officials, or by peers, and a sanctioning group may offer positive sanctions for one substance and negative for another. In all cases, however, the controls operate as traditions that are maintained through adaptation to historical, geographic and other situational conditions.

These controls restrict the availability or recognizability of the second generic set: the series of moves that several prominent studies suggest are necessary if a person is to use material objects for deviant ends. Whether it be money for embezzlement, or drugs or alcohol for problematic usage, crucial among the steps to deviancy appear to be (1) a redefinition of the person's problems as amenable to relief by use of the substance, (2) knowledge of how to use the substance in the necessary way, and (3) rationalizations to oneself and others in order to legitimize special usage of the substance (Cressey, 1953; Becker, 1963; Lofland, 1969; Orcutt, 1978; Maloff et al., 1979).

If an informal social controls view is sound, then a sociological explanation for low alcohol problem rates among Jews would seek to identify both the adaptations within this group of the generic informal controls on substance use and the way in which these control processes inhibit the three steps to substance abuse. In so doing, we may avoid the generalizability problems of group- or period-specific explanations (e.g., that Jewish drinking patterns result from Orthodoxy per se), and the potential question-begging in a plural society of group-nonspecific explanations (e.g., that peer drinking in general produces individual drinking).

The data reported below are from a three-year study that examined the drinking practices and secular and religious lifestyles of a scientifically selected random sample of American Jews. After describing the methodology, we report findings as to whether alcohol problems are actually rare among Jews. The remainder of the paper suggests an informal social controls view of how a Jewish community continues to avoid the moves toward alcohol abuse. We describe the learning, beliefs, social interaction patterns, and practical drinking strategies involved in maintenance of low alcohol problem rates.

METHOD

A stratified random sample was developed in a city of central New York that is consistently listed among the top few northeastern cities for market and social scientific research because it offers "mean demographics" on variables such as population size, ethnic composition, and age and income distributions. Estimates of the size of the Jewish population range from 8,000 to 12,000. To develop the sample required six months of negotiation with Jewish community organizations, many of whom were reluctant to allow their lists of names to be used for any purpose. Many separate lists from religious, social and secular Jewish organizations were utilized to construct a universe for sampling. These included congregational lists from five of the seven temples in the city, membership lists from nationally affiliated Jewish organizations, and a list of potential contributors to the local Jewish Welfare Federation. The latter list alone is reliably alleged to include the names of 95% of all Jews in the area and includes many unaffiliated ("secret") or nonpracticing Jews, who have been identified by Federation members. Through computer searches, duplicate names were eliminated and a random sample was generated. To this we added a random sample of Jewish students at the local university, produced by telephone screening of a random sample of the entire university student population. The students were added in light of estimates that 30% of the local Jewish community are university students, and in light of data suggesting that most Jewish youths attend college.

Potential respondents, in telephone contacts to set up appointments for in-depth interviews, were informed that the interviews would concern "lifestyle issues." The topic of drinking was not introduced during these requests for interview appointments. After considerable efforts we achieved a relatively low (9%) rejection rate (cf. Goldstein, 1964).[3]

In order better to compare subgroups which earlier studies have found to be important, we stratified the sample into equal cells of four persons each, as follows: young adult, middle age, and older; male and female; and Orthodox, Conservative, Reform and nonpracticing[4] subjective denominational affiliations. In so doing we were unable, despite hundreds of telephone calls, to fill the two cells for young Orthodox. The resulting sample consists of 88 Jewish individuals, representing 88 different families. Income distributions and education levels are similar to those of Jews nationally, except that our sample underrepresents the lowest income levels.[5]

The interviews were as extensive as we could make them. The schedule combined short-answer demographic questions with structured open-ended questions that encouraged respondents to discuss the issues fully. Many of the 144 scheduled questions were parts of series of planned "how," "why," "could you tell me more," and similar probes. Essentially our interview schedule used Snyder's (1958) as a developmental reference, in order to permit direct comparisons to his findings, but went into greater detail on

---

[3] A good deal is known about those who did refuse. Their reasons were lack of time or recovery from major surgery. Only two persons refused for other reasons ("invasion of privacy" in the case of a college professor, and fear of having answers recorded in the case of an elderly concentration camp survivor). Of those who refused for lack of time or recovery from surgery, we were able to determine that in the first case they did maintain regular and busy schedules, and in the other that they had been hospitalized.

[4] Nonpracticing Jews are taken to be those who describe themselves with that label, and includes persons sometimes called "secular," "Jews only by birth," or "inactive." All have in common that they see themselves as uninvolved in religious activities or organizations. To further ensure that we did not miss this group, we attempted snowball techniques, requesting that our early interviewees tell us in confidence of such persons they have identified in the community. None of the names mentioned, however, turned out to be absent from the original master list we had used for sampling purposes.

[5] Specifically, the median reported family income for 1977 is approximately $20,000, the range is $6,000 to $102,000, and the mean is $27,500. Converting our median income and that of the most recent national Jewish survey (Massarik and Chenkin, 1973) to standard (1967) dollars, median income for our sample is approximately $11,500, which is within a few hundred dollars of the national Jewish median. Eleven percent of our sample is in the highest income category reported in the national survey (17,000 standard dollars), compared to 8% nationally. On the other hand, our sample notably underrepresents the lowest income levels (3% below 5,000 standard dollars, compared to 19% nationally). The median and mean number of years of education for our respondents is 15, with a range of 6 to 22, and approximates the national sample's median of 14 years, and mean of 13.4. In a final item of comparison, 91% of our respondents are American-born, compared to 77% nationally.

"secular" topics. Thus, the schedule primarily emphasized issues regarding family life, thorough family and personal drinking history, religious practice, assimilation, feelings about ethnicity, attitudes towards drinkers and drinking, important life events, participation in Jewish communities, eating behavior, peer relations, and recreational activities. The interview was standardized to the extent that questions were presented to each respondent with the same wording and in identical order.

Ordinarily, interviews lasted 2 to 3 hours, with a range of 1½ to 3½ hours. Each interview was transcribed verbatim and coded by at least two independent coders. Only those responses about which the coders agreed and about which the respondent did not contradict himself or herself during the course of the interview are reported below as supporting our contentions.

There is considerable evidence within almost every transcript that the interviewer established good rapport and open, detailed discussions. It became almost commonplace, for instance, that the interviewer would be invited to dinner or invited to join the respondent's temple. Frequently, respondents would comment that they were offering more detail than they would give their own friends, or that they, as one respondent put it, "know what we should say, but that's not the way it is."

We are convinced that our familiarity with the research community, use of positive and negative wording on separated questions about key items, and friendly rapport amid extensive probing have paid off in valid, high-quality data. Undoubtedly, some respondents avoided some issues, but we doubt that many escaped very far from the net we cast.

Our sample and data are limited, however, in at least four ways. First, the 40+ hours required to sample, interview, transcribe, code and analyze an average interview, and limited funding, prohibited expanding the research to include interviews with family and friends of the respondents. Second, a desire to avoid giving respondents an opportunity to phrase "good" answers to sensitive questions restricted us to a single interview with each respondent. Third, the sample size and scheme (disproportionate stratification) permit only approximate estimates of percentage distributions.[6] Finally, respondents are all from one community. We tried to select a community of average size and in the region of the country where most American Jews live, but our findings may not generalize to some other locations, especially internationally or within the largest metropolitan areas. We accepted these limitations in exchange for intensive research opportunities and with the hope that future research will supplement ours.

JEWISH ALCOHOL PROBLEM RATES

Despite seemingly conclusive evidence within the literature cited in the first paragraph of this paper, we were frankly skeptical that Jewish alcohol abuse rates could be so low. We suspected that low alcohol abuse rates among Jews resulted more from the ability to hide excessive drinking and from the collection procedures of social scientists than from actual drinking patterns of Jews. We had each known an alcoholic Jew who managed to escape public detection. In addition, recent reports have argued that the Jewish alcohol abuse rate is rising (Glatt, 1973; Spiegel, 1978; Zimberg, 1976; *New York Times*, 1977; *Jewish Record*, 1977; Dropkin and Blume, n.d.[a]).

---

[6] Our equal cells turn out to be more closely proportionate, however, than might be expected, when compared to national Jewish samples. Our sample is divided into thirds by age groups, compared to these actual percentages nationally (of those of interview age, i.e., 15 years or older): 24% ages 15–24, 29% ages 25–44, and 47% 45 or older. The mean age in both our and the national sample (Massarik and Chenkin, 1973) is 35 years. Our sample consists of 27% each in nonpracticing, Reform and Conservative cells and 18% in Orthodox, compared to national sample estimates of 11% for Orthodox, 42% Conservative, 33% Reform and 14% nonpracticing (Lazerwitz, 1979). The sampling and coding procedures used in the national survey probably led, however, to undersampling of nonpracticing Jews. Sex distributions are roughly equal nationally and are equal in our sample. Thus, the most noteworthy disproportionalities in our sample appear to be an underrepresentation of persons 45 and older, and of persons with conservative religious affiliation.

We built into our methodology every technique we could find that would encourage full access to the extent of alcohol abuse in a Jewish community. The interviewer emphasized legal guarantees of absolute confidentiality. He was trained to conduct the structured interview as a conversation, by memorizing the schedule and undergoing extensive practice and pretesting sessions to work out segues, elaborate probes, and pacing. The interview schedule involved not only detailed drinking histories but challenging questions (e.g., "Who was the last alcoholic you knew?").

To our surprise, no member of the sample met the standard definitions of alcohol abuser (Kessel and Walton, 1965; Robinson, 1976; Glatt, 1970; Jellinek, 1960; National Council on Alcoholism, 1972). For instance, none has been intoxicated more than a few times, nor suffered social, economic or medical consequences as a result of drinking, nor could be said to be addicted to alcohol; and the heaviest drinker in the sample consumes fewer than ten glasses in any week. Of their Jewish friends and family members, only about one-fourth of the sample have ever even heard rumors of drinking problems, and in most of these cases the stories referred to distant relatives.[7]

A detailed analysis of drinking patterns within the sample will be presented in another paper. In the present context, 59% report having never been intoxicated, 23% report having been intoxicated only once or twice, and 18% report more frequent intoxication. In all cases these more frequent intoxication episodes were restricted to college or military years, or to special occasions such as weddings and Bar Mitzvahs. Of the full sample, 7% (all businessmen who drink at meals with clients) report one or more drinks per day, 5% (all Orthodox) are abstainers, and for the most part the remainder drink two or fewer glasses of alcoholic beverages occasionally. Seventy-six percent of those who drink mentioned alcohol use solely for social occasions, while the remaining 24% mentioned social drinking plus the occasional use of alcohol to relax.

In addition to our sample we solicited several key informants who occupy special positions, including rabbis, physicians, alcoholism counselors, a central figure in the regional Alcoholics Anonymous (AA) organization, and a city court judge—all of whom we interviewed solely about knowledge of Jewish alcohol abusers. All efforts turned up one unmistakable conclusion: Jewish alcohol problem rates in this community are very low. The highest number of alleged Jewish alcoholics reported to us came from a clinician who is dedicated to uncovering such persons. "There is an alarming problem with alcoholism in the Jewish community," he insisted. "I can name five alcoholics for you." All of the other key informants reported that there are not more than one or two Jewish alcoholics in this community.

Although one can easily imagine Jewish alcohol problems increasing in some places, the published reports have not established this. Most rely upon evidence that Jewish membership in AA groups is increasing in New York City and Los Angeles (*New York Times*, 1977; Dropkin and Blume, n.d.[a]; Spiegel, 1978). Our key informant from the AA regularly attends meetings throughout the central New York region and reports having seen one or two Jewish members in the decade she has been involved. In any event, an increase in AA membership may signal only an increase in seeking help, rather than an increase in prevalence of alcohol problems.

Much of the discussion of increasing rates depends upon speculation, as in Spiegel's (1978) allegation that there may be as many as 10,000 Jewish alcoholics in California's San Fernando Valley. We could locate only one study with any descriptive figures. Dropkin and Blume (n.d. [a],[b]) have been conducting searches throughout the New York City area to locate Jewish alcoholics for their sample. They report locating approximately 80 persons in the first four months, many of whom "traveled long distances to be interviewed," and that they are "far from having exhausted the supply." Given that approximately one million Jewish adolescents and adults live in the New York City area, even if the researchers locate one hundred times as many Jewish alcoholics as they had at the time of their report, the demonstrated Jewish abuse rate would remain less than 1%.

---

[7] None of this is to say that Jews do not buy or own alcohol. Indeed, the vast majority of homeowners in our sample boast of well-stocked liquor and wine cabinets. However, they also note either that these cabinets seldom require restocking, or that bottles are thrown out before they are empty. A nonpracticing businessman in his 40s:

> I have bottles and bottles of liquor that just have never been opened. Or things that have been opened that have never been touched through the years.

A Conservative college student in her teens:

> My father being a doctor, people just give him liquor. He says, "Another one?" For my brother's Bar Mitzvah we had over 200 people. We didn't even have to buy any liquor, just used the ones he'd been piling up.

The discussion about increasing Jewish alcohol abuse rates is of more than demographic interest. Not only is it being used to create more services for alcoholic Jews, but the existence of public claims of higher rates may actually help to increase the prevalence of alcohol problems within Jewish communities. This becomes evident upon examination of the first of four preventive social processes we located in the Jewish community.

FOUR PREVENTIVE PROCESSES

Our working assumption was that many factors combined to produce low alcohol problem rates among Jews, and we encouraged the several coders to find any evidence they could regarding approximately 50 hypothesized control processes that we expected to be operative. These included specific religious practices, various childhood experiences, emphases on self control, aspects of assimilation, distaste for alcoholic beverages, gender proscriptions, easy intoxication, peer affiliations, anxiety produced by drinking, styles of handling responsibilities and challenges, and general avoidance of drugs. We had also suspected that respondents suffered few stressful life events that would lead to alcohol problems, but found several histories of many events that respondents and others (e.g., Holmes and Rahe, 1967) found severely stressful, but without increased drinking. In addition, we informed the coders of our suspicion that still other processes, which we had not imagined, were also operative, and encouraged them to identify these within the transcripts. In the few cases where such additional processes were suggested by the coders, these were added to checklists used in coding the remaining transcripts, and previously coded transcripts were reexamined.

Against our expectations, only three of the processes we had anticipated, and one additional process (#1) uncovered during coding, were evident in more than a few of the interviews; but these four processes prove very powerful and in some cases subsume other processes. As will be noted below, the four appear regularly in the transcripts and put the Jew in a special lifelong relationship with alcohol. The protective social processes are as follows: (1) association of alcohol abuse with non-Jews; (2) integration of moderate drinking norms, practices and symbolism for oneself and significant others during childhood by means of religious and secular ritual; (3) continual reiteration of moderate drinking through restriction of most primary relationships to other moderate drinkers; and (4) a repertoire of techniques to avoid drinking more than one wants to drink amid social pressure. Together, these processes make it difficult for Jews to go through the moves listed at the beginning of this paper as necessary for deviant use of a substance. The four processes act as social controls that avert persons from redefining problems as amenable to relief by means of alcohol, learning to use alcohol to solve problems, or rationalizing such usage to themselves and others.

1. Alcohol Problems As Non-Jewish

The first protective process evident within our sample depends upon an assumption in everyday logic which holds that if one's group lacks a particular attribute, one must also lack that attribute. It depends as well upon a most basic sociological finding: that definitions of situations can structure situations (McHugh, 1968).

The fundamental point is Durkheim's,

that people control their consumption by referring to social norms that prescribe how much of a given substance people like them usually consume, and Sumner's, that members of groups contrast themselves with images of other groups. Within the Jewish community the belief system prescribes that extensive drinking is an outgroup characteristic, moderate drinking an ingroup characteristic.

The saliency of this belief is evidenced by 52% of our respondents telling us, although we did not even ask them about this issue, that alcohol problems happen to non-Jews.[8] When one excludes the college-age respondents, for whom we note in our discussion of the third protective process that the processes are generally inoperative, fully 69% of the remaining sample told us, without any stimulus from the interviewer, that Jews are not problem drinkers.

In most cases the respondents directly stated the association. An Orthodox counselor in his 60s:

> Nobody ever drank to get drunk. I mean that wasn't, isn't a Jewish concept. Liquor and wine is part of Jewish, you know, holiday and tradition. More sociability, at parties.

A Reform college professor in his 40s:

> [A friend] drinks a lot. I don't think he's an alcoholic, but his idea of socializing is to drink, so I buy a bottle of Scotch to have in the house, just in case, because he likes J&B. So when he comes over, I'll always offer him a drink. He and his wife are very drink-oriented people. They drink, and I associate that with non-Jewishness myself. I always associate Jewish tradition with food, as most people do, I think.

A Reform housewife in her 30s has long held this association:

> We were exposed to liquor plenty. Actually, my father worked for a beer company. It sounds like a stupid generalization, but non-Jewish people drink more heavily than Jewish people. That's a generalization I've been brought up with . . . and I still think it's true.

In some cases the association is apparent in less direct statements. A Reform housewife in her 50s:

> I've been a guest at [a local Jewish Country Club]. . . . You know, they never spend money on their bar there like they do in most Gentile country clubs.

Ours is a reconfirmation of the finding within earlier studies (Snyder, 1958:chap. 5; Kramer and Leventman, 1961). Indeed, Zimberg (1976) goes so far as to claim that "the sociocultural attitudes equating Jewish identity with sobriety are perhaps the major factors accounting for low alcoholism in Jews." That Jews define alcohol problems as not Jewish is customarily interpreted within the literature as one way in which the pious Jew maintains his sense of moral superiority (Snyder, 1978:162–9). Our data support this interpretation among the Orthodox, about whom the earlier literature concentrated, but verbalization of the belief is no less among other Jews in our sample, for whom it seems to be taken as a learned fact, as the above quotations suggest.

There are apparently three sets of experiences that make most of our sample accept the folk association of alcohol problems with non-Jews. First, the respondents indicate that eating is considered the Jews' naughty excess (a point discussed in the next section). Second, their own "fieldwork" leads them to accept the association. Only 13% say they know a Jewish person who might be considered a heavy drinker, and 78% say they find no evidence that alcohol problems could occur in their own families (in contrast to Dropkin and Blume's [n.d. (a)] sample of Jewish alcoholics, among whom 63% report a family history of alcoholism). Third, Jews see their own coping strategies as emphasizing rationality (Glazer, 1952). Explained a nonpracticing businessman in his 30s:

> [When] I run into a problem, something that I can't cope with, or tremendous excitement when we had the baby, I purposely didn't drink because I was going to lose control of myself. Or specifically when we had the baby I was so excited I didn't want to have any of my senses damaged, and I'm conscious of it at this point. When we get into some very sensitive business issues, or high

---

[8] The Yiddish expression is *shikker vie a goy* (drunk as a Gentile), usually quoted by our respondents as *shikker is a goy*.

> pressure times that occur during the year, I will purposely stay off it, only because I want to be as sharp as possible, and it does dull [you].

To the extent that defining one's group as nonalcoholic helps to prevent members of the group from becoming problem drinkers, recent campaigns promoting the idea that increasing numbers of Jews are alcoholics may, ironically, increase alcohol abuse. That the campaigns are having an effect is evident in several interviews. One finds questioning and confusion about the association between alcoholism and non-Jews among several of the younger respondents. A conservative professional student in his 20s:

> Traditionally in the Jewish community there's very little alcoholism. I'm convinced that is no longer true. The fact of the matter is that a number of congregations have opened up Alcoholics Anonymous chapters. . . . My family were never drinkers at all, and more and more people I know drink now. I still think to a certain extent it's a Gentile characteristic. From what I hear, there's a great difference between Jewish and Gentile celebrations. Jewish celebration is punctuated by the enormous amount of food service. From what I was told the Gentile idea of a party [as] being booze and pretzels still holds true to a great extent.

This first protective process thereby turns out to be two-sided. A recent newsletter quotes a psychologist's claim that "the Jewish alcohol abuser is . . . the most difficult to work with. They 'know' they don't exist. They believe they have the same reality as unicorns. This makes it very difficult for them to identify themselves as alcohol abusers" (Spencer, 1979). Other clinicians (Dropkin and Blume, n.d. [a], [b]) report that Jewish alcoholics use as a defense mechanism the definition of alcoholic as non-Jewish ("Since I'm Jewish, I can't be alcoholic").

Thus, on the one hand, the definition of alcohol problems as non-Jewish serves as an eligibility control that can contribute to avoidance of alcohol problems within the larger Jewish group by making more difficult the moves which we have noted are necessary for substance abuse. If extensive drinking is considered non-Jewish, it becomes more difficult for a Jew to believe that problems are solved through the use of alcohol, to learn how to use alcohol to solve problems, or to rationalize to significant others such deviant use of alcohol. On the other hand, for those Jews who despite the deterrents do find their way to alcohol problems, the Jewish community's definition can hinder the work of helping services personnel by promoting denial on the part of the alcoholic.

Needless to say, holding the definition of alcohol problems as non-Jewish cannot alone provide Jews with continuing moderation of drinking patterns. One's conception that a substance is undesirable (or "dirty") depends in large part upon respect for the conventions of one's groups (Douglas, 1966). These conventions are revealed not only by stories about how the group deals with the substance but also by the conduct of users. In fact, the very perpetuation of the definition of excessive drinking as non-Jewish depends upon the actual drinking practices of Jews—practices which reinforce either the truth or the falsity of the association. The first process of control depends upon the three additional processes, which together maintain moderate drinking practices. The four processes form what might be visualized as a fabric, with each strand depending upon the others.

2. *Moderation Practices from Childhood*

As noted at the beginning of this paper, Snyder (1958) argued that Jewish sobriety "depends upon the continuity and vitality of the Orthodox religious traditions." He emphasized the importance of Orthodox religious affiliation per se for his respondents, but also within his work one finds the possibility for extension of the protective characteristics of Orthodoxy. He argued, for instance, that "the thread of orthodox life may be woven into many regional fabrics without losing its sobering influence." Our data suggest that the protections have been extended even further, into the contemporary "secularized" community. Through religious and ceremonial usage of alcohol, Jews continue to learn "prescriptive" (Mizruchi and Perruchi, 1970) drinking norms. In contrast to the expectations of those authors (Zimberg, 1976; Malzberg, 1940; Bales, 1944) who expected the loss of traditional protections with the loss of Orthodoxy, we have detected a good deal of tenacity and adaptability of traditions and their latent symbolism.

Our data do confirm the drift away from Orthodox affiliation, as evidenced most dramatically by our inability to fill the cells for young Orthodox respondents. However, the data indicate that a vast majority of respondents' families continue to practice regular ceremonial and religious ritualistic practices that involve controlled drinking of alcohol. For instance, 89% of the sample celebrate Passover Seder, in which prescribed amounts of wine are consumed as part of a religious and historical service. Included in this group who hold Seders are 85% of the respondents who consider themselves "nonpracticing." In addition, 77% of the sample report that they attend Sabbath services at least occasionally, where wine is consumed as a religious rite and in many cases during the post-service gathering. Another indication of continuing traditions is the prevalence of the ritual *brieth* (briss) upon the birth of a male child, rather than a simple medical circumcision, even among our "nonpracticing" and Reform respondents. Also noteworthy is the pattern we found among the 10% of the sample in exogamous marriages. In each case there was evidence that the Gentile spouse had begun to foster Jewish customs and an identification with Jewish lifestyle, even if he or she had not formally converted to the religion.

In short, the Orthodox affiliations are generally absent, but children continue to have plenty of opportunities to learn to associate the drinking of alcohol primarily with special (dare we say "sacred") occasions (see Knupfer and Room, 1967:690). Currently met in new ways are the conditions Snyder (1978) listed on the last page of his book: "Where drinking is an integral part of the socialization process, where it is interrelated with the central moral symbolism and is repeatedly practiced in the rites of a group, the phenomenon of alcoholism is conspicuous by its absence."

Sobriety amid drinking continues to be the norm in Jewish homes. Our interviews suggest that Jews perpetuate this association and its connection with ritual, not only by affiliation with religious life but also by restricting drinking to special secular occasions, and by cataloguing drinking as a symbolic part of festive eating.

Almost without exception, our interviewees report that drinking in their homes during childhood was predictable, and drunkenness was never condoned or was condoned only on very rare occasions. A Conservative secretary in her 60s:

> Parents don't usually put a drink in front of themselves or their children. Mine didn't, and I wouldn't with my kids. Some kids will say, "You do it, why can't I do it?" We never did drink except for a special occasion.

A nonpracticing medical researcher in her 30s:

> It was a ritual in our daily life. It was a family routine. [My father] would sit and read the paper for a little while and have the drink and then we would have dinner and my mother would have a drink with him.

A Conservative college student in her teens:

> My parents do not drink, which I think is one reason why I don't drink, because I never really saw my father come in after work and drink. We'd have wine with dinner, if someone brought it over, and I think there was a bottle of white wine in the refrigerator.

Nearly half of the sample could not remember their first drink, but recall that all of their early drinking was during childhood, in the home as part of religious ceremonies. Of the remainder of the sample, 89% recall that their first drinks were recreational with their families, in the home or at the synagogue, and prior to the age of 13. Thus, only about 5% of the sample recall their first drinks as outside the family and later than childhood.

Our sample thus meets the conditions that several studies have found result in low adult alcohol problem rates: parents who agree about drinking norms for themselves and their children; formative drinking experiences within the family; and controlled drinking at an early age (Wilkinson, 1970; McGonegal, 1972). Jews are sheltered from what Pittman (1967) calls the ambivalent drinking culture of the United States, which consists of conflicting attitudes toward alcohol consumption. The Jewish family maintains instead what Pittman calls a permissive drinking culture in which drinking is permitted, but excessive drinking is not.

In addition, Jews receive an early advantage over many other ethnic groups by means of the main beverage served in the home and the form of introduction to this beverage. Most of the sample first tasted wine, and nearly the entire sample report that most drinking in their parents' homes involved wine. Thirty-one percent of those who have a preference list wine as their current favorite alcoholic drink, and another 44% name other sweet drinks (e.g., rum and coke, grasshoppers, screwdrivers) as current favorites. Virtually the entire sample drink wine more than any other alcoholic beverage. In contrast, and despite stereotypes of alcoholics as "winos," few alcohol abusers concentrate upon wine or sweet drinks (HEW, 1972), perhaps because these beverages become unpleasant in large quantities.

Our interviewees tend to drink only a glass or two of wine when they do drink, and they tend to view wine as quite apart from intoxicating alcohol, indeed as almost nonalcoholic. During an interview with a retired Conservative businessman in his 60s, the following exchange was recorded:

> Q: Did your mother ever drink with your father?
> A: My mother never drank.
> Q: Nothing? Not even wine?
> A: Oh, she drank wine, yeah. On occasions.

A Conservative retired salesman in his 70s:

> A: I don't drink at all.
> Q: You don't drink at all?
> A: Very seldom. I'll just have some wine.

These early drinking experiences probably proved protective by the form of the drinking, however, more than by the content of the drinks. Nearly all of the respondents recall that they were included as full participants on at least some regular occasions—given roughly as much to drink as were the adults. Lolli et al. (1958) note that Italians remember childhood drinking as part of a regular family, social or religious custom and display low alcohol abuse rates, while Italian-Americans recall a casual sip-taste and display higher rates of alcohol abuse.

Not only is Jewish drinking limited primarily to predictable family, social and religious occasions but also, as suggested above, the drinking is tightly connected with eating (cf. Hill, 1977). Most of the interviewees alluded to the relative importance of food over drink, and the absence of drinking without eating. A nonpracticing businessman in his 40s:

> I can remember more my mother arranging platters of cold cuts and potato salad or baking because she was having people [to the house] than I can ever remember her serving drinks. The emphasis was always on the food, and the liquor was deemphasized. If it was there it was because there were people who expected it to be there.

A Conservative social services administrator in his 40s was talking about what made him think that an acquaintance had a drinking problem:

> He did drink, and there is a difference between just drinking or having a schnaps or having wine with Friday night meal and the need to have a drink when there's no specific meal involved. That's my distinction, and he had a need to have alcohol for whatever his personality needs.

Several interviewees spoke as if they saw drinking as almost inseparable from eating. A Conservative schoolteacher in her 30s:

> Drinking has just never entered as an activity. It's part of a thing like eating. It's OK with foods. It goes, to me, it goes along with eating.

The moderating effect of associating drinking with eating was first noted by Feldman (1923) in a comparison of the contrasting practices of English Gentiles and English Jews. In recent decades several studies (reviewed in Plaut, 1967; and Zucker, 1976) have suggested that groups characterized by widespread drinking but few alcohol problems have integrated their drinking into a variety of basic activities. In the case of Jews, this integration is taken a significant step further, toward making drinking a symbolizer. Drinking serves as a symbolic punctuation mark that helps to separate certain good events (religious services, weddings, dinners, etc.) from all other events (cf. Mandelbaum, 1965). By being a symbolizer, the drinking takes on a symbolic character itself. However, owing at least in part to the emphasis on wine, a beverage that carries an almost nonalcoholic social meaning, and the association of excessive drinking with non-Jews, drinking has little chance of becoming substantive as well as symbolic, as among Irish men who demonstrate manhood by drunkenness or as among the Aztecs who seek intoxication in religious ceremonies. All of this adds up to avoidance by Jews of two of the moves involved in the deviant use of a substance: belief that problems can be alleviated through such usage, and learning how to use the substance to solve problems.

On the other hand, probably no sorts of childhood experiences can, on their own, protect a group from excessive substance use in adulthood, where so many experiences intervene. The two remaining protective factors maintain in adults the drinking patterns and associations they learned as children.

3. *Insulation by Peers*

Haer's (1955:180) conclusion—"the behavior of contemporaries, friends and spouse are much more potent in establishing drinking practices than the behavior of the previous generation"—has found continuing support (Orcutt, 1978; Greeley and McCready, 1978; Maloff et al., 1979). For the drinking patterns learned in childhood to be maintained, and for the self-definition of Jews as moderate drinkers to remain believable, Jews must collectively reiterate moderate drinking patterns in adulthood. This is accomplished in part through family practices, as we have already noted. However, the serious threat to moderate drinking occurs outside the home—at parties and other public occasions where considerable drinking is often encouraged or expected.[9]

Indeed, our data suggest two situations in which members of the sample radically increased their consumption and their instances of intoxication. Drinking rates during college and military service periods increased radically for at least one-fourth of those for whom the question applies. Several respondents report that nearly all of their intoxication experiences occurred during college or military service. These changes are evident for both males and females, and independent of the decade during which the respondent attended college. The only exception is the Orthodox, for whom drinking does not increase in college or in the military. As Snyder (1958) noted, the Orthodox seem to stay together during these experiences and maintain traditional views and practices.

A Reform veterinarian in his 30s told the kind of story we heard from nearly all of the relevant respondents:

> The only times I ever drank were in my junior year when we had a party at the [fraternity] house. You'd really get drunk, to the point where you got sick and everything. Everybody does that, every once in a while. That happened maybe twice a year. The only thing I ever had prior to that was a glass of wine. I didn't like beer, and never drank beer after college.

---

[9] Norms of usage and intoxication tend to vary as predictable functions of participation in peer groups using alcohol (Becker, 1963; Orcutt, 1978). One finds oneself in not one but in various contexts, each of which includes physical location and other persons in assorted interactions and combinations. "As people move from one social context to another . . . the environment suggests alternative codes of conduct" (Maloff et al., 1979). Thus, to be part of a moderate drinking culture may well be context specific. There is no prima facie reason why a Jew without drinking problems in one setting will be without these problems in other settings.

[10] Compare, for example, Maddox and Williams's (1968) finding among blacks:

> Heavy drinking is described as a common, socially institutionalized, culturally integrated means of narcotizing the drinker against the frustrations of a restrictive environment There is nothing distinctive in the notion that alcohol can be used occasionally to modify reality. Its significance here lies in the imputed legitimacy of heavy drinking as a way to "make out" in a hostile world.

Several interviewees became intoxicated for the first and last times in college, and they note the negative aspects of the experience. A nonpracticing saleswoman in her 40s:

> I used to consume more alcohol back in college. You'd have a party and people would drink and would get drunk. When I was first married I can remember giving a party and I got drunk, and I handed out towels to everyone, and I invited them to stay and ended up sitting in the bathroom sink and squirting everyone with shaving cream, singing "brush your teeth with Brill Cream [*sic*]." I woke up the next day, and I felt awful. I just kept calling everyone up and saying, "What [did] I say?" As I grew up I realized that I just didn't like the feeling.

More than tastes changed, however. Many respondents say that during college and military service they and other Jews tried to "fit in" through any means available, and that most of their friends were non-Jews. In contrast, almost without exception they report that after college and the military ("as an adult") they were less willing to "fit in" by means of activities they found unappealing.

Seventy-six percent of the sample report that all or nearly all of their friends are Jewish, although most say that their work associates are primarily non-Jews. The respondents are not, as a group, ideologically opposed to primary group assimilation, however. When asked if it matters whether they spend recreational time with Jews or non-Jews, 64% answered negatively, 25% affirmatively, and 14% (including two who answered affirmatively) described types of situations in which it would or would not matter.

Of the 24% of the sample who indicate that many of their friends are non-Jews, in most cases the discussions reveal mixed feelings about this state of affairs. A nonpracticing medical researcher in his 30s:

> I don't think it makes any difference, but I am aware of it. . . . When I meet a new person or when I hear a name it is always a thought. Oh, he's Jewish or he isn't, or with some women, I wonder if she's Jewish. So it's something I look for subconsciously, I would say, and that I'm aware of. . . . I identify myself as Jewish and so I identify, or I assume certain things about Jews that I don't assume about other people.

Several commented that it is unimportant to them whether a friend is Jewish or not, but that being with Jews is easier. Others suggested that some of their non-Jewish friends remind them of Jews. A conservative school teacher in her 30s:

> I think [my close friend] would like to be Jewish. I really do. Maybe that's part of what she likes about me. Her sister was married to a Jewish fellow and kids us that somewhere in her background there's gotta be a Jew because she likes Jewish foods.

The 76% reporting primarily Jewish friends tend to give a variety of reasons. Many persons of various ages and affiliations say they "just end up" with Jews, others indicate that they feel more comfortable with Jews, several older persons say they have a language problem and prefer to speak Yiddish or German, and some of the more religious members of the sample say they disapprove of various prejudices and moral practices of non-Jews. The majority of the sample who have primarily Jewish friends for the most part are no less certain about what attracts them to Jews than are the other respondents. In many cases we conducted considerable probing to obtain impressionistic answers, such as the following from a Reform housewife in her 30s.

> Most of my good friends are Jewish, my closest friends. One of my closest friends, though, is a girl who converted. And basically she converted, and is it. She is still—she's not Jewish. She's fun and everything else, but her values and feeling on certain things are definitely not Jewish . . . and a lot of times we discuss things she won't understand, because she doesn't come from the same background. . . . I seem to gravitate toward Jewish people. I just have a warmer feeling. A feeling of, I can't pinpoint it, but, just certain feelings.

The general gravitation toward Jewish things and people (90% see "Jewishness" as important to their lives) protects Jews from excessive drinking in obvious and not so obvious ways. Firstly, one is unlikely to find others who would encourage, cooperate with, or even condone excessive drinking. Thus, the rationalization of extra drinking, a necessary move if one is to become an alcohol abuser, becomes difficult. This is self-evident in those cases where Jews drink primarily among other Jews. The process goes further, however, to selection of non-Jews who drink like Jews (cf. Alexander and Campbell, 1970). A nonpracticing college professor in his 50s:

> Most people at the party were not Jewish, actually. This one guy was making a real ass of himself. He'd had too much to drink and it made everybody uncomfortable. I guess our friends just are not heavy drinkers. I mean, we carry drinks around at our parties, but when this guy got drunk he really stood out. . . . I think he eventually got the message, because he was one of the first to leave.

Staying with Jews and other moderate drinkers makes inconvenient or unavailable the use of alcohol to various substantive ends—in contrast to groups such as the Oaxacans, where intoxication provides persons with license to shout insults and attend social gatherings without invitation (Dennis, 1975). Jews who become intoxicated are more likely to derive only scorn or pity.

A net result of the moderate drinking environment is that Jews hold images of social interactions over their life spans that do not include much drinking. This point is evident, for example, in the case of an Orthodox businessman in his 40s who answered the question, "When did people drink when you were growing up?" with, "My family just didn't drink. People we are with now just don't seem to drink." Or the Conservative male college student who answered a question about parents' drinking patterns with, "My parents rarely drink at all. Myself, I went through the drinking phase with my friends, but we've turned the page now."

Group avoidance is thus a dynamic process. By modeling consistent group drinking practices and norms in adulthood, Jews perpetuate the drinking associations for the next generation. Also, because unique alcohol consumption practices serve to emphasize ethnic differences in this multiethnic nation (Cisin, 1978), adult drinking patterns contribute not only to perpetuation of the first protective process (defining drunkenness as non-Jewish), but also to group boundary maintenance and increased personal identification with Jewish attributes, even among Reform and nonpracticing Jews.

4. Avoidance Repertoire

Despite the social interaction patterns just noted, the drinking histories reveal that at least 46% of the sample have found themselves during adulthood under social pressures to drink more than they wish to drink. We noted several exchanges similar to the following with a nonpracticing businessman in his 40s:

> Q: Does it matter if others drink with you?
> A: No, it seems to matter more whether I drink with them.
> Q: What do you mean?
> A: My not drinking tends to make more people uncomfortable than their drinking makes me.

The drinking histories, symbolic associations, and uniformity of group opinion we have noted thus far seem to make these respondents generally unafraid to offer an assertive no when they are encouraged to drink more than they wish. When asked how they avoid excessive drinking under a variety of pressure situations, many respondents did not seem to understand why we thought such a situation might be a problem. They perceive their avoidance repertoire as nonaction. A Conservative housewife in her 40s:

> I don't care one way or another whether I have a drink. If everybody is drinking and I feel like having a drink I'll have a drink. If everybody is drinking and I don't want a drink, I don't drink.

Nevertheless, upon further probing, the respondents do reveal a variety of techniques used in such avoidance. Several report that they begin a party with a mixed drink, but refill the glass throughout the night with only the mixer (e.g., moving from gin and ginger ale to ginger ale alone). Other respondents report that their family as a unit will act to prevent unwanted drinking. Some tell of plans whereby the wife will reprimand the husband for drinking too much at a party, when actually he is still "nursing" his first or second drink. Other couples develop reputations for themselves as nondrinkers by making jokes, or by avoiding such situations. A Conservative teacher in her 30s:

> Our colleagues think we're weird, first of all because they don't understand that we enjoy being together as a family, and neither one of us, even when we are working, want to go out to "happy hour" with our faculty. We want to go home and see each other. We don't want to spend our Friday nights drinking with people at work.

During the Passover Seder service one is called upon to drink four glasses of wine. An Orthodox teacher in her 40s explained how the family avoids doing so:

> We don't fill the glasses to the top. We cheat. Whoever drinks, we fill it up. We don't really follow to the letter. We cheat a little bit. In my case, I don't drink at all. . . . I just moisten my lips—this is the best that I can do. You know, my head would get dizzy. I just don't like it.

Other respondents reported specific avoidance repertoires they had worked out for particular situations. For example, a Conservative photographer in his 40s told of problems he used to experience at weddings, where sometimes he was "on the verge of being hit because they felt I wasn't showing proper respect for the bride and groom by toasting them." This photographer reports that since those early years he has consistently utilized a series of "convincing lines," to indicate that he cannot drink while he is working. He couples this with an increased number of photographs during the periods of heavy drinking, "Being Jewish, your reflexes and everything else sharpen you, and that expedience staves off any future attempts for people to push drinks on you," he explained.

By consistently utilizing an avoidance repertoire one misses opportunities to learn the deviant uses of alcohol, which is one of the crucial moves in development of alcohol problems. Indeed, our respondents' avoidance techniques sometimes seem the obverse of the final move customarily undertaken by substance abusers. Rather than finding rationalizations for drinking, our respondents find rationalizations for *not* drinking.

CONCLUSIONS

Distinctive group-maintained patterns of eating and drinking often remain stable, despite acculturation and secularization of the ethnic group. Acculturation seems to bring radical changes in drinking patterns to those groups who lack traditional informal controls directed at the use of alcohol, and among whom intoxication can be viewed as useful in itself (Price, 1975). This paper has suggested that processes of control have a long history within Jewish communities and have adapted to changing times.

We have described four processes of informal control that appear regularly within a Jewish community. Perhaps other mechanisms also exist in Jewish groups, but these four together effect all of the generic controls noted earlier, from cross-cultural studies: recipes regarding which substances to use and to which effects; persons who teach usage patterns; rules of eligibility; sanctions that reinforce usage norms; and social relations making various usages convenient or inconvenient. They create an environment in which a person would find it difficult to go through the moves that are necessary for deviant use of a substance: belief that use of the substance can alleviate problems, knowledge of how to use the substance, and rationalizations to legitimize the deviant usage.

In short, the processes we have noted serve as informal controls which militate against moves toward alcohol problems, as follows: (1) Defining alcohol problems as non-Jewish begins the cultural recipe for moderate drinking and excludes the entire group from eligibility for regular intoxication, while minimizing the possibilities for seeing alcohol use as related to one's problems. (2) Drinking practices are learned in childhood primarily from one's family, with an emphasis on wine and exclusion of substantive use of alcohol (i.e., to other than symbolic ends).[10] (3) Moderate drinking and the association of alcohol with special occasions, and the association of drunkenness with outgroups is reinforced in adult peer groups through drinking with those who share these expectations and who punish or exclude others who do not. Such peers made it difficult to rationalize deviant drinking or even to learn such drinking practices. (4) A repertoire of avoidance techniques decreases the necessity to change one's drinking patterns amid temporary social pressures toward increased consumption, and thereby further contributes to evasion of opportunities to learn new uses for intoxication.

More generally, our research suggests some ways in which Jews, as they have become a more secular ethnic group, have managed to "secularize" and expand the traditions that Snyder (1958) found epitomized in the Orthodox Jew of the 1950s.

REFERENCES

Alexander, C. Norman, and E. Q. Campbell
1970 "Normative milieux and social behaviors." Pp. 268–89 in G. L. Maddox (ed.), The Domesticated Drug. New Haven: College and University Press.
Bales, Robert F.
1944 "The 'fixation factor' in alcohol addiction." Ph.D. dissertation, Department of Sociology, Harvard University.
Becker, Howard S.
1963 Outsiders. New York: Free Press.
Blane, Howard T.
1977 "Acculturation and drinking in an Italian American community." Journal of Studies on Alcohol 38:1324–46.
Cahalan, D.
1970 Problem Drinkers. San Francisco: Jossey-Bass.
Cahalan, D., and I. H. Cisin
1968 "American drinking practices." Quarterly Journal of Studies on Alcohol 29:142.
Chafetz, M. E., and H. W. Demone
1962 Alcoholism and Society. New York: Oxford University Press.
Cisin, I. H.
1978 "Formal and informal social controls over drinking." Pp. 145–58 in J. Ewing and B. Rouse (eds.), Drinking: Alcohol in American Society. Chicago: Nelson-Hall.
Cressey, Donald R.
1953 Other People's Money. Glencoe: Free Press.
Dennis, P.
1975 "The role of the drunk in an Oaxacan village." American Anthropologist 77:856–63.
Douglas, Mary
1966 Purity and Danger. New York: Praeger.
Dropkin, D., and S. B. Blume
n.d. (a) "The Jewish alcoholic: an under-recognized minority?" Unpublished paper.
n.d. (b) "The Jewish alcoholic woman: rara avis?" Unpublished paper.
Feldman, W. M.
1923 "Racial aspects of alcoholism." British Journal of Inebriety 21:1–15.
Glad, D. D.
1947 "Attitudes and experiences of American-Jewish and American-Irish male youths." Quarterly Journal of Studies on Alcohol 8:406–72.
Glatt, M. M.
1970 The Alcoholic and the Help He Needs. Herts: Priory Press.
1973 "Jewish alcoholics and addicts in the London area." Toxicomanies 6:33–9.
Glazer, Nathan
1952 "Why Jews stay sober." Commentary 13:181–6.
Goldstein, S.
1964 The Greater Providence Jewish Community: A Population Survey. Providence: S. Goldstein.
Greeley, Andrew M., and W. C. McCready
1978 "Societal influences on drinking behavior." NORC paper presented to the International Medical Advisory Conference of the Brewing Associations, Toronto, October.
Haer, John
1955 "Drinking patterns and the influence of friends and family." Quarterly Journal of Studies on Alcohol 16:178–85.
Heilman, Samuel C.
1977 "Inner and outer identities: sociological ambivalence among orthodox Jews." Jewish Social Studies 39:227–40.
HEW
1972 Alcohol and Health. First Special Report to the U.S. Congress from the Secretary of Health, Education and Welfare. Washington, D.C.: U.S. Government Printing Office.
Hill, T. M.
1977 "Survey of Jewish drinking patterns." Military Chaplain's Review: 65–77.
Holmes, T. H., and R. H. Rahe
1967 "The social readjustment rating scale." Journal of Psychosomatic Research 11:213–8.
Horton, D.
1943 "The functions of alcohol in primitive societies." Quarterly Journal of Studies on Alcohol 4:199–320.
Jellinek, E. M.
1960 The Disease Concept of Alcoholism. New Haven: Hillhouse Press.
Jewish Record
1977 "Alcoholism among Jews? There but not acknowledged." April 8.
Kant, Immanuel
[1789] 1941 Anthropolie. In E. M. Jellinek, "Immanuel Kant on Drinking," Quarterly Journal of Studies on Alcohol 4:778.
Keller, Mark
1970 "The great Jewish drink mystery." British Journal of Addiction 64:287–95.
Kessel, N., and H. Walton
1965 Alcoholism. Harmondsworth, Eng.: Penguin.
King, A. R.
1961 "The Alcohol Problem in Israel." Quarterly Journal of Studies on Alcohol 22:321–4.
Knupfer, G., and R. Room
1967 "Drinking patterns and attitudes of Irish,

Jewish and white Protestant American men." Quarterly Journal of Studies on Alcohol 28:676–99.

Kramer, J. R., and S. Leventman
1961 Children of the Gilded Ghetto. New Haven: Yale University Press.

Lazerwitz, Bernard
1979 "Past and future trends in the size of American Jewish denominations." Journal of Reform Judaism 26:77–81.

Levy, L.
1973 "Drug use on campus." Drug Forum 2:141–71.

Lofland, John
1969 Deviance and Identity. Englewood Cliffs: Prentice Hall.

Lolli, G., E. Serriani, G. Golder, and P. Luzzatto-Fegiz
1958 Alcohol in Italian Culture. New Brunswick, N.J.: Center of Alcohol Studies, Rutgers University.

Lowenthal, U., D. Wald, and H. Klein
1975 "Hospitalization of alcoholics and the therapeutic community." Harefuah 89:316–20.

McClelland, D. C., W. N. Davis, R. Kalin, and E. Wanner
1972 The Drinking Man. New York: Free Press.

McGonegal, J.
1972 "The role of sanction in drinking behavior." Quarterly Journal of Studies on Alcohol 33:692–7.

McHugh, Peter
1968 Defining the Situation. Indianapolis: Bobbs-Merrill.

Maddox, George, and J. R. Williams
1968 "Drinking behavior of Negro collegians." Quarterly Journal of Studies on Alcohol 29:117–29.

Maloff, Deborah, H. S. Becker, A. Fonaroff, and J. Rodin
1979 "Informal social controls and their influence on substance use." Journal of Drug Issues 9:161–84.

Malzberg, B.
1940 Social and Biological Aspects of Mental Disease. Utica, N.Y.: State Hospital Press.

Mandelbaum, D. G.
1965 "Alcohol and culture." Current Anthropology 6:281–8.

Marshall, Mac
1979 Weekend Warriors. Palo Alto: Mayfield.

Massarik, Fred, and A. Chenkin
1973 "United States national Jewish population study." Pp. 264–306 in M. Fine, M. Himmelfarb, and H. Jelenko (eds.), American Jewish Yearbook 1973. New York: American Jewish Committee.

Merton, Robert K.
1957 Social Theory and Social Structure. New York: Free Press.

Mizruchi, Ephraim H., and R. Perruchi
1970 "Prescription, proscription and permissiveness: aspects of norms and deviant drinking behavior." Pp. 234–53 in G. Maddox (ed.), The Domesticated Drug. New Haven: College and University Press Service, Inc.

Mulford, H. A.
1964 "Drinking and deviant behavior. USA 1963." Quarterly Journal of Studies on Alcohol 25:674.

National Council on Alcoholism
1972 "Criteria for the diagnosis of alcoholism." American Journal of Psychiatry 129:41–9.

New York Times
1977 "Alcoholism on rise for New York Jews." January 23: Sec. 1:32.

Orcutt, James D.
1978 "Normative definitions of intoxicated states." Social Problems 25:385–96.

Patai, Raphael
1977 The Jewish Mind. New York: Scribner's.

Pittman, David J.
1967 "International overview: social and cultural factors in drinking patterns, pathological and non-pathological." Pp. 3–20 in D. J. Pittman (ed.), Alcoholism. New York: Harper and Row.

Plaut, T. F.
1967 Alcohol Problems: A Report to the Nation by the Cooperative Commission on the Study of Alcoholism. New York: Oxford University Press.

Price, John
1975 "An applied analysis of North American Indian drinking patterns." Human Organization 34:17–26.

Riley, J. W., and C. Marder
1947 "The social pattern of alcoholic drinking." Quarterly Journal of Studies on Alcohol 8:265–73.

Robbins, Ewin, L. Robbins, W. Frosch, and M. Stern
1970 "College student drug use." American Journal of Psychiatry 126:85–93.

Roberts, B. H., and J. K. Myers
1967 "Religion, national origin, immigration and mental illness." Pp. 68–72 in S. I. Weinberg (ed.), Sociology of Mental Disorders. Chicago: Aldine.

Robinson, D.
1976 From Drinking to Alcoholism: A Sociological Commentary. London: Wiley.

Room, Robin
1968 "Cultural contingencies of alcoholism." Journal of Health and Social Behavior 8:99–113.

Schmidt, W. and R. E. Popham
1976 "Impressions of Jewish alcoholics." Journal of Studies on Alcohol 37:931–9.

Snyder, Charles
1958 Alcohol and the Jews. New Haven: Yale University Press.
1978 Alcohol and the Jews. Carbondale: Southern Illinois University Press.

Spencer, Jeri M.
1979 "West coast seminar lifts veil of denial." Alcoholism and the Jewish Community Newsletter (Spring):1–3.

Spiegel, M.
1978 "Jewish alcoholism growing problem for community." Los Angeles Jewish Community Bulletin (December 1).
Unkovic, C. M., R. J. Adler, and S. E. Miller
1975 "A contemporary study of Jewish alcoholism." The Alcohol Digest 9:vi–xiii.
1977 "The contemporary state of Jewish alcoholism." Contemporary Jewry 1:19–26.
Wilkinson, R.
1970 The Prevention of Drinking Problems. New York: Oxford University Press.
Zimberg, S.
1976 "Socio-psychiatric perspective on Jewish alcohol abuse." Paper presented to the Task Force on Alcoholism of the Commission on Synagogue Relations, New York, March.
Zucker, R. A.
1976 "Parental influences on the drinking patterns of their children." Pp. 211–38 in M. Greenblatt and M. Schuckit (eds.), Alcoholism Problems in Women and Children. New York: Grune and Stratton.

THREE PARAMETRIC TECHNIQUES FOR CONTINGENCY TABLE ANALYSIS: A NONTECHNICAL COMMENTARY*

MICHAEL SWAFFORD
Vanderbilt University

American Sociological Review 1980, Vol. 45 (August):664–690

This article presents a direct comparison of three "new" parametric techniques for contingency table analysis—linear probability, logit, and log-linear modeling. The comparisons are rendered as nontechnical as possible, so that those familiar with ordinary regression will readily see how each of these techniques is analogous to regression in cases where one variable is deemed dependent on the others. The paper highlights the availability of linear probability modeling, a technique which has been largely ignored by sociological practitioners despite some attractive features. It also describes eight common errors in published applications of log-linear modeling.

During the past two decades, the statistical technology for analyzing contingency tables has undergone tremendous development. Researchers trained in the use of nonparametric techniques have begun to find that their statistical tools are deemed outmoded and that log-linear models are now "in." However, efforts to master the new techniques—or merely to appreciate the nature of their advantages and shortcomings—are frequently frustrated by methodological articles which contain misleading examples or which leave the impression that the new techniques are quite unlike others which are already widely understood. Moreover, several sociological articles which might serve as illustrative applications suffer technical flaws. Even those articles which avoid such pitfalls often couch their descriptions of the findings in such obscure terms that readers cannot discern any compelling connection between statistical results and substantive claims. Consequently, many who could well utilize one of the new techniques—as well as many others who need to read articles employing these techniques—find them inaccessible for all practical purposes.

Whenever one of the variables of a contingency table is treated as a dependent variable, the new techniques bear such a close resemblance to ordinary regression that almost anyone with a command of regression should easily grasp their essentials.[1] Of course, this similarity has

---

* Direct all communications to: Michael Swafford; Department of Sociology; Box 1811-B; Vanderbilt University; Nashville, TN 37235.

I would like to express my gratitude to several individuals whose comments on an earlier draft of this article were very helpful: Glenn Firebaugh, Walter Gove, J. Richard Landis, Tony Oberschall, William Rushing, and an anonymous *ASR* referee. I am also grateful to Yvonne Bishop, Gary Koch, and Jay Magidson for their observations on certain issues. It goes without saying that none of the above scholars should be held accountable for the views expressed in this article.

[1] Log-linear modeling is flexible enough to handle a wide range of situations in which the data are not viewed in this fashion. Nevertheless, viewing one variable as dependent constitutes the single most common conception underlying *sociological*

long been perfectly obvious to mathematical statisticians as well as to sociologists who have read the seminal articles on this subject in statistical journals. Moreover, even the titles of two of Goodman's articles (1972; 1975) suggest the resemblance of some of his own work to ordinary regression.

Nevertheless, focusing on the relationship of the "new" techniques to ordinary regression provides a convenient framework within which to illustrate and compare contingency table analyses based on three alternative models—linear probability, logit, and log-linear—the first of which has rarely been used by sociologists, despite some attractive features. Also, it provides a useful context in which to evaluate several important flaws or shortcomings in applications of log-linear modeling published in recent years. Finally, the relationship of the techniques to ordinary regression deserves discussion if only because it is not fully understood by many. One of the most widely cited nontechnical introductions to log-linear modeling (Davis, 1974) stresses an application which is *dissimilar* to regression, and several authors (to be cited later) have followed its example under inappropriate circumstances.

LINEAR PROBABILITY MODELS

Ordinary regression is, of course, conceptually well suited for analyses in which a certain dependent variable is theorized to be the function of several independent variables, but it could not be properly invoked for analyzing contingency tables until recently. As is well known, categoric *independent* variables present no barrier to ordinary regression since one may dummy code them without violating any assumptions underlying regression. In fact, analysis of variance is, in most respects, equivalent to regressing an appropriate dependent variable on categoric independent variables (Fennessey, 1968), and multiple classification analysis (Andrews et al., 1967) is but another variation on this theme. Instead, the difficulty inheres in the categoric condition of the dependent variable. Roughly speaking, ordinary regression generates numerical predictions on an interval or ratio scale, but such numbers would usually be meaningless in reference to categoric dependent variables.

This generalization does not, however, apply to *dichotomous* dependent variables. If, say, 10 respondents disagree with some statement while another 30 agree with it, the weighted mean obtained when "disagree" is coded "0" and "agree" is coded "1" is .75—the proportion agreeing with the statement. Since the mean of a dichotomous variable coded in this manner is interpretable, a regression-like analysis constitutes a viable approach to analyzing the contingency table. In this circumstance, one can estimate how various independent variables affect the *probability* that cases will fall into a given category.[2]

However, a dichotomous dependent variable characteristically violates two assumptions underlying ordinary regression (ordinary least squares: OLS)—that the dependent variable should exhibit (1) constant variance (homoscedasticity) and (2) a normal distribution at each level of the independent variables. Ignoring these assumptions casts a shadow over both the efficiency of the estimated regression coefficients and the validity of the tests of significance.

This much is widely recognized. Considerably less-appreciated among sociologists is the fact that Grizzle, Starmer, and Koch (GSK) have developed some straightforward modifications to ordinary regression which permit one to proceed legitimately with a rather routine

---

analyses in which log-linear modeling has been employed, and this case therefore deserves special attention. Moreover, even for those whose ultimate aim is to understand more general applications, the special case in which log-linear modeling is analogous to regression provides a heuristically useful starting point.

[2] Some statisticians would stress that modeling probabilities (or logits and odds, to be discussed later) is fundamentally different than modeling individual events (Hildebrand et al., 1977). Such a distinction has not proven very fruitful in the analysis of the data soon to be presented. It goes without saying, however, that one should not employ the techniques described here if probabilities and odds are theoretically irrelevant.

analysis without violating such assumptions (Grizzle et al., 1969; Bhapkar and Koch, 1968; Johnson and Koch, 1971; Lehnen and Koch, 1974; and Reynolds, 1977a). Briefly stated, the GSK procedure simply makes use of a generalized (weighted) least squares (GLS) routine—an estimation procedure which has long been available for situations in which the assumption of homoscedasticity is violated; and it substitutes a statistically sound chi-square test of significance for what would be a questionable F-test. Regression analysis proceeds in much the same manner as it does with a continuous dependent variable, and the interpretation of the resulting coefficients is nearly the same as well.

The main features of the GSK approach are well illustrated by the following reanalysis of the data from the National Opinion Research Center (NORC) analyzed by Davis (1974: 201) in his introduction to log-linear modeling. As he presents them, these data from Schwartz's NORC study (1967) are cast in a four-way contingency table. Educational attainment is trichotomized; time and region are dichotomized. Thus, twelve (3 × 2 × 2) subgroups are defined (Table 1, cols. 1–4). Respondents are further classified according to whether or not they agreed that blacks should have equal job opportunity (cols. 6 and 7). For example, 52 of 258 (adult) southern respondents with only grade-school educations favored equal job opportunity in 1946. The proportion who agreed (.20) is recorded in col. 8.

Consider the two related hypotheses that (1) living outside the South increased the likelihood that one would favor equal opportunity but that (2) the magnitude of this regional effect diminished between 1946 and 1963. Given these hypotheses, education might serve as an important control variable. Highly educated people may be more prone than others to favor equal job opportunity; if so, regional differences might merely reflect the higher educational attainment of those outside the South. Similarly, any reduction in the regional difference over time might merely reflect that the educational attainment of Southerners gained on that of others during the 17-year period.

One way to proceed is to examine the regression of the proportion favoring equal job opportunity (J) on time (T), education attainment (E), and region (R), along with appropriate interaction terms. A statistically significant coefficient for region would, of course, lead us to reject the null hypothesis of no regional effect; a statistically significant coefficient for the time-region interaction would lead us to reject the null hypothesis of no change in regional effect over time.

Under one variant of the GSK approach, the data would be organized as in Table 2. The criterion[3] variable (col. 13) is the proportion in each subgroup who agreed that blacks should have equal job opportunity.[4, 5] The coding of the independent variables (main effects) and their interactions (Table 2, cols. 1–12) is explained in the notes to Table 2; such coding schemes have long been available for use in ordinary regression and analysis of variance (Kerlinger and Pedhazur, 1973; Evers and Namboodiri, 1978; Fienberg,

---

[3] The dependent variable is "attitudes towards equal opportunity." However, the quantities which are predicted by the model may take any of several forms—*proportions* agreeing that blacks should have equal opportunity, *odds* of agreeing rather than disagreeing that they should have equal opportunity, etc. These alternate forms are termed *criterion* variables to distinguish them from the *substantive* dependent variable, which remains the same no matter which criterion variable is used. Incidentally, many technical articles on this subject use the term "response variable" instead of "dependent variable."

[4] Note that the regression coefficients obtained from the grouped data in Table 2 are identical to those which would be obtained if one enumerated all 3,579 cases, coding the dependent variable "1" for respondents favoring equal job opportunity, and "0" otherwise—the common sense approach in ordinary dummy regression (Coleman, 1970). Tests of significance, however, would be inappropriate in this case. For comments on the effect of weighting on tests of significance, see Goodman (1975).

[5] This analysis assumes that *each subgroup* constitutes a representative sample of the entire *subpopulation* of individuals eligible for inclusion in the sample and that the observations for the various subpopulations are independent of one another. These assumptions will be met when appropriate probability sampling procedures are used. Also, it should be pointed out that the behavior of significance tests of the sort employed here is unknown when subgroups contain less than about 10 cases; ideally they should contain at least 25 cases (Lehnen and Koch, 1974:286–7).

Table 1. Predictions under Several Models of Some NORC Data

	Independent Variables			Observations						Predictions: GSK[a]			Predictions: Logit and Multiplicative Modeling Based on Log-Linear Modeling[b]				Predictions: General Log-Linear Modeling[c]	
				Frequencies			Alternate Criterion Variables			Linear Probability	Logit Model		Logit Model		Frequencies		Frequencies	
Subpopulations (1)	Time (2)	Education (3)	Region (4)	Total (5)	Agree (6)	Disagree (7)	Proportion Agreeing (8)	Odds of Agreeing (9)	Logits (10)	Proportion Agreeing (11)	Odds of Agreeing (12)	Logits (13)	Odds of Agreeing (14)	Logits (15)	Agree (16)	Disagree (17)	Agree (18)	Disagree (19)
1	1946	Grade	South	258	52	206	.20	.25	−1.38	.20	.27	−1.32	.27	−1.32	54.53	203.47	60.98	202.16
2			Other	676	311	365	.46	.85	−.16	.46	.84	−.18	.84	−.18	308.47	367.53	302.02	368.84
3		High	South	227	64	163	.28	.39	−.93	.28	.38	−.97	.38	−.97	62.27	164.73	68.35	160.66
4			Other	813	439	374	.54	1.17	.16	.54	1.19	.17	1.18	.17	440.73	372.27	434.65	376.34
5		College	South	93	42	51	.45	.82	−.19	.45	.79	−.23	.80	−.23	41.20	51.80	39.79	46.06
6			Other	287	204	83	.71	2.46	.90	.71	2.48	.91	2.49	.91	204.80	82.20	206.21	87.94
7	1963	Grade	South	115	71	44	.62	1.61	.48	.61	1.38	.32	1.38	.32	66.64	48.36	57.97	51.90
8			Other	209	148	61	.71	2.43	.89	.71	2.66	.98	2.69	.99	152.36	56.64	161.03	53.10
9		High	South	157	126	31	.80	4.06	1.40	.78	3.86	1.35	3.85	1.35	124.66	32.24	117.38	37.60
10			Other	466	410	56	.88	7.32	1.99	.88	7.54	2.02	7.52	2.02	411.34	54.66	418.62	49.40
11		College	South	69	55	14	.80	3.93	1.37	.85	6.42	1.86	7.30	1.99	60.69	8.31	65.53	10.62
12			Other	209	201	8	.96	25.13	3.22	.96	12.43	2.52	14.27	2.66	195.31	13.69	190.47	11.38

[a] Calculations performed by the author using GENCAT, a program relying on generalized least squares (GLS) to estimate coefficients for linear probability, logit, or log-linear models. See fn. 25 for source of program.

[b] Calculations performed with ECTA, a program relying on maximum likelihood estimation to estimate coefficients for logit or log-linear models. Compare cols. 10, 13 and 15; also compare cols. 9, 12, and 14.

[c] Predictions based on Davis's (1974) model: (ER) (RJ) (RT) (EJT). Calculated with ECTA.

Table 2. A Saturated Model (Effect Coded)

Constant (1)	T (2)	E_h (3)	E_c (4)	R (5)	TE_h (6)	TE_c (7)	TR (8)	E_hR (9)	E_cR (10)	TE_hR (11)	TE_cR (12)	Criterion Variable (13)	N (14)
1	−1	−1	−1	−1	1	1	1	1	1	−1	−1	.20	258
1	−1	−1	−1	1	1	1	−1	−1	−1	1	1	.46	676
1	−1	1	0	−1	−1	0	1	−1	0	1	0	.28	227
1	−1	1	0	1	−1	0	−1	1	0	−1	0	.54	813
1	−1	0	1	−1	0	−1	1	0	−1	0	1	.45	93
1	−1	0	1	1	0	−1	−1	0	1	0	−1	.71	287
1	1	−1	−1	−1	−1	−1	−1	1	1	1	1	.62	115
1	1	−1	−1	1	−1	−1	1	−1	−1	−1	−1	.71	209
1	1	1	0	−1	1	0	−1	−1	0	−1	0	.80	157
1	1	1	0	1	1	0	1	1	0	1	0	.88	466
1	1	0	1	−1	0	1	−1	0	−1	0	−1	.80	69
1	1	0	1	1	0	1	1	0	1	0	1	.96	209

Notes: The main effects and interactions of the independent variables are represented in effect coding (cols. 1–12). See Kerlinger and Pedhazur (1973:116–31).

Col. 2 Time: −1 = 1946; 1 = 1963.

Cols. 3–4 Education: grade school = −1 −1; high school = 1 0; college = 0 1; E_c = college education; E_h = high school education.

Col. 5 Region: −1 = South; 1 = Other.

Cols. 6–7 Time-Education interaction (col. 2 × 3 and 4).

Col. 8 Time-Region interaction (col. 2 × 5).

Cols. 9–10 Education-Region interaction (cols. 3 and 4 × col. 5).

Cols. 11–12 Education-Region-Time interaction (col. 2 × cols. 9 and 10).

1978). Suffice it to say, the effect coding in Table 2 represents the omitted category with a "−1," whereas dummy coding (more familiar to most sociologists) represents it with a "0." Both effect and dummy coding yield identical predictions, but the interpretation of regression coefficients based on the two systems differs slightly.

Table 2 presents a *saturated* model; that is, all possible main effects and interactions are included as predictors.[6] Note that the number of predictors (12) equals the number of observations to be predicted. Such a model always yields perfect predictions, but is unsatisfactory because it is not parsimonious: comprehending 12 regression coefficients is no easier than comprehending 12 observations. The saturated model would be expressed in equation form as follows:

$$P = \beta_1 + \beta_2 T + \beta_3 E_h + \beta_4 E_c + \beta_5 R + \beta_6 TE_h + \beta_7 TE_c + \beta_8 TR + \beta_9 E_hR + \beta_{10} E_cR + \beta_{11} TE_hR + \beta_{12} TE_cR , \qquad (1)$$

where P is the proportion favoring equal opportunity in a given subgroup; T, E_h, E_c . . . TE_cR are the 11 design variables defined in Table 2; β_1 is the regression constant; β_2 . . . β_5 are the main effects; β_6 . . . β_{10} are the first-order (two-way) interaction effects; and β_{11} and β_{12} are the second-order (three-way) interaction effects. Notice that one category of each independent variable has been omitted, just as in ordinary dummy regression. For instance, the lowest category of education is omitted; high school education is represented by E_h; college education is represented by E_c. The coefficient for the omitted category of a variable equals the negative of the sum of the coefficients for the other categories of that variable. For example, the coefficent for "lowest education" equals $-(\beta_3 + \beta_4)$. In other words, under effect coding, the sum of the effects is defined to be zero just as it is in analysis of variance.[7]

---

[6] One might hypothesize that the step between grade school and high school is about the same as the step between high school and college. If so, one could save a degree of freedom by treating education as a single variable, coding grade school "−1," high school "0," and college "1." Orthogonal coding for purposes of testing linear and polynomial trends is described by Winer (1971) and by some of the references cited in the previous paragraph. Alternative coding schemes may give rise to alternative saturated models.

[7] Under *dummy* coding, the coefficient for the omitted category would be zero, and the coefficients

Table 3 summarizes the effort to account for the observations with a more parsimonious model than the saturated model just described. The variables in each equation are enumerated in col. 2; the chi-square due to error for each equation is recorded in col. 4. (*Disregard cols. 5 through 8.*) The strategy employed involves testing whether the *errors* in prediction for each equation are statistically significant, in which case the model is said to provide a *poor* fit.[8]

Consider Model 1 first. The error under Model 1 (which includes only the regression constant) is analogous to the total (deviation) sum of squares in ordinary regression or the total sum of squares corrected for the grand mean in ANOVA. In other words, this is the variation which one would like to explain with the independent variables. As was pointed out in a previous paragraph, Model 2 (a saturated model) explains all of this variation—that is, it yields no error.

Models 3 through 10 represent the steps of a backward selection procedure aimed at discovering which terms of the saturated model can be eliminated without introducing statistically significant error into the predictions.[9,10] Model 3 eliminates only the two time-education-region interaction terms from the saturated design. The chi-square due to *error* generated by this restriction is a mere .9 out of the 1406.6 yielded by Model 1. (In other words, 99.94% of the error variation under Model 1 is explained by Model 3.[11]) Since this error is not statistically significant, the null hypothesis that there is no second-order (three-way) interaction in the population is not rejected. This implies that TE_hR and TE_cR may be excluded from all subsequent models.

Model 4 eliminates all first-order interaction terms as well. In this case the chi-square due to error is so large (35.4, 7 df) as to require that the null hypothesis be rejected. Therefore, at least one of the first-order interactions must be retained in the final model. Models 5–10 reveal that the only first-order interaction terms which can be eliminated are E_hR and E_cR.[12] The chi-square due to error for Model 10 is only 2.1, which with four degrees of freedom is obviously nonsignificant, bespeaking a good fit. Thus, the final model includes a constant, main effects, and the TR, TE_h and TE_c interaction terms.

The excellent correspondence between the observed proportions and the predictions under Model 10 may be seen by comparing cols. 8 and 11 in Table 1. Notice, incidentally, that the largest error is found where the fewest cases are concentrated—in subgroup 11. This tends

---

for the other categories would provide comparisons with the omitted category rather than the unweighted mean. (See Kerlinger and Pedhazur, 1973:chap. 7.) In keeping with my ultimate aim, I have purposely omitted subscripts *i, j, k* and *l*, which would normally appear in equations presented in this paper. Given the coding in Table 2, this should not result in any confusion.

[8] When hypotheses are developed ex post facto, one should make adjustments to compensate for capitalizing on Type I errors. See Fienberg (1978), Haberman (1978), and Kerlinger and Pedhazur (1973) for comments on how to make these adjustments. As usual, the most desirable approach is to state one's hypotheses in advance.

[9] In the effort to settle on an acceptable model, one may instead use forward selection, examine the coefficients of saturated models (Goodman, 1971:45), or study the standardized residuals (Haberman, 1978).

[10] Even trivial effects may be statistically significant when sample sizes are large. Under these circumstances, one might well make better sense of the data by omitting substantively trivial interactions from one's model.

[11] It must be emphasized that "variance explained" in this context means something fundamentally different than it does in ordinary regression and ANOVA (and for this reason Goodman's efforts [1971] to come up with a figure comparable to multiple R in ordinary regression do not seem very useful). In contingency table analyses such as those described in this article, individual variation within cells is not included in the total variation to be explained. This has the effect of eliminating an important component of what would be considered error variance in an individual-level analysis. Therefore, the variance explained in contingency table analyses is invariably higher than in ordinary regression analyses of individual-level data. In fact, it is always possible to explain all the (between-cell) variance by including enough interaction terms (though such models are usually uninteresting by virtue of their lack of parsimony). This implies that even a "perfectly fitting" model of a contingency table would not explain the behavior of individuals perfectly.

[12] Notice that all interactions of a kind are treated as a unit. For example, either both education-region interaction terms are included in each model or both are excluded.

Table 3. Selecting the Best Model under Three Alternative Statistical Techniques

(1) Model No.	(2) Independent Variables in the Model[a]	(3) df	(4) X^2 Error[b] GSK Linear	(5) X^2 Error[c] GSK Logit	(6) X^2 Error[d] MLE Logit	(7) Log-linear Models Used for Logit Modeling	(8) Extended Representation of Symbols in Col. 7[e]
1	k (constant only)	11	1406.6*	516.9*	738.0*	(TER) (J)	J
2	k, T, E_h, E_c, R, TE_h, TE_c, TR, E_hR, E_cR, TE_hR, TE_cR	0	0.0	0.0	0.0	(TERJ)	J JT JE JR JTE JTR JER JTER
3	k, T, E_h, E_c, R, TE_h, TE_c, TR, E_hR, E_cR	2	0.9	6.7	6.9	(TER) (JTE) (JTR) (JER)	J JT JE JR JTE JTR JER
4	k, T, E_h, E_c, R	7	35.4*	26.7*	26.9*	(TER) (JT) (JE) (JR)	J JT JE JR
5	k, T, E_h, E_c, R, E_hR, E_cR	5	34.0*	25.1*	24.9*	(TER) (JER) (JT)	J JT JE JR JER
6	k, T, E_h, E_c, R, TR	6	13.3*	21.0*	21.7*	(TER) (JTR) (JE)	J JT JE JR JTR
7	k, T, E_h, E_c, R, TE_h, TE_c	5	23.7*	14.0*	14.3*	(TER) (JTE) (JR)	J JT JE JR JTE
8	k, T, E_h, E_c, R, TR, E_hR, E_cR	4	12.8*	19.5*	20.0*	(TER) (JTR) (JER)	J JT JE JR JTR JER
9	k, T, E_h, E_c, R, TE_h, TE_c, E_hR, E_cR	3	21.9*	12.3*	12.5*	(TER) (JTE) (JER)	J JT JE JR JTE JER
10	k, T, E_h, E_c, R, TE_h, TE_c, TR	4	2.1	8.0	8.3	(TER) (JTE) (JTR)	J JT JE JR JTE JTR

Note: The asterisks in cols. 4–6 indicate that the models give rise to statistically significant errors in prediction ($\alpha = .01$).

[a] k represents the regression constant.

[b] Minimum Neyman chi-square statistic.

[c] Minimum Neyman chi-square statistic.

[d] Likelihood-ratio chi-square statistic.

[e] The (TER) term in col. 7 is not expanded in col. 8 because it does not involve the substantive dependent variable J. However, the (TER) term *is* part of the models described in col. 8. Cols. 7 and 8 are equivalent to one another (see the text). Col. 8 is provided merely to render the correspondence between cols. 2 and 7 easy to see.

Table 4. Coefficients for a Linear Probability Model of the NORC Data

	Effect Coded				Dummy Coded[b]	
	Full Samples (1)	S.E. (2)	1946[a] (3)	1963 (4)	1946 (5)	1963 (6)
1. Constant	.62		.44	.80	.20	.62
2. Time (T)						
1946	−.18	.01				
1963	.18	.01				
3. Education (E)						
grade school	−.12	.01	−.11	−.13	.00	.00
high school	.00	.01	−.03	.03	.08	.16
college	.12	.01	.14	.10	.25	.23
4. Region (R)						
South	−.09	.01	−.13	−.05	.00	.00
Other	.09	.01	.13	.05	.26	.10
5. TR interaction						
1963 × South	.04	.01				
1963 × Other	−.04	.01				
6. TE interaction						
1963 × grade school	−.01	.01				
1963 × high school	.03	.01				
1963 × college	−.02	.01				

Note: Based on Model 10 in Table 3.

[a] The coefficients for 1946 and 1963 may be obtained from those based on the entire sample. See Kerlinger and Pedhazur (1973:251–60).

[b] Notice that identical predictions are obtained from the dummy-coded and the effect-coded equations. For example, the prediction for college-educated people in the South in 1963 is .85 (.80 + .10 − .05) according to the equation in col. 4; likewise, it is .85 (.62 + .23 + .00) according to the equation in col. 6.

to be the case because the procedure weights each observation (proportion) by the *inverse* of its binomial-based variance, and the *smaller* the number of cases on which a proportion is calculated, the larger its variance.

As in most regression analyses, the question at hand requires that one focus on the regression coefficients (Table 4, col. 1), not merely on tests of significance. In this case, the hypotheses would lead one to pay particular attention to the effects of region. The algebraic difference between the coefficient for South (−.09) and Other (.09) is .18. This may be interpreted as follows: living outside the South, rather than in it, raised the probability of favoring equal job opportunity by .18, controlling for time and education.

However, the statistically significant time-region interaction term indicates that the regional effect just mentioned changed between 1946 and 1963. Therefore, it is useful to write separate regression equations for the two time periods (Kerlinger and Pedhazur, 1973: 251–6). Notice that the algebraic difference of the regional coefficients is .26 in the 1946 equation, but only .10 in the 1963 equation (Table 4, cols. 3 and 4). Notice, also, that if dummy rather than effect coding had been employed, precisely the same message would have been carried by the regression coefficients (cols. 5 and 6).

Finally, the answer to the questions which gave rise to this contingency table analysis may be stated as follows:

> In 1946, living outside the South (rather than in it) added .26 to the probability of favoring equal job opportunity (controlling for educational attainment).[13] By 1963, this regional effect had dropped to .10.

LOGIT MODELS

Logit modeling closely parallels linear probability modeling. For our purposes, there is only one essential difference to bear in mind: instead of using the *proportion in category 1* as the criterion variable, it uses the logit—*the natural logarithm of the odds of falling into category 1 rather than category 2* of the dependent variable (Bishop et al., 1975: 22–3). In the NORC data analyzed above, for instance, among adult Southerners with only a grade school education in 1946, 52 agreed that blacks should have equal job opportunity while 206 did not agree (Table 1, cols. 6 and 7). The odds of agreeing, then, were 52:206, or (in reduced form) .25:1 (col. 9). The logarithms of the odds (the *log-odds* or the *logits*) appear in Table 1, col. 10.

---

[13] As in ordinary regression and path analysis, the estimates of effect may be biased if the model is improperly specified—that is, if the equation omits variables which both correlate with at least one independent variable and affect the dependent variable.

Readers may find it instructive to begin by comparing the conclusion in the previous section with that based on the logit model soon to be developed in this section:

> In 1946, living outside the South (rather than in it) added 1.14 to the logit of favoring equal job opportunity (controlling on education). By 1963, it was adding only .66 to the logit.

As will be demonstrated later, this finding may be restated in everyday language as follows:

> In 1946, living outside the South (rather than in it) multiplied the odds of favoring equal job opportunity by 3.13 (controlling on education). By 1963, this factor had dropped to 1.93.

The equation underlying a saturated logit model of the NORC data is as follows:

$$\begin{aligned}\ln(\Omega) = {} & \lambda_1 + \lambda_2 T + \lambda_3 E_h + \lambda_4 E_c + \lambda_5 R \\ & + \lambda_6 TE_h + \lambda_7 TE_c + \lambda_8 TR \\ & + \lambda_9 E_h R + \lambda_{10} E_c R + \lambda_{11} TE_h R \\ & + \lambda_{12} TE_c R\,, \qquad (2)\end{aligned}$$

where Ω is the odds of favoring rather than opposing equal opportunity; λ_1 is the regression constant; $\lambda_2 \ldots \lambda_5$ are the main effects; $\lambda_6 \ldots \lambda_{10}$ are the first-order interaction effects; λ_{11} and λ_{12} are the second-order interaction effects; and T . . . TE_cR are defined in Table 2. Notice that the form of this equation matches that of Equation 1 except that the criterion variable here is the logit. The λ's are regression coefficients which serve the same function as the β's in Equation 1.

Unsurprisingly, then, logit modeling can be conducted in the same manner as linear probability modeling. The data are again organized as in Table 2, except that the logits (Table 1, col. 10) serve as the criterion variable. The effort to find a parsimonious model proceeds with the same logic as before (refer back to Table 3, cols. 1, 2, 3, *and* 5). Again, it turns out that the only interaction terms which can be eliminated are E_hR, E_cR, TE_hR, and TE_cR (Model 10). The correspondence between the observed and predicted logits may be seen by comparing cols. 10 and 13 of Table 1.

As was the case in the previous section, the hypotheses at hand require that one examine the regression coefficients associated with the logit model (Table 5, col. 1); the TR interaction again renders it useful to write separate equations for the two time periods (cols. 3 and 4); and, of course, the coefficients based on dummy coding (cols. 5 and 6) carry precisely the same message as those based on effect coding. The two coefficients for region (1.14 and .66) appear in the illustrative conclusion at the beginning of this section.

It is possible to transform equations for the logit so that the results are instead stated in terms of odds. If one sets each λ_i in Equation 2 equal to $\ln(\gamma_i)$, every term in Equation 2 is given in terms of natural logarithms. At this point, one may take advantage of three mathematical principles: (1) if $x=y$, $e^x=e^y$; (2) $e^{\ln(x)}=x$; and (3) $e^{\ln(x)+\ln(y)}=x \cdot y$. By applying the exponential (antilog) function to both sides of Equation 2, the following multiplicative equation is obtained:

$$\begin{aligned}\Omega = {} & \gamma_1 \cdot \gamma_2^{T} \cdot \gamma_3^{E_h} \cdot \gamma_4^{E_c} \cdot \gamma_5^{R} \cdot \gamma_6^{TE_h} \cdot \gamma_7^{TE_c} \cdot \gamma_8^{TR} \\ & \cdot \gamma_9^{E_hR} \cdot \gamma_{10}^{E_cR} \cdot \gamma_{11}^{TE_hR} \cdot \gamma_{12}^{TE_cR}\,, \qquad (3)\end{aligned}$$

where Ω again represents the odds of favoring equal job opportunity rather than opposing it; γ_1 is a constant; the other γ's are main and interaction effects; and the exponents T . . . TE_cR are the design variables defined in Table 2.[14] (See

---

[14] Most people, including Goodman, write the design variables in Equation 2 (T . . . TE_cR) as superscripts rather than as multiplicands. This practice obscures the relationship between Equation 2 and Equation 3 and it masks the similarity between Equation 2 and Equation 1.

Table 5. Coefficients for Logit and Multiplicative Models of the NORC Data

	Logit Model		Logit Model Effects Coded		Logit Model Dummy Coded		Multiplicative Model Dummy Coded		Logit Model from Log-Linear Equation[a]	
	(1) Coefficients	(2) S.E.	(3) 1946	(4) 1963	(5) 1946	(6) 1963	(7) 1946	(8) 1963	(9) Coefficients	(10) S.E.
1. Constant	.62	.06	−.27	1.51	−1.32	.32	.27	1.38	.64	.06
2. Time (T)										
1946	−.89	.06							−.91	.06
1963	.89	.06							.91	.06
3. Education (E)										
grade school	−.67	.07	−.48	−.86	.00	.00	1.00	1.00	−.69	.07
high school	.02	.07	−.13	.17	.35	1.03	1.42	2.80	.00	.07
college	.65	.07	.61	.69	1.09	1.55	2.97	4.71	.69	.09
4. Region										
South	−.45	.05	−.57	−.33	.00	.00	1.00	1.00	−.45	.06
Other	.45	.05	.57	.33	1.14	.66	3.13	1.93	.45	.06
5. TR interaction										
1963 × South	.12	.05							.12	.06
1963 × Other	−.12	.05							−.12	.06
6. TE interaction										
1963 × grade school	−.19	.07							−.21	.07
1963 × high school	.15	.07							.13	.07
1963 × college	.04	.07							.08	.09

Note: These coefficients are based on Model 10 in Table 3. The multiplicative model coefficients may be used as follows: In 1946, among high school graduates outside the South, the odds of favoring equal opportunity, instead of opposing it, were (.27 * 1.42 * 3.13) = 1.174 (see Table 1, col. 9).

[a] Disregard cols. 9 and 10 until after log-linear modeling has been discussed.

Goodman, 1972:35.) In Equation 3, the coefficients estimate how changes in the independent variables *multiply the odds* of falling into category 1 rather than category 2, holding other variables constant. The coefficients of the multiplicative model which is equivalent to the final logit model above are contained in cols. 7 and 8 of Table 5, and the notes to that table illustrate how to use them. Notice that the multiplicative effect for region in 1946 (3.13) equals $e^{1.14}$, where 1.14 is the corresponding coefficient in the logit model (col. 5). The multiplicative effects for region appear in the second illustrative conclusion stated at the beginning of this section.

Technical explications of logit modeling appear in Theil (1970), Ashton (1972), and Plackett (1974). Further comparisons between linear probability and logit modeling will be set forth later.

LOG-LINEAR MODELS

Log-linear modeling, *as it applies to situations in which one variable is considered dependent on others*, is easily understood in terms of the approach just outlined. It proceeds with the same logic and arrives at the same conclusion as logit (or multiplicative) modeling. In other words, the results of the log-linear analysis which is about to be described virtually match those stated at the beginning of the previous section.

Log-linear models derive their name from the fact that they predict the *logarithm* of a certain quantity using a *linear* combination of predictor variables. In this sense, even logit models such as Equation 2 might be termed "log-linear."[15] However, the most general saturated log-linear model for the NORC data is given by the following equation:

$$\begin{aligned}\ln(F) = {} & \lambda^*_{13} + \lambda^*_{1}J + \lambda^*_{14}T + \lambda^*_{15}E_h \\ & + \lambda^*_{16}E_c + \lambda^*_{17}R + \lambda^*_{2}JT \\ & + \lambda^*_{3}JE_h + \lambda^*_{4}JE_c + \lambda^*_{5}JR \\ & + \lambda^*_{18}TE_h + \lambda^*_{19}TE_c + \lambda^*_{20}TR \\ & + \lambda^*_{21}E_hR + \lambda^*_{22}E_cR + \lambda^*_{6}JTE_h \\ & + \lambda^*_{7}JTE_c + \lambda^*_{8}JTR + \lambda^*_{9}JE_hR \\ & + \lambda^*_{10}JE_cR + \lambda^*_{23}TE_hR \\ & + \lambda^*_{24}TE_cR + \lambda^*_{11}JTE_hR \\ & + \lambda^*_{12}JTE_cR\,, \end{aligned} \tag{4}$$

---

[15] See Haberman (1974:313) and (1978:292).

where F is the frequency in a given cell of the full table (Table 1, cols. 6 and 7); J, T . . . JTE_cR are design variables calculated in the same manner as those in Table 2; and the λ^*'s are regression coefficients whose interpretation will be discussed shortly. The utility of writing the subscripts (1 . . . 24) out of sequence will become apparent to those who read the Appendix.

Equation 4 is a regression equation; its coefficients can be estimated with regression techniques (Grizzle and Williams, 1972; see also Haberman, 1974). Nonetheless, one peculiarity sets it apart from virtually all regression equations which have appeared in sociological literature: *it has no substantive dependent variable*. Unlike the proportions and logits considered above, the criterion variable in Equation 4 is not associated with any of the four substantive variables. In fact, "attitudes towards equal job opportunity" (J)—the *dependent* variable in the hypotheses at hand—appears in 12 of the 24 terms on the *predictor* side of the equation.

Nevertheless, as Bishop (1969) has demonstrated, there is a close relationship between log-linear and logit modeling. The logit modeling described in the previous section necessarily takes for granted the associations observed among *independent* variables, just as ordinary regression does. As will soon be illustrated with the NORC data, when log-linear modeling is conducted so as to include all observed associations among independent variables, the resulting log-linear equation includes the coefficients of a logit model closely related to Equation 2. More precisely, as is demonstrated in the Appendix, when Equation 4 is utilized to predict the logit of favoring equal job opportunity (J), those terms which do not involve J drop out, and the following equation results:

$$\begin{aligned}\ln(\Omega) = {} & 2\lambda^*_1 J + 2\lambda^*_2 JT + 2\lambda^*_3 JE_h \\ & + 2\lambda^*_4 JE_c + 2\lambda^*_5 JR + 2\lambda^*_6 JTE_h \\ & + 2\lambda^*_7 JTE_c + 2\lambda^*_8 JTR \\ & + 2\lambda^*_9 JE_hR + 2\lambda^*_{10} JE_cR \\ & + 2\lambda^*_{11} JTE_hR + 2\lambda^*_{12} JTE_cR\ , \quad (5)\end{aligned}$$

where $\ln(\Omega)$ is the logit of favoring equal job opportunity, and the other terms are defined as above.

Equation 5 corresponds closely to the logit model given in Equation 2. The J in Equation 5 always equals unity and can therefore be ignored in comparing the two equations[16]; and $2\lambda^*_t$ in Equation 5 equals λ_t in Equation 2. Thus, *to obtain the coefficients of a logit model from an appropriate log-linear model, double the coefficients of those terms in the log-linear equation which involve the (substantive) dependent variable, and disregard the others*.[17] For example, two times the coefficient for J *in Equation 4* equals the regression constant *in Equation 2*; two times the coefficient for JR equals the main effect of R on the logit of J; and two times the coefficient for JTE_cR equals the TE_cR interaction in an equation for the logit of J.

On this basis, log-linear modeling in situations where one variable is considered dependent on others can be illustrated with the NORC data. The models described in Table 3, col. 8 represent the steps of a backward selection procedure equivalent to the one in col. 2, employed above in logit and linear probability modeling. Model 1 includes only the regression constant of a logit model; recall that the regression constant is derived from the coefficient for J in a log-linear model. Model 1 provides a measure of the error variation to be explained with independent variables and their interactions (col. 6). Model 2 is the saturated model in Equation 4, with only those terms involving J stated. Model 3 eliminates the highest-order interaction on the logit of J without introducing significant error. Model 4 eliminates all but the main effects of T, E, and R on the logit of J; the null hypothesis is rejected. Models 5 through 10 test which interactions can be omitted without rendering the chi-square due to error statistically significant. Once again, Model 10 is clearly the best.

---

[16] The question at hand concerns the logit or odds that a respondent *agreed* that blacks should have equal opportunity. This is represented by J=1. If, instead, one wanted the logit or odds that a respondent *disagreed*, J would always equal −1.

[17] Under certain circumstances, it is not necessary to double the coefficients. See Gokhale and Kullback (1978:113).

Log-linear models are rarely represented as in Table 3, col. 8. Instead, symbols such as those in col. 7 are utilized. To avoid confusion, bear in mind two facts about these symbols. First, when, say, JER is enclosed in parentheses, the model includes not only the three-way interaction JER, but also all lower-order effects involving the same variables: JE, JR, ER, J, E, and R.[18]

Second, notice that all the log-linear models *include* (TER). This fact might be perplexing to readers who recall that TE_hR and TE_cR were *omitted* from most of the logit and linear probability models considered in previous sections. In reality, however, there is no inconsistency. Bear in mind that it is the *J*TER term in a log-linear model which corresponds to the TE_hR and TE_cR terms in a logit model, and that JTER is omitted from Models 3 through 10 (Table 3, col. 8) just as TER was omitted from the logit and linear probability models described in col. 2. The (TER) term is automatically included in all log-linear models in this analysis because doing so takes all the observed associations among independent variables for granted, just as in ordinary regression. Thus, the inclusion of (TER) is of no substantive significance.

The coefficients for log-linear Model 10 are presented in Table 6. As is illustrated in the notes to Table 6, these coefficients can be used to predict the *cell frequencies* in Table 1, cols. 6 and 7 (see cols. 16 and 17). However, the hypotheses in question require that one focus, instead, on the logit model which can be derived from this log-linear equation. As was stated above, the coefficients of such a logit model are obtained by doubling the coefficients of terms involving J (preceded by asterisks in

Table 6. A Log-Linear Model of the Cell Frequencies in the NORC Data

	(1)
Grand Mean	4.549
Time (T)	
1963	−.417
1946	.417
Education (E)	
grade school	.213
high school	.397
college	−.610
Region(R)	
South	−.481
Other	.481
Job Attitudes (J)	
*agree	.321
don't agree	−.321
TR interaction	
1963 × South	.116
1963 × Other	−.116
TE interaction	
1963 × grade school	−.059
1963 × high school	.052
1963 × college	.007
*EJ interaction	
grade school × agree	−.344
high school × agree	−.001
college × agree	.345
*RJ interaction	
South × agree	−.226
Other × agree	.226
*TJ interaction	
1963 × agree	.456
1946 × agree	−.456
ER interaction	
South × grade school	.067
South × high school	−.081
South × college	.014
*TRJ interaction	
1963 × South × agree	.059
1963 × Other × agree	−.059
*TEJ interaction	
1963 × grade sch. × agree	−.105
1963 × high sch. × agree	.066
1963 × college × agree	.040
TER interaction	
1963 × South × grade sch.	.051
1963 × South × high school	.015
1963 × South × college	−.067

* Terms preceded by an asterisk are relevant to a logit model. Other terms merely account for the exact number of cases in each subpopulation. (Note that, because (TER) is included in the log-linear model, the number of cases predicted for each subpopulation (Table 1, cols. 16 and 17) equals the observed number (Table 1, col. 5).

Example: To estimate the cell frequency for Southerners with a grade-school education who agreed that blacks should have equal opportunity in 1963, first estimate the log of the frequency using the coefficients above. $\ln(F) = 4.549 - .417 + .213 - .481 + .321 + .116 - .059 - .344 - .226 + .456 + .067 + .059 - .105 + .051 = 4.20$. $e^{4.20} = 66.69$. Therefore, the predicted frequency is 66.69 (Table 1, col. 16; the discrepancy is due to rounding error). The observed frequency was 71 (col. 6).

---

[18] Some authors use different symbols to indicate the distinction between the three-way effect JER and the family of effects (JER): JER, JE, JR, ER, J, E, and R. For example, many would use J*E*R instead of (JER). Others do not bother to make the distinction. It turns out that (JER) or J*E*R is a convenient symbol because most log-linear modeling is hierarchical: high-order effects such as JER can only be included if lower-order effects involving J, E, and R are also included.

Table 6). These coefficients are presented in Table 5, col. 9, where they may be easily compared with those calculated in the previous section (col. 1). Since the coefficients are quite similar, the substantive conclusion of this log-linear analysis is virtually identical to the one presented at the beginning of the section on logit modeling. The coefficients are not all identical because the GSK procedure employed in the previous section was based on generalized least squares, while the log-linear modeling in this section was based on maximum-likelihood estimation. This difference will be treated further in the next section.

The *automatic* inclusion of (TER) in all the above log-linear models is the subject of some controversy. Unlike ordinary regression, log-linear modeling is flexible enough to test null hypotheses about the associations among independent variables—except, of course, if the observed associations are artifacts of one's sampling design.[19] As is explained in the latter portion of the Appendix, the derivation of Equation 5 does *not* depend on taking the associations among independent variables for granted. Bishop (1969) would take advantage of this fact. She would test those associations among independent variables which were not affected by the study design and drop the nonsignificant ones. Doing so would increase the precision of the estimates, she argues. Presumably this is the case because fewer predictors leave more degrees of freedom to the error variation. She would still interpret the coefficients as effects on the logits; for example, two times the coefficient for RJ is the effect of region on the logit of favoring equal job opportunity, controlling on other variables in the equation.[20]

Fienberg (1977:80) has suggested another reason why one might omit appropriate nonsignificant interactions among independent variables: doing so may relieve the computational problems caused by empty subgroups which frequently arise when several polytomous independent variables are cross-classified. For further advice on this subject, see Fienberg (1977) and Haberman (1978).

However, many other competent statisticians argue strongly for including the associations among independent variables, even when the statistical assumptions of general log-linear modeling do not require it (Goodman, 1972:38; 1973b; 1979b; Gokhale and Kullback, 1978:114; Haberman, 1978:chap. 5). Taking these associations for granted (that is, including them in the model) insures that the number of respondents in each subgroup is reproduced. Consider, for example, the cell frequencies predicted in the above log-linear analysis (Table 1, cols. 16–7). It was predicted that 195.31 respondents would agree, while 13.69 would disagree, in subgroup 12. These numbers sum to 209.00—precisely the number of respondents who were actually in this subgroup (col. 5). If the (TER) term were not included, the predictions would not generally sum to the observed frequencies in each subgroup. Such a model would end up predicting not only the logits in each subgroup but also how many respondents were in each subgroup. Most researchers, however, would find the latter predictions utterly irrelevant to the theoretical questions which motivated their research. In short, including the interactions among independent variables renders the equation fully comparable to regression. Another advantage of including all the interactions among independent variables is that doing so allows one to calculate the unexplained variation in the criterion variable, as in Model 1.

The above explication of log-linear modeling has focused on contingency tables in which one variable is considered dependent on others—a perspective which pervades contemporary sociological analysis. It must be emphasized, however, that log-linear modeling can handle a host of research questions which do not adopt this perspective. In the *statistical* literature on log-linear modeling, it has been more common for hypotheses to focus on the independence or conditional independence of variables without stipulating causal priority. For example, the fact that Model 10 (Table 3) includes (JTR) but not (JT*E*R) implies that (JTR) was not conditional on E in the population. (More precisely, the null hypothesis that the joint distribution of T, R, and J is independent of E is not rejected.) Similarly, the "T" in the (JTR) term implies that RJ, the association between region and attitudes towards equal opportunity, *was* conditional on time (within each level of education).[21] Further illustrations of this approach will appear later, in a critique of Davis's (1974) analysis of the NORC data. See also Hauser et al. (1975) and Stolzenberg and D'Amico (1977), Spilerman and Miller (1977), and Stolzenberg (1977). How to interpret log-linear models in terms of independence and conditional independence is discussed by Goodman (1970), Bishop et al. (1975:33–4; 45–7), and Haberman (1978:198–207; 259–61). Some other innovative applications of log-linear modeling which fall outside the scope of this paper may be found in Goodman (1969; 1979a), Duncan (1979a; 1979b), and Schuessler (1978).

---

[19] How sampling design affects the range of legitimate log-linear models is considered later.

[20] This point was confirmed in a telephone conversation with Bishop. She does not deal with the interpretation of coefficients in her article.

SOME COMPARISONS

It is useful to begin by reviewing some excerpts from the conclusions which were yielded by each of the techniques just outlined:

(1) living outside the South added .26 to the probability of favoring equal job opportunity in 1946 (controlling on education);
(2) living outside the South added 1.14 to the logit of favoring equal job opportunity in 1946 (controlling on education); and
(3) living outside the South multiplied the odds of favoring equal job opportunity by 3.13 in 1946 (controlling on education).

The first conclusion was derived with linear probability modeling; the second, with logit modeling. Whether the logit model was obtained directly (with the GSK technique) or indirectly (with log-linear modeling), the conclusion remained virtually the same. The third conclusion is mathematically equivalent to the second; the exponential function was employed merely to give the coefficients an interpretation as effects on the odds rather than on the logits.

Logit models have a distinguished history in statistics and are quite commonplace in biological assay. However, most sociologists are far more at ease with such concepts as *probability* and *percentage difference* than with *logit*. One might well expect, then, that linear probability modeling would be the most popular of the above techniques for analyses in which one variable is considered dependent on others. However, it has seldom been used in the discipline of sociology (Giles et al., 1976; Rushing, 1978). Among the reasons for this is that many sociologists believe that log-linear modeling is more statistically respectable than alternatives. With few exceptions, however, sociological articles on this subject have not compared log-linear modeling with the most sophisticated alternatives for contingency table analysis, but rather with ordinary regression using a statistically illegitimate dummy dependent variable (Goodman, 1975; Knoke, 1975; Gillespie, 1977).[22] Naturally, log-linear modeling comes out on top statistically in such a comparison. However, such comparisons are for the most part irrelevant in this case.

Before dwelling on differences between the techniques described above, it would be well for us to review the many similarities between them. First, they are all based on regression equations (1, 2, and 4). True, the general log-linear model (Equation 4) is fundamentally different from others in that its criterion variable is not substantive. Nevertheless, it *is* a regression equation, and it can be utilized to derive yet another regression equation (5) which does have a substantive criterion variable. However, though all of the above techniques amount to regression, ordinary regression cannot be utilized to calculate estimates without doing violence to assumptions about the dependent variable. In the relatively new parametric techniques described above, statisticians have given us procedures for estimating regression coefficients which are appropriate despite the fact that the criterion variables exhibit heteroscedasticity and a nonnormal distribution. They have not, however, even pretended to offer a radical new conception of how to analyze tables in which one variable is considered dependent.

---

[21] The expresssion "within each level of education" is an important part of the interpretation. As will be explained later, investigators frequently misinterpret terms such as JR as zero-order associations or as partial associations controlling on only one of the other variables in the table.

[22] This is not to suggest that Goodman was unaware of the existence of the GSK technique; also, Gillespie does focus on some shortcomings of log-linear modeling in his article.

This, then, suggests a second similarity. The above techniques all yield statistically respectable estimates—BAN estimates (best asymptotic normal). The sampling distributions are understood well enough so that confidence intervals can be calculated (based on the assumption of a very large sample size). In this respect, they are superior to their nonparametric predecessors. Consider, for example, the results of an analysis using gamma: in 1946, the partial gamma between region and job bias (controlling on education) was .52; by 1963, it had dropped to .30. Such a nonparametric analysis does not allow one to estimate the precision of the estimates nor (equivalently) to test the null hypothesis that there was no difference over time.[23] The new techniques, on the other hand, permit rigorous tests of interaction.

Another advantage which all the techniques share is that their coefficients are interpretable as precise measures of effect, whereas nonparametric measures of association usually are not. Consider, for example, a literal statement of the meaning of the partial gamma presented in the previous paragraph: errors in predicting whether or not people favored equal opportunity in 1946 are reduced 52% (from chance level) when information on region is taken into account (controlling for education). In this sense, gamma is a measure of proportional reduction of error, not an estimate of effect. One is not misled by gamma, but one does not end up with a conventional measure of effect.

Still another advantage which all three share is that they permit comparisons of the effects of independent variables. For instance, one can easily compare the effects of education and region in the final linear probability model (Table 4, col. 5): in 1946, the direct effect of having completed college rather than just grade school was to raise the probability of favoring equal opportunity by .25; this was about the same as the direct effect of living outside the South (.26). A more common approach is to compare the variation explained by each independent variable. However, this can be misleading.

Yet another similarity which deserves mention is that, in the above analyses, all the techniques yielded models containing identical main effects and interaction terms (Table 3, Model 10). This is not always the case. However, it is often the case because the criterion variables (logits and proportions) correlate highly with one another when most of the proportions fall between .25 and .75 (Goodman, 1975; Fienberg, 1977:22). In the NORC data, the correlation is .97 (between cols. 8 and 10 in Table 1). If more of the proportions had fallen at the extremes, the interaction terms in the final models might have differed somewhat from one another. This, in itself, would not imply that either model was superior in the sense of being closer to the "truth"; rather, it would merely reflect the fact that modeling proportions is not synonymous with modeling logits.

Finally, in a negative vein, it should be pointed out that the techniques are similar in their inability to handle continuous independent variables. Further comments on this issue will be set forth later.

Now let us consider three differences between the techniques. One salient difference involves the method of estimating coefficients. The coefficients of linear probability models and also of logit models tested with the GSK technique must be estimated with generalized least squares (GLS), while those of log-linear models (and of logit models derived from them) may be based on maximum-likelihood estimation (ML).[24] The importance of this difference remains to be demonstrated, however. True, Grizzle, Starmer, and Koch (GSK) (1969:502) did once acknowledge that their GLS coefficients might have less precision than ML estimates, and Goodman (1971:60) was quick to point out this admission in comparing his log-linear approach with their approach. However, as was pointed out above, as samples become large, the GLS and ML estimates in question take on the same properties (BAN). Furthermore, Grizzle and Williams (1972:139–40) have more recently argued that each of the estimation procedures enjoys certain advantages over the other, and they have adduced limited evidence to the effect that their GLS estimates are as good as ML estimates based on small samples. It appears that one should not take for granted the superiority of ML estimates without further evidence. And even if one *were* to establish their statistical superiority, it would not necessarily follow that the magnitude of superiority would make any practical substantive difference. One must also take into account other differences in deciding which technique to employ in a given situation.

---

[23] There *are* conventional tests of significance of *zero-order* gammas. Also, the GSK technique itself can be used to obtain estimates of the standard errors of numerous measures of association and partial association: gamma, Yule's Q, Kendall's tau$_b$ and tau$_c$, Somer's D, and Goodman and Kruskal's tau's (Kritzer, 1977).

It is, in fact, easy to illustrate how restricting oneself to ML estimates (because of their alleged superiority) might do injustice to one's analysis. Among sociologists, the most widely distributed computer program for log-linear modeling (Goodman's ECTA), while producing ML estimates, restricts analysis to hierarchical modeling (see fn. 18).[25] The emphasis on hierarchical models is usually explained by the fact that ML estimates for nonhierarchical models are more difficult to calculate than those for hierarchical models (Haberman, 1978) and because they are often difficult to interpret substantively (Bishop et al., 1975). However, it would be a mistake to presume that only hierarchical models of contingency tables deserve consideration. Suppose, for example, one wished to treat the trichotomous variable "education" in the NORC data as if it exerted a linear effect on the logit or probability of favoring equal opportunity. Under this circumstance, one might let "−1" represent elementary education; "0," high school education; and "1," college education. One might also calculate interaction terms by multiplying education as coded here times other independent variables. Such a model might well prove useful in analyzing sociological data. However, it does *not* correspond to a *hierarchical* log-linear model (Haberman, 1978:303–5).[26] The GSK procedure, as well as other techniques based on GLS estimation, can routinely test this and a wide range of other models. This, then, provides an example of how the alleged statistical advantages of ML estimates might be offset by other considerations.

A second important difference between the techniques concerns their criterion variables. The range of the criterion variable of logit models is unrestricted. In other words, the log of the odds (logit) is free to vary between negative and positive infinity. In contrast, since the proportion of cases in a given category of a dependent variable cannot logically fall below zero nor above 100%, the range of the criterion variable of linear probability models logically is restricted to [0,1]. The significance of this difference is that linear probability modeling might produce out-of-range (illogical) estimates, while logit modeling cannot. Advocates of logit modeling consider this a major advantage of their approach to contingency table analysis. However, the importance of this fact should not be overestimated. Linear probability modeling seldom produces out-of-range predictions in practice. To wit, a reanalysis of most of the published tables appearing in sociological articles listed below (under "References") resulted in no such predictions in the best models. Generally, such predictions are most likely to occur when one has a small sample and some of the population parameters fall near either zero or unity (Johnson and Koch, 1971:440). In the event that one did obtain an out-of-range prediction, one could modify the GLS estimation procedure to eliminate such predictions or one could of course resort to another model.[27]

---

[24] The GSK technique is capable of producing log-linear models with GLS estimates.

[25] Goodman's ECTA may be obtained for a nominal fee by writing Leo Goodman at the University of Chicago. Jay Magidson (Abt Associates Inc.; 55 Wheeler Street; Cambridge, MA 02138) has just begun to market a program which, although based on Goodman's technique, permits nonhierarchical modeling and nested designs. (See Magidson, 1976). Other well-known programs for log-linear modeling include C-TAB and MULTIQUAL, both available for a substantial fee through International Educational Services; Box A3650; Chicago, Illinois 60690. See also Haberman's program (1979:571 ff.). Routines for the log-linear analysis of contingency tables are also under development for eventual inclusion in SPSS (Statistical Package for the Social Sciences). Finally, GSK's program (GENCAT) may be obtained for a fee from J. Richard Landis, Department of Biostatistics, The University of Michigan, Ann Arbor, Michigan 48109.

[26] Hierarchical modeling *can* handle orthogonal coding. For example, a four-category variable can be represented by linear, quadratic, and third-degree trends (Winer, 1971). However, hierarchical modeling does not allow one to omit the quadratic and third-degree trends as in the example just given in the text. This is but one example of a nonhierarchical model of potential interest.

The difference in criterion variables has implications which go beyond out-of-range predictions. It is hardly profound, but nevertheless useful, to bear in mind that the GSK approach does allow one to model proportions directly. True, proportions are not inherently superior to logits or odds. Nevertheless, they are more widely understood than the latter, and some investigators will, no doubt, appreciate the possibility of explaining their regression coefficients as effects on probabilities, rates, or proportions. The regression coefficients produced by logit modeling cannot be interpreted in this manner, though the predicted logits can at least be transformed to proportions.[28, 29]

On the other hand, Theil (1970) and Cox (1970) argue, in effect, that logit modeling is better suited to handle the ceiling and floor effects found in modeling proportions, and that it is therefore inherently superior to modeling proportions.[30] This may well be true in many situations. However, it is easy to find examples in which modeling proportions is at least as effective as modeling logits. The NORC data analyzed in this article constitute a case in point.

A final difference worthy of mention here is this: logit modeling is in effect multiplicative in the natural scale (that is, when odds constitute the criterion variable, as in Equation 3); linear probability modeling is additive. If, perchance, one's theory stipulates that some variable exerts one or the other kind of effect, one normally ought to utilize the corresponding model. For example, the amount learned (or forgotten) during a given period of time is often theorized to be a product of the amount known in the first place, just as the amount of interest paid is a product of the principal. In this case, theoretical considerations indicate a logit model. More often than not, however, sociological theories are not so precise as to stipulate whether effects are additive or multiplicative, and the choice between techniques can seldom be made on this basis.

---

[27] As Jay Magidson has pointed out to me, if the GLS estimation procedure were modified to constrain estimates to the [0,1] interval, those estimates would become somewhat less "efficient" than otherwise. Incidentally, logit modeling suffers a logical difficulty of sorts too. If, for example, 27 people in some subgroup agree with a statement and none disagree, the odds of agreeing to disagreeing (27:0) are undefined. So is the logit. However, this usually presents no computational difficulty unless the model is saturated, in which case adding ½ to the frequency of every cell is recommended.

[28] $P = \Omega/(1+\Omega)$, where P is the proportion in a given category, and Ω is the odds of falling in that category. Also, although the coefficients of logit and linear probability models are not mathematically equivalent, they usually have much the same substantive implications. For example, consider the direct effect of region in 1963 (Table 4, col. 4). Controlling for education, the expected proportion favoring equal opportunity was .75 (regression constant minus regional effect). The expected odds, then, were 3.00 (.75/.25) in the South. Similarly, it may be shown that the expected odds outside the South were 5.67 (that is, .85/.15). Thus, the linear probability model implies that living outside the South increased the odds of favoring equal opportunity 1.88 times (5.67/3.00). This is near the estimate (1.93) from the multiplicative model (Table 5, col. 8).

[29] Those analyzing panel studies may well find the results from the GSK approach more appealing than those from other approaches. See Landis and Koch (1979).

[30] If what they say is true, it would seem that logit models will be more parsimonious than linear probability models when many of the observed proportions fall near either zero or unity.

In sum, it would be unwise to conclude that any of the above techniques is invariably superior to the others. The worth of a tool must be judged in terms of the task at hand. Thus, a technique must be selected only after asking questions of the following sort: Which technique allows me to ask the questions I need to ask, and which one is most useful in making sense of the data?

POLYTOMOUS DEPENDENT VARIABLES

To this point, only the analysis of dichotomous dependent variables has been explicated. The techniques outlined above can also be utilized to analyze polytomous dependent variables. As will soon become apparent, however, the techniques do not offer any fundamental conceptual breakthrough in the handling of polytomous dependent variables, and one can often do no better than analyze a series of dichotomies.

As a basis of illustration, imagine that NORC had allowed three, rather than two, categories of response to its question about equal job opportunity for blacks: "strongly agree" (SA), "agree somewhat" (AS), and "disagree" (DA). One could, of course, collapse categories so as to pit SA against the two lower alternatives or DA against the two upper alternatives. In this case, one logit would be predicted for each of the twelve subgroups of respondents (as in Table 1, col. 13). However, one may preserve the trichotomy by employing what Theil (1970) terms a "conditional logit model." (See also Goodman, 1970.) In this case, two logits would be predicted simultaneously for each subgroup: $\log(n_{SA}/n_{DA})$ and $\log(n_{AS}/n_{DA})$, where n_{SA}/n_{DA} is the odds of strongly agreeing rather than disagreeing and n_{AS}/n_{DA} is the odds of agreeing somewhat rather than disagreeing.[31] In general, when there are k categories in the dependent variable, $k-1$ logits for each subgroup are predicted by a conditional logit model.

Although there is nothing mathematically questionable about conditional logit modeling, the results are often difficult to interpret substantively. Consider, for example, the hypothetical data presented in Table 7 (cols. 7–9), in which attitudes towards equal opportunity are viewed as a function of respondents' schooling (col. 1). The odds which would be predicted by a saturated model (rendered multiplicative through the exponential transformation, as in Equation 3) are recorded in cols. 10 and 11. Notice that the figures in col. 11 seem to indicate that prejudice *increases* with education (n_{AS}/n_{DA} decreases as education increases). The odds in col. 10, on the other hand, suggest that prejudice *decreases* with education, as expected. The point is that, in *conditional* logit modeling, the proper interpretation of the odds involving any two categories (say, AS and DA) cannot be made without taking into account information about the other categories (SA, in this case). Making sense of all the coefficients simultaneously can be very challenging, especially when $k \geqq 4$. In this example, by the time one has integrated the information in cols. 10 and 11 so as to avoid a misimpression about the relationship between education and prejudice, one arrives at a result which is functionally equivalent to the one which would have been obtained if the trichotomy had been collapsed into a dichotomy in the first place—pitting SA against the two lower categories or DA against the two higher categories.

A related point is illustrated by the following example. Suppose that one wished to explain the odds that people *who completed high school* obtained at least some higher education instead of terminating their schooling, and suppose that one possessed trichotomous data on people who had finished their schooling: "did not complete high school" (NHS), "completed high school but no more" (HS), and "attended college or more" (C). Given this trichotomous dependent variable, one could utilize conditional logit modeling so as to predict two logits simultaneously: $\log(n_C/n_{HS})$ and $\log(n_{NHS}/n_{HS})$. The first of these would seem to be relevant to the question at hand. However, under conditional logit modeling, the model and the predictions for $\log(n_C/n_{HS})$ are *conditional* on the information about people who did not complete high school (NHS)—information which is irrelevant to the question at hand. It is important to understand that, if one simply disregarded NHS and developed an ordinary logit model for dichotomous dependent variables, one would probably obtain a different model and different predictions for $\log(n_C/n_{HS})$ than under conditional logit modeling. And given that the question does not concern people who failed to complete high school, the model and predictions from ordinary logit modeling would be patently superior to those obtained from conditional logit modeling. (See Fienberg, 1977; and Fienberg and Mason, 1978.)

---

[31] Mathematically equivalent results would be obtained if either SA or AS was treated as the anchor category instead of DA (which appears in both denominators).

The point is not that conditional logit modeling is worthless. Rather, the point is that it should be utilized only after one is certain that its conditional results are useful in answering the question posed by the research, and that, even if the results are conceptually appropriate, they will often be difficult to interpret. As will be documented later, published sociological applications of conditional logit modeling have often obscured these points.

When the categories of the dependent variable are ordered, another approach to analyzing polytomous variables is available: assigning numbers to the categories. For example, one might assign "1" to strongly agreeing that blacks should have equal opportunity; "0" to agreeing somewhat; and "−1" to disagreeing. A mean could then be calculated for each subgroup, and those means would become the criterion variable in an analysis based on the GSK procedure. (See Table 7, col. 15 and fn. e.) Under ordinary regression, assigning numbers to categories is objectionable for at least two reasons: (1) because the criterion variable would usually violate the assumptions of normality and homoscedasticity which underlie tests of significance, and (2) because calculating means on the basis of ordinal numbers entails a "leap of faith" which should be avoided if possible. Under the GSK procedure, the first objection is adequately

Table 7. Interpreting a Conditional Logit Model

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
	Observed Frequencies:[a]				Geometric Mean[b]	Observed Proportions			Odds[c]		Coefficients[d]			Weighted Proportions[e]
Education	SA	AS	DA	Total		SA	AS	DA	n_{SA}/n_{DA}	n_{AS}/n_{DA}	SA	AS	DA	
Elementary	21	35	14	70	21.75	.30	.50	.20	1.50	2.50	−.035	.476	−.441	.10
Secondary	38	44	18	100	31.11	.38	.44	.18	2.11	2.44	.200	.347	−.547	.20
Higher	30	14	6	50	13.61	.60	.28	.12	5.00	2.33	.790	.028	−.819	.48

[a] Hypothetical frequencies.

[b] The cube root of the product of cols. 2, 3 and 4.

[c] Col. 10 equals col. 7 divided by col. 9; col. 11 equals col. 8 divided by col. 9.

[d] Col. 12 equals the log of the ratio of col. 2 to col. 6; col. 13 equals the log of the ratio of col. 3 to col. 6; col. 14 equals the log of the ratio of col. 4 to col. 6. Note that these coefficients may be easily transformed into the conditional odds in cols. 10 and 11. In row 1, $.476 - (-.441) = .917, e^{.917} = 2.50$ (col. 11)

[e] Col. 7 times 1 plus col. 8 times 0 plus col. 9 times −1.

handled; however, the second one remains a problem. Under certain circumstances, one may avoid the second objection by utilizing Thurstonian psychometric techniques (Torgerson, 1958). The "method of successive intervals" might allow one to assign the categories values on an interval scale (for example, 1.5, .2, −2.0). However, on many occasions one must simply weigh the objection to assigning numbers on a priori grounds against what may turn out to be a considerable advantage in making sense of the data. Some reputable statisticians such as Grizzle et al. (1969) have been willing to assign numbers to categories despite the leap of faith entailed.

For relatively new work in this vein which applies to logit modeling, see Duncan and McRae (1978) and Duncan (1979a).

PATH ANALYSIS

Since ordinary regression can sometimes be employed to obtain estimates for path diagrams, it is natural to ask whether the regression techniques outlined above may be employed for the path analysis of categoric variables. Goodman (1973a; 1973b; 1979b) has already devoted considerable effort to demonstrating how logit modeling can be used for this purpose. However, there appear to be several reasons why his approach will never be as useful as ordinary path analysis has been with continuous variables.

Consider a simple system in which X affects Y, and Y affects Z. In the path analysis of continuous variables, the symbol "Y" means the same thing in both structural equations; that is, the score on X affects *the score on Y*, and *the score on Y* in turn affects the score on Z. On the other hand, under the logit modeling proposed by Goodman for the analysis of categoric variables, the state of X affects *the logit of Y*, and *the state of Y* (not the logit of Y) affects the logit of Z. In other words, the criterion variable in the first equation does *not* become the independent variable in the second equation. As a consequence, a very nice feature of ordinary path analysis is lost. In ordinary path analysis, the observed correlation between, say, X and Z is easily decomposed into the component generated by the direct path from X to Z (zero in this example) and the component generated through the intervening variable Y. The observed correlation (r_{xz}) equals the product of the standardized path coefficients p_{yx} and p_{zy} in this example. In contrast, path analysis via logit modeling does *not* allow one to reconstitute the observed association between X and Z by examining the "path" coefficients. This is one of the reasons why it will probably never gain the currency that ordinary path analysis has enjoyed. For related observations, see Fienberg (1977:91), Gillespie (1977) and Brier (1978).

Another difficulty encountered in the path analysis of contingency tables is that the techniques outlined above usually detect important interaction effects. For example, in all the above analyses of the NORC data, the effect of region on attitudes towards equal opportunity was found to be contingent on time. Such interactions are awkward to depict in path diagrams. Notice that Goodman utilizes (rare) data without such interactions to illustrate his approach. Others have simply ignored the interaction effects in drawing their path diagrams (Gortmaker, 1979). Of course, one could adopt some convention to depict the interactions on a path diagram, but it is not yet clear that doing so would be worth the trouble.

Finally, there is an unsatisfying aspect to Goodman's approach to nonrecursive modeling—modeling in which feedback among variables is hypothesized. Suppose, for example, that W affected Y and that X affected Z. Furthermore, suppose that the two endogenous (dependent) variables Y and Z also affected one another. Path analysis would allow one to estimate (simultaneously) the effect of Z on Y (p_{yz}) and the effect of Y on Z (p_{zy}) (Duncan, 1975). Note that there is usually no theoretical justification for assuming that $p_{zy} = p_{yz}$ and that ordinary path analysis does not constrain the two to equal one another. Now suppose that the above four variables were dichotomous. Under the system developed by Goodman, the effect of Z on the logit of Y would be mathematically constrained to equal the effect of Y on the logit of Z (Goodman, 1977:32–3; 1973a; 1973b). It follows, then, that his technique should only be employed when this constraint is substantively reasonable. And because this is seldom the case, it would appear that his approach has only limited utility.

These shortcomings are not due to any lack of imagination on Goodman's part. He has explored the possibilities further than anyone else. Nevertheless, these shortcomings point to the fact that the path analysis of categoric variables does not yet offer some of the most important advantages currently offered by the path analysis of continuous variables.

EIGHT COMMON ERRORS

Most sociologists learn new statistical techniques by following examples rather than by reading original statistical treatises. Unfortunately, such a practice would likely mislead those who wish to master the "new" techniques for analyzing contingency tables: a high proportion of sociological articles based on these techniques contains statistical or methodological errors. Moreover, these errors are by no means confined to early sociolological applications. For example, Gortmaker (1979:286) claims that the criterion variable of his logit model is the log of the death *rate* instead of the log of the *odds* of dying rather than surviving; he misinterprets his coefficients accordingly. Kessler et al. (1979) make numerous errors, some of which will be described shortly, others of which are discussed in Gove and Swafford (1980). The following list of common mistakes will, perhaps, prove useful in efforts to avoid such pitfalls in situations where one variable is considered dependent on others. Because linear probability modeling has seldom found its way into sociological articles, the list focuses on log-linear and logit modeling.

(1) Failing to let theory guide the analysis. Recall that the equation for log-linear modeling (4) is peculiar: it has no substantive dependent variable. As a consequence, it is possible to churn out a log-linear analysis without even thinking about the causal relationships among variables. This is not to say that the technique is flawed. As was pointed out earlier, not only can one derive the coefficients of a logit model from Equation 4 (provided one stipulates the dependent variable), one can also test hypotheses about the conditional independence of variables. Davis (1974) takes the latter approach in his log-linear analysis of the NORC data; in doing so, he illustrates the theoretical morass which can be generated by failing to focus on a hypothesis. He arrives at the following model: (ER) (RJ) (RT) (EJT).[32] He (Davis, 1974:221) then offers the following interpretation of the first two terms:

> At last we are ready to state the substantive conclusions of this investigation. . . . First, there is a significant partial association between education and region, controlling for time and jobs, that is essentially similar in magnitude within each control condition. Second, there is a statistically significant partial association between region and jobs, controlling for time and education, that is essentially similar in magnitude within each control condition.

This interpretation is technically correct.[33] However, notice that the two conclusions are theoretically incompatible: the second conclusion is relevant only if education is either an antecedent or an intervening variable; the first one is relevant only if it is not. In addition, one would be hard put to come up with a theory which justified controlling on attitudes and education in interpreting the association between region and time. Davis recognizes shortcomings such as these, but he offers little advice on how to avoid them.

Goodman repeatedly advocates that logit modeling be employed when one variable is dependent on others (1970;

---

[32] Since Davis does not attempt to develop a logit model, he does not automatically include the (TER) term. Notice that his model does not reproduce the number of respondents in each subgroup. (Compare Table 1, col. 5 with the corresponding entries in cols. 18 and 19.)

[33] He might have described the (ER) term as follows: there is a (nonzero) conditional association between E and R which does not vary across combinations of J and T. However, a conditional association which does not vary across conditions is equivalent to the corresponding partial association.

1971; 1973a; 1973b).[34] In this example, viewing attitudes towards equal opportunity (J) as the dependent variable allows one to develop a logit model, and it therefore greatly simplifies the analysis: certain potential models become irrelevant and are therefore not tested, and coefficients have an interpretation far clearer than the one set forth by Davis (1974:224–7). This is not to suggest that log-linear modeling serves no useful function except as a vehicle for logit modeling. (For elaboration, see the concluding paragraphs of the above section on log-linear modeling.) However, opting for general log-linear modeling when logit modeling can be utilized normally complicates the analysis.

(2) Omitting interactions among independent variables when deriving a logit model from a log-linear model. Recall that (TER) was automatically included in all log-linear models tested in Table 3. In this manner, the observed associations among independent variables were taken for granted, as in ordinary regression. As was pointed out in the discussion of log-linear modeling, Goodman and most other statisticians advocate this procedure when logit models are derived from log-linear models. Nevertheless, several sociologists have failed to follow this procedure (Knoke, 1975; Knoke and Hout, 1976; Cohen and Kluegel, 1978; Kessler et al., 1979; also, see Gillespie's comment [1978] on Reynolds [1977b]). True, as was pointed out above, at least two competent statisticians have offered arguments for omitting statistically nonsignificant interactions among independent variables. However, these sociologists have not explicitly invoked those arguments.

One of the consequences of excluding interactions among independent variables is that one cannot calculate the unexplained variation in the *criterion* variable (as in Table 3, Model 1). At least two investigators have calculated this variation on the basis of irrelevant models (Knoke and Hout, 1976; Kessler et al., 1979).

(3) Failing to restrict modeling so as to reflect the study design. Again, consider the NORC data analyzed above. It is likely that the number of people sampled in each region during 1946 and 1963 was determined by the sampling procedure employed in gathering the data (see Davis, 1974:224). This implies that the TR association observed in the sample was fixed by the study design itself. As such, it reveals nothing about the TR association in the population. It would therefore be pointless to test the null hypothesis that T and R are independent in the population by omitting TR from some models. Instead, TR should be included in every model. *In general, no matter what hypotheses are in question, when observed frequencies are fixed by the study design, only those log-linear models which reflect the study design are legitimate* (Bishop et al., 1975:30). Incidentally, all the models described in Table 3 do fulfill this requirement by virtue of the inclusion of (TER).

Most authors are not explicit enough about their sampling procedures to allow one to determine whether or not they have followed this prescription. However, a clear violation appears in Kessler et al. (1979). The authors categorize the users of clinics (one state of variable D) into four quartiles on both variable B and C. Thus, BD and CD are both partly artifacts of the study design. Yet the authors test models which omit these associations. Also, notice that Davis (1974) does not constrain his models to reflect NORC's sampling design even though he (p. 224) acknowledges that it was not a simple random sample. Furthermore, he erroneously states that log-linear modeling assumes a simple random sample. The tests of significance for all the techniques outlined above are appropriate under stratified sampling as long as the tested models reproduce the frequencies which were fixed by the study design (Bishop et al., 1975).

(4) Categorizing continuous dependent variables. Knoke and Thomson (1977) categorize a dependent variable measured on an interval scale in order to render the data appropriate for logit modeling. Ordinary regression (perhaps with a transformed dependent variable to correct for skewness) should normally be utilized

---

[34] It would appear, then, that he would criticize authors such as Knoke (1974) and Burke and Turk (1975) who talk in terms of causal models but who do not derive logit models from their log-linear models.

in such a case: it allows one to test the same interactions as logit modeling permits, and the difficulty of interpreting coefficients from conditional logit models is avoided. Ordinary regression allows one to interpret coefficients as effects on the original scale of the dependent variable.

(5) Categorizing and collapsing independent variables. First, when one has a proper categoric dependent variable but continuous independent variables, one ought to consider discriminant analysis (readily available in SPSS). If one's independent variables include both continuous and categoric ones, there may be no fully satisfactory technique. If there happen to be enough cases at each level of the continuous variable, one may utilize the GSK procedure and assign numeric values to each level of the variable. Otherwise, one may be compelled to include categoric variables in a discriminant analysis or to categorize continuous variables for the sake of contingency table analysis. (See Bishop et al., 1975:357–61; see also Press and Wilson, 1978.)

Second, it is not necessary to collapse independent variables into dichotomies for the sake of interpretation. Many investigators consider the coefficients of general log-linear models particularly awkward to interpret substantively when they involve polytomous variables. As Guest (1974:501) puts it: "For our research, the Goodman system has one major operational drawback: the effect parameters are clearly interpretable only for dichotomous variables." Though it *is* difficult to interpret effects on polytomous *dependent* variables (as in conditional logit modeling), it is *not* at all difficult to interpret the effects of polytomous independent variables on dichotomous dependent variables. Consider the coefficients for the trichotomous variable "education" in the multiplicative model of the NORC data (Table 5, col. 7). They may be read as follows: in 1946, having a college (rather than a grade-school) education multiplied the odds of favoring equal opportunity by 2.97 (controlling on region); having a college rather than a high school education multiplied the odds by 2.09 (that is, 2.97/1.42). Numerous authors have unnecessarily collapsed the categories of their independent variables in the effort to make their data conform to misunderstood constraints of log-linear modeling.

Finally, bear in mind that Goodman (1979b:1086–7) inveighs against categorizing and collapsing variables:

> But none of Goodman's articles have suggested that the appropriate methods developed for the analysis of *qualitative* variables (e.g., methods developed for the analysis of conditional independence models, logit models, log-linear models, etc.) should be applied to arbitrarily dichotomized (or polytomized) *quantitative* variables. Indeed, the appropriate methods for the analysis of *qualitative* variables should not be applied in this inappropriate way.

One should not eschew categorizing continuous *independent* variables on the basis of this quotation, for other competent statisticians have expressed a more tolerant view of categorizing continuous variables (Bishop et al., 1975:343). However, one should be wary of arbitrary categorizations, as Goodman warns. The number of categories in a variable determines the number of degrees of freedom associated with it, and this, in turn, can affect the statistical significance of main effects and interactions in the model. In other words, trichotomizing a variable may give rise to a different model than dichotomizing it. (See Reynolds, 1977b; Gillespie, 1978; and Goodman, 1979b.)

(6) Relying too much on variance explained in evaluating the importance of variables. Even if *identical* unstandardized regression equations explained a certain dependent variable in two populations, the "relative importance" of two predictor variables might vary drastically across the two populations as a result of differences in the variances of those predictors. This, in itself, should make investigators cautious about judging importance on the basis of unique variance explained (that is, variance explained by a variable when it is last to enter the equation). However, an added caution is warranted in the case of contingency table analysis. More often than not, interactions are detected by such an analysis, and the importance of those interactions may easily be misjudged on the basis of the unique

variance they explain. Spenner (1975:515) terms a certain interaction as "relatively small" because it explains only 13% of the total error chi-square. But consider the NORC data above. The direct effect of region on the odds of favoring equal job opportunity was found to drop from 3.13 to 1.95 between 1946 and 1963. Intuitively, the difference between these coefficients represents a fair-sized interaction. Yet it explains less than *two* percent of the variation. (Subtract the error chi-square for Model 10 from the one for Model 7, then divide the difference by the error chi-square for Model 1.) See also fn. 11 for further comments on the interpretation of variance explained.

(7) Failing to collapse tables when necessary. Consider the simple system W affects X, X affects Y, and Y affects Z. It is well known that the path coefficient representing the effect on W on X may be obtained by simply regressing X on W, provided that the disturbance term is uncorrelated with W. According to Goodman, a proper path analysis of categoric variables also requires that Y and Z be ignored at this stage. In other words, one must collapse the four-way table (WXYZ) into the two-way table (WX) to properly estimate the effect of W on the logit of X. If one instead uses the four-way table, one obtains the *partial* effect of W on the logit of X controlling on Y and Z. Of course, it would be nonsensical to control for the consequent variables. Errors of this sort occur in Kessler et. al. (1979) and Knoke (1974:60). See also Reynolds (1977b), Goodman (1979b), and Gillespie (1978).

(8) Failing to interpret the magnitude of coefficients. It is no doubt commonplace to point out that effects can be statistically significant, yet substantively unimportant because of small magnitude. Obviously, understanding the magnitude of coefficients is important. More often than not, however, log-linear analyses in sociological publications dwell on tests of significance and falter in explicating the meaning of coefficients. This fact is more than a little ironic, for, supposedly, one of the primary virtues of parametric techniques is that they produce coefficients of estimable precision. What is the point of such precision if one can only vaguely state the meaning of coefficients? There is nothing mysterious about these techniques in cases where one variable is considered dependent on others, and confusion about the connection between coefficients and ideas is usually the fault of the author, not of the reader.

The most grievous examples of this problem occur in articles relying on conditional logit modeling (Knoke and Hout, 1976; Knoke and Thomson, 1977; Burke and Turk, 1975; Hogan, 1978). If coefficients are given at all, they are invariably presented as in Table 7, cols. 12–14. The precise interpretation of such coefficients is horrendous. Consider, for example, the "−.441" and "−.547" in col. 14. These may be interpreted as follows: the log of the ratio of the number of people who disagreed to the geometric mean of the people in all three response categories drops from −.441 among those with elementary school educations to −.547 among those with secondary school educations. (See Table 7, fn. d.) Such abstruse quantities should be utilized only by those who are willing to explain the substantive significance of the magnitudes fairly precisely.

Some good sociological applications of logit modeling may be found in Hagan (1977) and Fienberg and Mason (1978). This is by no means a complete list of good examples.

A FINAL OBSERVATION

Reviewing the gradual adoption of the "new" forms of contingency table analysis, one cannot help but be struck anew by the extent to which the available statistical techniques condition the hypotheses raised by investigators. For example, whether variables are continuous or categoric, it is entirely plausible that the effect of one independent variable is contingent on the level of another—that there is interaction or specification. This possibility is routinely ignored in the ordinary regression of continuous variables, where it is inconvenient to examine, yet it serves as a major substantive focus in many contingency table analyses, where the new techniques have made such questions easy to raise. This is not to say that the new techniques have markedly raised the maturity of sociological analysis: one can easily find numerous cases in which investigators have done violence to their theories or their data in the name of the new techniques. The point is that one must ever be watchful that all statistical techniques are kept in the service of ideas, and that whenever the new techniques are not employed in such a fashion as to render the connection between ideas and data clear, they have been ill-used.

APPENDIX

The purpose of this appendix is to explicate the relationship between log-linear and logit modeling by demonstrating how Equation 5 is derived from Equation 4. In this example, the aim is to derive a logit equation which predicts log (F_1/F_2), where F_1 is the number of respondents in a given subgroup who *agreed* that blacks should have equal opportunity, and F_2 is the number who disagreed in that same subgroup. Now, $\ln(F_1/F_2)=\ln(F_1)-\ln(F_2)$. This implies that the logit can be estimated by first estimating $\ln(F_1)$, then subtracting $\ln(F_2)$. These latter two quantities can be estimated with log-linear equations such as Equation 4. Specifically:

$$\begin{aligned}\ln(F_1) = {} & \lambda^*_{13} + \lambda^*_1 J_1 + \lambda^*_{14}T + \lambda^*_{15}E_h + \lambda^*_{16}E_c \\ & + \lambda^*_{17}R + \lambda^*_2 J_1 T + \lambda^*_3 J_1 E_h + \lambda^*_4 J_1 E_c \\ & + \lambda^*_5 J_1 R + \lambda^*_{18}TE_h + \lambda^*_{19}TE_c + \lambda^*_{20}TR \\ & + \lambda^*_{21}E_h R + \lambda^*_{22}E_c R + \lambda^*_6 J_1 TE_h \\ & + \lambda^*_7 J_1 TE_c + \lambda^*_8 J_1 TR + \lambda^*_9 J_1 E_h R \\ & + \lambda^*_{10}J_1 E_c R + \lambda^*_{23}TE_h R + \lambda^*_{24}TE_c R \\ & + \lambda^*_{11}J_1 TE_h R + \lambda^*_{12}J_1 TE_c R \,, \qquad (6)\end{aligned}$$

$$\begin{aligned}\ln(F_2) = {} & \lambda^*_{13} + \lambda^*_1 J_2 + \lambda^*_{14}T + \lambda^*_{15}E_h + \lambda^*_{16}E_c \\ & + \lambda^*_{17}R + \lambda^*_2 J_2 T + \lambda^*_3 J_2 E_h + \lambda^*_4 J_2 E_c \\ & + \lambda^*_5 J_2 R + \lambda^*_{18}TE_h + \lambda^*_{19}TE_c + \lambda^*_{20}TR \\ & + \lambda^*_{21}E_h R + \lambda^*_{22}E_c R + \lambda^*_6 J_2 TE_h \\ & + \lambda^*_7 J_2 TE_c + \lambda^*_8 J_2 TR + \lambda^*_9 J_2 E_h R \\ & + \lambda^*_{10}J_2 E_c R + \lambda^*_{23}TE_h R + \lambda^*_{24}TE_c R \\ & + \lambda^*_{11}J_2 TE_h R + \lambda^*_{12}J_2 TE_c R \,. \qquad (7)\end{aligned}$$

Since both equations pertain to the same subgroup, terms not involving J are the same in both equations. Thus, when Equation 7 is subtracted from Equation 6, the terms with subscripts 13 through 24 drop out. Thus,

$$\begin{aligned}& \ln(F_1)-\ln(F_2)=\ln(F_1/F_2) = \\ & \quad (\lambda^*_1 J_1-\lambda^*_1 J_2) + (\lambda^*_2 J_1 T-\lambda^*_2 J_2 T) \\ & \quad + (\lambda^*_3 J_1 E_h-\lambda^*_3 J_2 E_h) \\ & \quad + \ldots + (\lambda^*_{12}J_1 TE_c R-\lambda^*_{12}J_2 TE_c R) \,. \qquad (8)\end{aligned}$$

Equation 8 may be simplified to Equation 5 in the text. Recall that the coefficients for each variable *sum* to zero under effect coding. For example, in Table 4, col. 1, the coefficient for South is −.09; the coefficient for Other is .09. In other words, the absolute values of the two coefficients are equal to one another; only the signs differ. Thus, when the second coefficient is *subtracted* from the first, the difference always equals twice the value of the first; that is, −.09−(.09)=−.18. Likewise, each of the parenthetical terms in Equation 8 equals twice the first of the two terms within the parentheses. For example,

$$(\lambda^*_1 J^*_1-\lambda^*_1 J_2) = 2^{1*}J_1$$

and $(\lambda^{½}_{12}J_1 TE_c R-\lambda^*_{12}J_2 TE_c R) = 2\lambda^*_{12}J_1 TE_c R.$

These terms appear in Equation 5, except that the subscript to J is not written out. This completes the derivation of a logit equation (5) from a log-linear equation (4).

Notice that deriving an equation for the logit from a log-linear equation does not depend on including the (TER) term in the log-linear equation. In other words, if, say, $\lambda^*_{24}TE_c R$ or $\lambda^*_{16}E_c$ were omitted from Equations 4, 6, and 7, Equations 8 and 5 would not change. This is because terms which do not involve J drop out when Equation 7 is subtracted from Equation 6. However, although these terms drop out, they *do* affect the estimates of F_1 and F_2 given by Equations 6 and 7, respectively, and they therefore affect the estimate of $\ln(F_1/F_2)$. As is explained in the text, statisticians agree that a logit model may be legitimately derived from a log-linear equation in which (TER) is included. A few, however, argue that leaving out nonsignificant terms from (TER) is also all right, as long as those terms are not affected by the study design.

REFERENCES

Andrews, F. M., J. N. Morgan, and J. A. Sonquist
1967 Multiple Classification Analysis. Ann Arbor: Institute for Social Research, University of Michigan.

Ashton, W. D.
1972 The Logit Transformation. New York: Hafner.

Benedetti, Jacqueline K., and Morton B. Brown
1978 "Strategies for the selection of log-linear models." Biometrics 34:680–6.

Bhapker, V. P., and Gary G. Koch
1968 "Hypotheses of 'no interaction' in multidimensional contingency tables." Technometrics 10:107–23.

Bishop, Y. M. M.
1969 "Full contingency tables, logits, and split contingency tables." Biometrics 25:119–28.

Bishop, Yvonne M. M., Stephen E. Fienberg, and Paul W. Holland
1975 Discrete Multivariate Analysis: Theory and Practice. Cambridge, Massachusetts: MIT Press.

Brier, Stephen S.
1978 "The utility of systems of simultaneous logistic response equations." Pp. 119–29 in Karl F. Schuessler (ed.), Sociological Methodology 1979. San Francisco: Jossey-Bass.

Burke, Peter J., and Austin T. Turk
1975 "Factors affecting postarrest dispositions: a model for analysis." Social Problems 22:313–29.

Cohen, Lawrence E., and James R. Kluegel
1978 "Determinants of juvenile court dispositions: ascriptive and achieved factors in

two metropolitan courts." American Sociological Review 43:162–76.

Coleman, J. A.
1970 "Multivariate analysis for attribute data." Pp. 217–45 E. F. Borgatta and G. W. Bohrnstedt (eds.), Sociological Methodology 1970. San Francisco: Jossey-Bass.

Cox, D. R.
1970 The Analysis of Binary Data. London: Methuen.

Davis, James A.
1974 "Hierarchical models for significance tests in multivariate contingency tables: an exegesis of Goodman's recent papers." Pp. 189–231 in H. Costner (ed.), Sociological Methodology 1973–74. San Francisco: Jossey-Bass.

Duncan, Otis Dudley
1975 Introduction to Structural Equation Models. New York: Academic Press.
1979a "Constrained parameters in a model for categorical data." Sociological Methodology and Research 8:57–68.
1979b "How destination depends on origin in the occupational mobility table." American Journal of Sociology 84:793–803.

Duncan, Otis Dudley, and James A. McRae, Jr.
1978 "Multiway contingency analysis with a scaled response or factor." Pp. 68–85 in Karl F. Schuessler (ed.), Sociological Methodology 1979. San Francisco: Jossey-Bass.

Evers, Mark, and N. Krishnan Namboodiri
1978 "On the design matrix strategy in the analysis of categorical data." Pp. 86–111 in Karl F. Schuessler (ed.), Sociological Methodology 1979. San Francisco: Jossey-Bass.

Fennessey, James
1968 "The general linear model: a new perspective on some familiar topics." American Journal of Sociology 74:1–27.

Fienberg, Stephen
1977 The Analysis of Cross-Classified Categorical Data. Cambridge, Massachusetts: MIT Press.
1978 "A note on fitting and interpreting parameters in models for categorical data." Pp. 112–18 in Karl F. Schuessler (ed.), Sociological Methodology 1979. San Francisco: Jossey-Bass.

Fienberg, Stephen E. and William M. Mason
1978 "Identification and estimation of age-period-cohort models in the analysis of discrete archival data." Pp. 1–67 in Karl F. Schuessler (ed.), Sociological Methodology 1979. San Francisco: Jossey-Bass.

Giles, Michael W., Douglas S. Gatlin, and Everett F. Cataldo
1976 "Racial and class prejudice: their relative effects on protest against school desegregation." American Sociological Review 41:280–8.

Gillespie, Michael W.
1977 "Log-linear techniques and the regression analysis of dummy dependent variables." Sociological Methods and Research 6:103–22.
1978 "The application of log-linear techniques to recursive models: comment on Reynolds." American Journal of Sociology 84:718–22.

Gokhale, D. V., and Solomon Kullback
1978 The Information in Contingency Tables. New York: Marcel Dekker.

Goodman, Leo
1969 "How to ransack social mobility tables and other kinds of cross-classification tables." American Journal of Sociology 75:1–40.
1970 "The multivariate analysis of qualitative data: interactions among multiple classifications." Journal of the American Statistical Association 65:226–56.
1971 "The analysis of multidimensional contingency tables: stepwise procedures and direct estimation methods for building models for multiple classifications." Technometrics 13:33–61.
1972 "A modified multiple regression approach to the analysis of dichotomous variables." American Sociological Review 37:28–45.
1973a "The analysis of multidimensional contingency tables when some variables are posterior to others:a modified path analysis approach." Biometrika 60:179–92.
1973b "Causal analysis of data from panel studies and other kinds of surveys." American Journal of Sociology 78:1135–91.
1975 "The relationship between modified and usual multiple-regression approaches to the analysis of dichotomous variables." Pp. 83–110 in David Heise (ed.), Sociological Methodology 1976. San Francisco: Jossey-Bass.
1977 "How not to analyze nonrecursive systems pertaining to qualitative variables." Political Methodology 4:23–34.
1979a "Multiplicative models for the analysis of occupational mobility tables and other kinds of cross-classification tables." American Journal of Sociology 84:804–19.
1979b "A brief guide to the causal analysis of data from surveys." American Journal of Sociology 84:1078–95.

Gortmaker, Steven L.
1979 "Poverty and infant mortality in the United States." American Sociological Review 44:280–97.

Grizzle, James E., C. Frank Starmer, and Gary G. Koch
1969 "Analysis of categorical data by linear models." Biometrics 25:489–504.

Grizzle, James E., and O. Dale Williams
1972 "Log-linear models and tests of independence for contingency tables." Biometrics 28:137–56.

Gove, Walter, and Michael Swafford
Forthcoming "Comment on Kessler et al." Social Forces.

Guest, Avery M.
1974 "Class consciousness and American political attitudes." Social Forces 52:496–510.

Haberman, Shelby J.
1974 The Analysis of Frequency Data. Chicago: University of Chicago Press.
1978 Analysis of Qualitative Data. Vol. 1. New York: Academic Press.
1979 Analysis of Qualitative Data. Vol. 2. New York: Academic Press.

Hagan, John
1977 "Criminal justice in rural and urban communities: a study of the bureaucratization of justice." Social Forces 55:597–611.

Hauser, Robert M., John N. Koffel, Harry P. Travis, and Peter J. Dickinson
1975 "Temporal change in occupational mobility: evidence for men in the United States." American Sociological Review 40:279–97.

Hildebrand, David K., James D. Laing, and Howard Rosenthal
1977 Prediction Analysis of Cross Classifications. New York: Wiley.

Hogan, Dennis P.
1978 "The variable order of events in the life course." American Sociological Review 43:573–86.

Johnson, William D., and Gary G. Koch
1971 "A note on the weighted least squares analysis of the Ries-Smith contingency table data." Technometrics 13:438–47.

Kerlinger, Fred, and E. Pedhazur
1973 Multiple Regression in Behavioral Research. New York: Holt, Rinehart, and Winston.

Kessler, Ronald C., James A. Reuter, and James R. Greenley
1979 "Sex differences in the use of psychiatric outpatient facilities." Social Forces 58:557–71.

Knoke, David
1975 "A comparison of log-linear and regression models for systems of dichotomous variables." Sociological Methods and Research 3:416–33.
1974 "Religious involvement and political behavior: a log-linear analysis of white Americans, 1952–1968." The Sociological Quarterly 15:51–65.

Knoke, David, and Michael Hout
1976 "Reply to Glenn." American Sociological Review 41:905–8.

Knoke, David and Randall Thomson
1977 "Voluntary association membership trends and the family life cycle." Social Forces 56:48–65.

Kritzer, Herbert M.
1977 "Analyzing measures of association derived from contingency tables." Sociological Methods and Research 5:387–418.

Landis, J. Richard, and Gary G. Koch
1979 "The analysis of categorical data in longitudinal studies of behavioral development." Pp. 233–61 in J. R. Nesselroads and P. B. Baltes (eds.), Longitudinal Methodology in the Study of Behavior and Development. New York: Academic Press.

Lehnen, Robert G., and Gary G. Koch
1974 "A general linear approach to the analysis of nonmetric data: applications for political science." American Journal of Political Science 18:283–313.

Magidson, Jay
1976 "The multivariate analysis of qualitative variance: analyzing the probability of an event as a function of observable variables." Ph.D. dissertation, Northwestern University.

Mason, William M., and James S. House
1976 "Reply to Guest." American Sociological Review 41:367–75.

Plackett, R. L.
1974 The Analysis of Categorical Data. London: Charles Griffin.

Press, S. James, and Sandra Wilson
1978 "Choosing between logistic regression and discriminant analysis." Journal of the American Statistical Association 73:699–705.

Reynolds, H. T.
1977a The Analysis of Cross-Classifications. New York: Free Press.
1977b "Some comments on the causal analysis of surveys with log-linear models." American Journal of Sociology 83:127–43.

Rushing, William A.
1978 "Status resources, societal reactions, and type of mental health admission." American Sociological Review 43:521–33.

Schuessler, Karl F. (ed.)
1978 Sociological Methodology 1979. San Francisco: Jossey-Bass.

Schwartz, M. A.
1967 Trends in White Attitudes Toward Negroes. University of Chicago, NORC Report No. 119.

Spilerman, Seymour, and Richard E. Miller
1977 "Comment on Stolzenberg and D'Amico: city nondifferences revisited," American Sociological Review 42:979–83.

Stolzenberg, Ross M.
1977 "Reply to Spilerman and Miller." American Sociological Review 42:983–8.

Stolzenberg, Ross M., and Ronald J. D'Amico
1977 "City differences and nondifferences in the effect of race and sex on occupational distribution." American Sociological Review 42:937–50.

Theil, Henri
1970 "On the estimation of relationships involving qualitative variables." American Journal of Sociology 76:103–54.

Torgerson, Warren
1958 Theory and Methods of Scaling. New York: Wiley.

Whittaker, J., and M. Aitkin
1978 "A flexible strategy for fitting log-linear models." Biometrics 34:487–95.

Winer, B. J.
1971 Statistical Principles in Experimental Design. New York: McGraw-Hill.

RESEARCH NOTE

SEX AND INVOLVEMENT IN DEVIANCE/CRIME: A QUANTITATIVE REVIEW OF THE EMPIRICAL LITERATURE

DOUGLAS A. SMITH AND CHRISTY A. VISHER
Indiana University

American Sociological Review 1980, Vol. 45 (August):691–701

Forty-four studies reporting data on the relationship between sex and indicators of deviance/criminality are reduced to a single data base. Contingency tables (1,118) are generated from the extant empirical literature on sex and deviance and comparable statistics are calculated, using instances where the sex-deviance relationship was reported for specific categories of class position, age, data type, year of study, level of family intactness, race, place of residence, and type of offense. The findings from 1,118 instances are summarized, and patterns are discussed. The overall results indicate that the magnitude of the relationship between sex and deviance is contingent on the year the data were gathered, the type of data used, the percentage classified as deviant in a particular table, whether the indicator of deviance is a single behavior or composite index, race, and the specific offense. Moreover, analysis demonstrates that trends in the sex-deviance relationship over time vary by type of data and by population group.

Interest in differences between males and females in deviance/criminality has generated several recent empirical investigations and spawned a plethora of theoretical debates. While this attention has touched on a variety of topics, one central issue is how differences between the sexes in deviant/criminal behavior vary over time. Some say women are becoming more aggressive and catching up to males in the volume of crimes they commit (Adler, 1975; 1977; Oakley, 1972; Noblit and Burcart, 1976; Vedder and Somerville, 1970; Ward et al., 1969) while others maintain that the new female criminal is more of a social myth than an empirical reality (Crites, 1976; Heidensohn, 1968; Steffensmeier, 1978). Unfortunately, little data have been brought to bear to assess the changing involvement of women in crime, and that which has been reported is mostly analysis of official indicators of criminality such as arrest rates (cf. Klein and Kress, 1976; Noblit and Burcart, 1976; Simon, 1975; Simon and Sharma, 1979; Steffensmeier, 1978; Ward et al., 1969). Although examination of arrest or conviction trends can be enlightening, these types of data alone cannot adequately answer many of the most relevant questions. In particular, changes in official statistics may indicate more about how social control agencies respond to males and females than about changes in the amount of deviance/crime committed by men and women (Chesney-Lind, 1973; Deming, 1977; Haskell and Yablonsky, 1974; Klein and Kress, 1976; Nagel and Weitzman, 1972; Noblit and Burcart, 1976; Simon, 1975). To correct for that bias, some contemporary observers have tried to summarize trends reflected in self-report studies to ascertain changes in the criminality of women relative to men (e.g., Cernkovich and Giordana, 1979; Datesman et al., 1975; Jensen and Eve, 1976). But often overlooked in

---

* Direct all communications to: Douglas A. Smith; Department of Sociology; Ballantine Hall; Indiana University; Bloomington, IN 47405.

We are indebted to several people who have assisted us in many respects. Professor Charles Tittle has been a constant source of invaluable comments on several drafts of this paper. Professors Neal Shover and Martin Gold generously made data available to us from the Tennessee Youth Survey and the 1967 and 1972 National Survey of Youth. Finally, Professors Larry Griffin, Barbara Reskin, and Peter Burke provided many insightful suggestions along the way. Of course, the present authors are solely responsible for the ideas presented or any errors that may appear in the text.

analysis of the self-report studies is that any survey is specific to the spatial-temporal setting in which it occurs (Przeworski and Teune, 1970), and, to date, no meaningful attempt to make sense out of the overall body of empirical evidence about male-female differences in criminality/deviance has been reported. Our knowledge about temporal changes in the relative rule-breaking propensity of males and females remains problematic.

To illustrate, Adler (1975) argues that women are increasing their involvement in all types of offenses and paints a picture of a rising new breed of violent, aggressive female offenders. But some scholars emphasize that women's involvement in crime may be contingent on the type of offense considered. While one group suggests there is a convergence between the sexes for violent crime (e.g., Noblit and Burcart, 1976), others point out that the amount of violent crime committed by adult women may be decreasing (Klein and Kress, 1976; Simon, 1976:661) or remaining stable (Crites, 1976; Heidensohn, 1968; Norland and Shover, 1977; Simon, 1975; Simon and Sharma, 1979; Steffensmeier, 1978). In addition, some argue that the gap between the sexes is narrowing only for property offenses such as larceny, embezzlement, and fraud (Crites, 1976; Klein and Kress, 1976; Noblit and Burcart, 1976; Simon, 1975; Steffensmeier, 1978), while others maintain that convergence is limited to personal offenses such as drug and alcohol use (e.g., Gold and Reimer, 1974). Moreover, several scholars have argued that variations in the sex-deviance relationship over time may depend upon demographic characteristics of potential offenders. Many suggest that the gender gap may be inversely correlated with the age of the offender (Adler, 1975; d'Orban, 1972; Mannheim, 1965; Noblit and Burcart, 1976; Radzinowicz, 1937; Pollack, 1950). Further, the relationship between sex and deviance could be contingent on urban/rural differences. Specifically, differences between males and females in deviance/crime involvement presumably may be converging more in urban than in rural areas because the social positions of males and females are more equal in urban than in rural areas (Newberg, 1967; Radzinowicz, 1937). Finally, race has been cited as a possible factor in the convergence controversy, with some scholars arguing that male-female differences are smaller for whites (Deming, 1977) and others suggesting that the gap between the sexes is smaller for blacks (Wolfgang, 1958). Thus, no consensus exists concerning the magnitude of or change in the sex-deviance relationship or even the variables that are important specifiers of a gender/deviance relationship.

The purpose of this paper is to provide some insights into the existing chaos. First, we reduce the extant research literature on sex and deviance (both self-report and official sources) to a single data base. Without such homogenization, meaningful conclusions are impossible because the various studies which report data on sex differences and deviance use different research methodologies, types of data, forms of reporting, and samples. Therefore, merely counting the proportion of studies that report certain relationships between sex and deviance would be likely to produce erroneous conclusions because the studies are simply not comparable in their original forms. The present approach allows us to treat the magnitude of association between sex and deviance, derived from each study, as a dependent variable to be explained by characteristics of the study. In addition, this method generates a meaningful and succinct summary of the available evidence.

METHODS

Our research design is patterned after the approach used by Tittle et al. (1978) in their analysis of the aggregate literature on social class and criminality. We attempted to locate every instance in the literature which reported a relationship between gender and various deviance/crime indicators including self-reports of actual deviances and official recordings of arrest, police contact, conviction, or incarceration. Each report of that relationship for a specific category of class position, age, offense type, race, level of family intactness, and level of urbanization was treated as a separate "instance." Thus, one published study could generate several "instances." The data reported in each study were rearranged into ordinal contingency tables and a gamma measure of association was calculated for each table.[1] This procedure allowed us to reduce the available evidence to 1,118 separate measures of association that serve as independent indicators of the magnitude and direction of the sex-deviance relationship.[2] While this data base represents the extant empirical literature on sex and involvement in deviance, it is unclear whether the pooled literature is representative of the general population for the time period covered (1940–1975). Thus, generalizations beyond the population of studies should be interpreted with some element of reserve.

To explain variation in the magnitude and direction of the sex-deviance relationship across studies, we considered methodological characteristics of the work, and demographic characteristics of the respondents, as they applied to each "instance" (contingency table). For each "instance," the following specific independent variables were indexed: (1) sample size; (2) the type of sample (complete, random, or convenience); (3) percentage classified as deviant; (4) year data were gathered (3 years prior to publication date, if not reported by author); (5) number of categories in the deviance indicator; (6) the type of deviance indicator (incidence, frequency, or seriousness); (7) sophistication of the measure of deviance (single behavior vs. some composite index); (8) the specific kind of deviance; (9) the class position, race, age, and level of family intactness of the respondents; (10) the type of place (rural, urban, or mixed) from which the sample was drawn; and (11) whether the data were self-reported or official indicators of crime/deviance.

FINDINGS

Our first goal is to determine the amount of variance in the relationship between sex and deviance that can be accounted for by subject characteristics, methodological aspects of the studies and the type of offense. Table 1 reports the results of such a test. Subject characteristics (column 1) are dummy coded, with an overall suppressed category containing all cases where no differentiation was made by class position, family background, race, age, or the level of urbanization of the area from which the sample was drawn. As a pool, subject characteristics account for 12.1% of the variance in the sex-deviance relationship. But this does not tell us how the relationship

---

[1] The contingency tables were ordered in such a way that positive values of gamma indicate a larger proportion of males were classified as deviant vis-à-vis females, and negative gammas imply a predominance of females were classified deviant vis-à-vis males. Gamma was selected as the measure of association because marginal distributions do not affect the magnitude of gamma and do not change its meaning as a Proportional Reduction of Error (PRE) measure of association (see Mueller et al., 1977:215–7).

[2] The specific 44 studies are: Akers, 1964; Austin, 1978; Ball et al., 1964; Berger and Simon, 1974; Brown, 1972; Burkett and Jensen, 1975; Cernkovich and Giordana, 1979; Dentler and Monroe, 1961; Forslund, 1970; Gold, 1966; 1970; Gold and Reimer, 1974; Green, 1970; Hathaway et al., 1960; Havighurst et al., 1962; Heise, 1968; Higgins et al., 1977; Hindelang, 1971; 1973; 1974; Hirschi and Stark, 1969; Jensen and Eve, 1976; Kratcoski and Kratcoski, 1975; Lowe, 1966; Moses, 1947; Nye, 1958; Palmore and Hammond, 1964; Porterfield, 1946; Rhodes and Reiss, 1970; Short and Nye, 1958; Norland and Shover, 1977, Tennessee Youth Survey Data; Slocum and Stone, 1963; Steffensmeier, 1978; Steffensmeier et al., 1979; Stewart, n.d.; Stinchcombe, 1964; Tittle and Villemez, 1977; Voss, 1963; 1964; Voss and Hepburn, 1968; Walberg et al., 1975; Wallerstein and Wyle, 1947; Weis, 1976; Williams and Gold, 1972. All tables were generated from the published reports except for Steffensmeier (1978), where we consulted the *Uniform Crime Reports* and the *Current Population Reports* (U.S. Bureau of the Census, 1974; 1975) to reconstruct Steffensmeier's procedure.

Of course, not all studies of sex differences in deviance reported data complete enough to permit derivation of contingency tables. For example, studies using matched pairs of offenders (Bertrand, 1969; Morris, 1965; Reige, 1972) or only institutionalized populations (e.g., Barker and Adams, 1962; Cowie et al., 1968; Cockburn and Maclay, 1965; Gibbons and Grizwold, 1957; Mott, 1973; Robin, 1963; Suval and Brisson, 1974) were not usable because the number of persons classified as deviant and nondeviant were fixed by the researchers. Thus, these data are not representative of the amount of deviant involvement, by sex, in a given population. In addition, some studies did not report data complete enough to identify the number of nonoffenders by sex (d'Orban, 1972; Radzinowicz, 1937; Simon, 1975); failed to report the total sample size (Wise, 1968); or used an a priori classification of subjects into deviance categories (Datesman et al., 1975).

Table 1. Regression Coefficients for Gamma Measures of Association between Sex-Deviance Regressed on Characteristics of the Study Designs Grouped into Subject Characteristics, Methodological Characteristics, and Offense Types (N = 1,118)

Characteristics of Subjects		Methodological Characteristics		Offense Type	
Low SES	−.051[a]	Sample Size	.0001*	Violent	−.028
	−.055[b]		.147		−.039
	(.027)[c]		(.00001)		(.031)
High SES	−.066*	Data Type	.221*	Person	−.419*
	−.071		.267		−.526
	(.027)		(.040)		(.033)
White	−.014	Deviance Categories	−.017	Youth	−.336*
	−.017		−.026		−.489
	(.025)		(.018)		(.031)
Nonwhite	−.091*	Type of Sample	.013	Property	−.125*
	−.107		.033		−.189
	(.026)		(.012)		(.030)
Rural	−.059	Year Data Gathered	−.010*		
	−.053		−.235		
	(.035)		(.001)		
Urban	.048*	% Classified Deviant	−.002*		
	.077		−.115		
	(.020)		(.000)		
Not-Intact	−.038	Scale	−.069*		
	−.032		−.090		
	(.035)		(.019)		
Intact	−.077*				
	−.064				
	(.035)				
Age	.055*				
	.270				
	(.007)				
R	.362	R	.551	R	.517
R^2	.131	R^2	.304	R^2	.267
Constant	.332	Constant	.983	Constant	.589

[a] Metric coefficient.
[b] Standardized coefficient.
[c] Standard error of metric coefficient.
* Significant as indicated by double its standard error.

between sex and deviance varies across subject characteristics. To solve this problem, we examined each variable separately. For example, to determine whether the relationship between sex and deviance is similar across categories of class position, we selected only those instances which reported data on the status position of respondents (N=260). We then estimated an equation suppressing one of the class variables (low SES) and including high SES and the other subject characteristics. If the sex-deviance relationship does not vary by class position, the metric coefficient for high SES in our reestimated equation should not be significantly different from zero. And, in fact, it is not (t=−.487). Thus, the relationship between sex and deviance does not differ across categories of class position. In examining all other subject characteristics, we discovered two significant differences. The association between sex and deviance proved to be smaller among nonwhite samples than white samples (t=2.284, N=316). And, second, samples of adults yielded stronger associations between sex and deviance than did samples of youth (t=7.587, N=1,118). No significant differences were found between rural and urban samples (t=1.790, N=442) or between those from intact and not-intact family backgrounds (t=−.951, N=146).

Methodological characteristics—type of sample, number of categories of deviance, percentage classified as deviant, year the data were collected, scale (a measure of the complexity of the indicator of deviance), type of data (self-report or official), and sample size—collectively explain 30.4% of the variance in the magnitude of association between sex and deviance. The type of sample and the number of categories on the deviance item have no significant effect on the sex-deviance relationship. But five variables do affect this relationship.

Specifically, the percentage classified as deviant in a particular instance exerts a negative effect on the magnitude of the sex-deviance association. As the percentage classified as deviant in a particular instance increases, the gap between male and female involvement in deviance decreases. This is probably because the percentage classified as deviant in a given instance indicates the seriousness of the offense being considered—visual inspection suggests that the greater the number of offenders, the less serious the offense. Hence, the results provide empirical support for the argument that the extent to which male deviance exceeds female deviance varies directly with the seriousness of the offense. The metric coefficient for type of data (.221) shows that there is greater disparity in the amount of deviance committed by males and females when official statistics are used. But, since data type is a dichotomous variable, we can be more specific. Given two studies of equal sample size, identical types of samples, the same number of categories of deviance, equal percentages classified as deviant, and both drawn in the same year, the gamma from the study using official data will be, on the average, .221 higher than the gamma using self-reports. The variable scale indicates whether the measure of deviance used in a particular instance is based on a single behavior or on two or more behaviors combined in some type of index. The results suggest that the gap between males and females is smaller when the indicator of deviance consists of more than one behavior. Possibly multiitem indexes of deviance are more likely to contain offenses that are less serious, and if females are more likely to commit less serious offenses, the differences in the sophistication of the measure will reflect this.

But the most important of the methodological variables is the year the data were gathered. Much theoretical debate has centered on the question of whether males and females are becoming more similar in deviance as the struggle for female equality has progressed. Hence, it is especially noteworthy that the data do show that the gap between males and females in the amount of deviance committed has been narrowing over time. But it is possible that this convergence may be more salient for certain demographic groups—a question to which we will turn later in the analysis.

The set of variables indicating type of offense account for 26.7% of the variance in the relationship between sex and deviance. These variables are dummy coded, and the analysis hinges on a suppressed category containing cases where the type of deviance was not specified (e.g., police contact, arrest) or where an index score was used which involved two or more types of acts. Gamma was regressed on the four offense categories, type of data used, and the year the data were collected, systematically rotating the suppressed category to determine if the sex-deviance relationship varies by type of offense, net of year and data type. This procedure showed that the gap between males and females is least for personal offenses (e.g., drug and alcohol violations). The gender discrepancy is intermediate for youth offenses (e.g., truancy, school problems) and property offenses (e.g., theft, embezzlement, fraud). Finally, males and females are found to differ most in involvement with violent offenses.

While the preceding analysis offers evidence of the effects of specific variables within three general categories (subject characteristics, methodological characteristics, and offense type), it is desirable to determine the net effect of each variable. When we regressed gamma on all 20 variables simultaneously, these variables explained 47.7% of the variance in the gammas; but, because many of these 20 variables add very little, we reestimated the equation by successively deleting those variables which made no significant contributions to the total variance in the sex-deviance relationship. The results are reported in Table 2.

Table 2 indicates that the year the data

Table 2. Regression Coefficients of Gamma Measures on Selected Characteristics of the Studies.*

Variable		Metric	Beta
Personal Offenses		−.299	−.288
Youth Offenses		−.178	−.248
Property Offenses		−.006	−.009
Violent Offenses		.091	.127
Year Data Gathered		−.010	−.264
Data Type		.201	.243
% Classified Deviant		−.001	−.078
Scale		−.032	−.073
Urban		.068	.107
Nonwhite		−.070	−.081
Age		.048	.062
R	.676		
R^2	.457		
Constant	.766		
N	1118		

* All variables significant except property offenses.

were gathered has a strong negative effect on the magnitude of gamma. But in order to answer the relevant theoretical questions, it is desirable to assess the effect of year the data were gathered by type of offense, subject characteristics, and types of data (self-report or official). Technically, we will compare the slopes for year *across* offense categories, subject characteristics, and type of data. Data for these comparisons are presented in Table 3.

The coefficients in the first column of Table 3 suggest a significant decline over time in the magnitude of the sex-deviance relationship for all offense types.[3] Additionally, the statistical tests for equality of the slopes indicate that the convergence between gender and deviance is significantly greater for personal and youth offenses than for property and violent offenses.[4] Moreover, the rate of convergence is similar across SES groups (t=1.075), for intact versus not-intact family situations (t=1.009), and in rural and urban places (t=.676).

However, the sex-deviance relationship has not declined at the same rate for whites and nonwhites. The data suggest that the gap between white males and females in amount of deviance/crime engaged in has remained stable over time (beta = −.006), while a significant convergence has occurred among nonwhite populations (beta = −.285), and the statistical tests indicate that the slopes were probably drawn from different populations (t=3.898). Similarly, the rate of convergence differs significantly across age categories. Although the gender-deviance relationship has narrowed for both youth and adults, the decline has been more rapid among youth than among adults (t=3.880). Finally, the analysis documents that both self-report and official data studies portray a narrowing gap between males and females in involvement in deviance. However, the self-report data indicate that the gender disparity is declining more rapidly than was

---

[3] To obtain the set of variables used as controls, we first regressed gamma on all 20 independent variables for the subsamples of violent, property, personal, and youth offenses. We then reestimated each of the four equations with only those variables which were significant when all were included in the original equation. Year was included even if it was not significant. Then, any variable which was significant in *any* of the four equations was selected to be included in the final pool of control variables. We did this so that each estimate of the effect of year for violent, personal, youth, and property offenses would be net of a common set of control variables. The specific variables used as controls for the estimates by offense types were (1) data type; (2) sample size; (3) percentage classified as deviant; (4) nonwhite; (5) urban; (6) age of respondents; (7) high SES and (8) scale. Thus, we estimated one equation with year, and with these eight controls, four times (once for each offense category); and the coefficients in Table 3 are the coefficients, for year, from this equation. The same logic was followed for data type and population comparisons.

[4] Statistical significance of metric coefficients across equations was measured by:

$$t = \frac{b_1 - b_2}{[V_1(SEb_1^2) + V_2(SEb_2^2)/(V_1 + V_2)]^{1/2}},$$

where b_1 = metric coefficient for population 1
V_1 = N − K − 1 for population 1 with N = sample size, K = number of regressors;
V_2 = N − K − 1 for population 2;
SEb_1 = standard error of metric coefficient for population 1.

The specific t-ratios for the slopes of year by offense categories are:

	PERSONAL	YOUTH	PROPERTY
VIOLENT	1.929	2.320	.656
PERSONAL		.293	2.735
YOUTH			3.149

Table 3. Slopes of Year by Offense Type, Data Type and Subject Characteristics[a]

Offense Type	Net Effects[b]	Data Type	Net Effects
Violent	-.0069*[c]	*Self-Report*	-.0132*
N = 245	-.172[d]	N = 948	-.372
(.561)[f]	(.0020[e])	(.344)	(.0010)
Personal	-.0112*	*Official*	-.0050*
N = 187	-.311	N = 170	-.191
(.170)	(.0025)	(.723)	(.0019)
Youth	-.0119*		
N = 246	-.395		
(.253)	(.0023)		
Property	-.0057*		
N = 349	-.164		
(.468)	(.0017)		
Subject Characteristics			
Low SES	-.0098	*Urban*	-.0110*
N = 130	-.130	N = 358	-.232
(.326)	(.0065)	(.401)	(.0022)
High SES	-.0169*	*Rural*	-.0093*
N = 130	-.207	N = 85	-.252
(.309)	(.0067)	(.317)	(.0036)
Intact Family	-.0144*	*Youth*	-.0112*
N = 74	-.291	N = 910	-.294
(.266)	(.0064)	(.356)	(.0010)
Not-Intact Family	-.0090	*Adults*	-.0066*
N = 74	-.174	N = 208	-.204
(.301)	(.0053)	(.600)	(.0017)
White	-.0005		
N = 158	-.006		
(.366)	(.0045)		
Nonwhite	-.0203*		
N = 158	-.285		
(.289)	(.0056)		

[a] Dependent variable is a gamma measure of the magnitude of the association between sex and deviance.

[b] See footnote 3.

[c] Metric coefficient.

[d] Standardized coefficient.

[e] Standard error of metric coefficient.

[f] Mean gamma for "instances" in a particular classification; .561 = mean gamma for all tables for violent offenses.

* Significant as indicated by double its standard error.

evidenced by official data sources (t=8.868).

SUMMARY AND DISCUSSION

Accumulated data suggest that the magnitude of the relationship between sex and deviance/criminality is smaller for: (1) self-report vs. official data sources; (2) personal and youth offenses vs. violent and property offenses and undifferentiated official records; (3) composite indicators of deviance vs. single behavior measures; and (4) nonwhite vs. white populations. Further, the sex-deviance relationship is larger for both adults vs. youth and urban vs. rural samples. Moreover, as the percentage classified as deviant in a particular instance increases, the magnitude of the sex-deviance relationship decreases. Finally, over time, the association between sex and deviance has been decreasing.

But the converging trend over time in the sex-deviance relationship can be more precisely specified. The data indicate a narrowing of the gap between males and females in instances of youth, personal, property, and violent offenses. But the rates of convergence vary across offense categories, suggesting that the leveling of deviant/criminal conduct by sex is specific to the type of offense. Indeed, it appears that women *are* closing the gap in terms of involvement in minor deviant acts but that equal gender representation in the area of serious criminal behavior has not yet occurred. And the trend toward equality in this arena is moving at a slower pace vis-à-vis involvement in less serious deviant acts. In addition, while the gender-deviance relationship is declining for both youths and adults, the data suggest that this trend is stronger for youths. This seems consistent with the view that women may be experiencing greater structural opportunities to commit deviant acts due to a leveling-out of the social roles of men and women. It is at least plausible that shifting sex-role ideologies may be more salient for younger females and, thus, may have a greater impact upon the behavior of this group. Indeed, the fact that the convergence noted in the sex-deviance relationship is stronger for youths is consistent with this hypothesis.

One of the most interesting findings is the interaction with race. The data show a significant convergence in male and female involvement in deviance/criminality for nonwhites but no discernible pattern for whites. Thus, the deviant conduct of black females is becoming more similar to that of black males. This finding is consistent with much stratification research which indicates a greater similarity of the economic and social roles of black females vis-à-vis black males, compared to white females vis-à-vis white males (e.g., Freeman, 1973; Treiman and Terrell, 1975). This finding also seems to support the argument that greater social equality of women is associated with a decrease in the relative disparity of deviant conduct, by sex. This interpretation follows from the fact that black women have experienced more rapid advances in educational and occupational attainment relative to black men, than have white women experienced relative to white men. Thus, as the social and economic roles of black men and women have been converging, their relative propensity toward deviant/criminal conduct has also narrowed.

One other interesting finding of the present research is the significant contrast between self-report and official data samples. While the relative involvement of males and females in deviance/crime is trending toward similarity for both self-report and official measures, the rate of decline is significantly greater for self-reported deviance. This difference may reflect the less serious item content of most self-report data (e.g., Hindelang et al., 1979). But while this argument seems plausible for explaining *static* differences in the magnitude of the sex-deviance relationship, the extension of this argument for explaining the different time trends seems less clear. For example, if the slopes of year for self-report and official data were parallel, then the difference between the *indicators* of male and female involvement in deviance would be a constant, over time. But the data reported in Table 3 reject the hypothesis of parallel slopes. Specifically, if the magnitude of the sex-deviance relationship evidenced by official data is some value (X) for a given year, and the gamma for the self-report data for the same year is (X-D) (where D is a measure of difference), then the present data suggest that, over time, D has been increasing. Thus, the difference between official and self-report measures of the sex-deviance relationship is expanding over time. While variable item content may help to explain the structural difference of these two slopes at a given point in time, other factors may be relevant in accounting for the different trends. One possibility may be that self-report and official indicators of deviance are not measuring the same latent variable (see Bridges, 1978). Thus, some interesting interpretations emerge. It may be that official data taps a dimension of serious criminal involvement while the self-report data deals with only a truncated distribution of deviant acts (those on the less serious end of the continuum). If this is the case, then the extant empirical literature suggests that, over time, men and women are becoming more similar in involvement in marginal (i.e., less serious) deviant acts, but that, for the more serious deviant acts, the convergence between men's and women's involvement is less rapid. Alternatively, the self-report measures may be more sensitive to actual changes in the deviant/criminal behavior of men and women than is the official data which may be an index of the response of social control agencies to that behavior. Given this interpretation, the present data would suggest that the gap between males and females in involvement in deviance/crime is narrowing, but that official reactions to this change are occurring at a significantly slower rate.

REFERENCES

Adler, Freda
1975 Sisters in Crime. New York: McGraw-Hill.
1977 "The interaction between women's emancipation and female criminality: a cross-cultural perspective." International Journal of Criminology and Penology 5:101–12.

Adler, Freda, and Rita J. Simon
1979 The Criminality of Deviant Women. Boston: Houghton Mifflin.

Akers, Ronald L.
1964 "Socio-economic status and delinquent behavior: a retest." Journal of Research in Crime and Delinquency 1:38–46.

Austin, Roy
1978 "Race, father-absence and female delinquency." Criminology 15:487–504.

Ball, John C., Alan Ross, and Alice Simpson
1964 "Incidence and estimated prevalence of recorded delinquency in a metropolitan area." American Sociological Review 29:90–6.

Barker, Gordon, and William Adams
1962 "Comparison of the delinquencies of boys and girls." Journal of Criminal Law, Criminology and Police Science 53:470–5.

Berger, Alan S., and William Simon
1974 "Black families and the Moynihan report: a research evaluation." Social Problems 22:145–61.

Bertrand, Marie-Andres
1969 "Self image and delinquency: a contribution to the study of female criminality and women's image." Acta Criminologica 2:71–138.

Bridges, George
1978 "Errors in the measurement of crime." Pp. 9–29 in Charles Wellford (ed.), Quantitative Studies in Criminology. Beverly Hills: Sage.

Brown, James W.
1972 "A test of a three-stage learning model of drug use." Criminology 9:449–66.

Burkett, Steven, and Eric Jensen
1975 "Conventional ties, peer influence and the fear of apprehension: a study of adolescent marijuana use." Sociological Quarterly 16:522–33.

Cernkovich, Stephen, and Peggy Giordana
1979 "A comparative analysis of male and female delinquency." Sociological Quarterly 20:131–45.

Chesney-Lind, M.
1973 "Judicial enforcement of the female sex role: the family court and the female delinquent." Issues in Criminology 8:51–71.

Cockburn, James J., and Inga Maclay
1965 "Sex differentials in juvenile delinquency." British Journal of Criminology 5:289–308.

Cowie, J., V. Cowie, and E. Slater
1968 Delinquency in Girls. London: Heineman.

Crites, Laura
1976 "Women offenders: myth vs. reality." In Laura Crites (ed.), The Female Offender. Lexington: Lexington Books.

d'Orban, P. T.
1972 "Female crime." Criminologist 7:29–51.

Datesman, S., Frank R. Scarpitti, and Richard M. Stephensen
1975 "Female delinquency: an application of self and opportunity theories." Journal of Research and Crime in Delinquency 12:107–23.

Deming, Richard
1977 Women: The New Criminals. Nashville: Thomas Nelson.

Dentler, Robert, and Lawrence Monroe
1961 "Social correlates of early adolescent theft." American Sociological Review 26:733–43.

Forslund, Morris A.
1970 "Standardization of Negro crime rates for Negro-white differences in age and status." Rocky Mountain Social Science Journal 7:151–60.

Freeman, Richard
1973 "Changes in the labor market for black Americans." Pp. 67–132 in Arthur Okun and George Perry (eds.), Brookings Papers on Economic Activity. Washington, D.C.: Brookings Institute.

Gibbons, Don, and Manzer Grizwold
1957 "Sex differences among juvenile court referrals." Sociology and Social Research 42:106–10.

Gold, Martin
1966 "Undetected delinquent behavior." Journal of Research in Crime and Delinquency 3:27–46.
1970 Delinquent Behavior in an American City. Belmont: Brooks/Cole.

Gold, Martin, and David Reimer
1974 "Changing patterns of delinquent behavior among Americans 13–16 years old: 1967–1972." Crime and Delinquency Literature 6:483–517.

Green, Edward
1970 "Race, social status and criminal arrest." American Sociological Review 35:476–90.

Haskell, M. R., and L. Yablonsky
1974 Crime and Delinquency. Chicago: Rand McNally.

Hathaway, Starke R., E. D. Monachesi, and L. A. Young
1960 "Delinquency rates and personality." Journal of Criminal Law, Criminology and Police Science 50:433–40.

Havighurst, Robert, Paul H. Bowman, Gordon Little, Charles Matthews, and James Pierce
1962 Growing Up in River City. New York: Wiley.

Heidensohn, Frances
1968 "The deviance of women: a critique and an enquiry." British Journal of Sociology 19:160–75.

Heise, David R.
1968 "Norms and individual patterns in student deviancy." Social Problems 16:78–92.

Higgins, Paul, G. L. Albrecht, and M. H. Albrecht
1977 "Black-white adolescent drinking: the myth and the reality." Social Problems 25:215–24.

Hindelang, Michael J.
1971 "Age, sex and the versatility of delinquent behavior." Social Problems 18:522–34.
1973 "Causes of delinquency: a partial replication and extension." Social Problems 20:471–87.
1974 "Moral evaluation of illegal behaviors." Social Problems 21:370–85.

Hindelang, Michael J., Travis Hirschi, and Joseph G. Weis
1979 "Correlates of delinquency." American Sociological Review 44:995–1014.

Hirschi, Travis, and Rodney Stark
1969 "Hellfire and delinquency." Social Problems 17:202–13.

Jensen, Gary, and Raymond Eve
1976 "Sex differences in delinquency." Criminology 13:427–48.

Klein, Dorie, and June Kress
1976 "Any women's blues: a critical overview of women, crime and the criminal justice system." Crime and Social Justice 5:34–49.

Kratcoski, John, and Peter Kratcoski
1975 "Changing patterns of boys and girls." Adolescence 10:83–91.

Lowe, Jay
1966 "Prediction of delinquency with an attitudinal configuration model." Social Forces 45:106–13.

Mannheim, Hermann
1965 Comparative Criminology. London: Routledge and Kegan Paul.

Morris, Ruth R.
1965 "Attitudes towards delinquency by delinquents, non-delinquents and their friends." British Journal of Criminology 5:249–65.

Moses, Earl R.
1947 "Differentials in crime rates between Negroes and whites based on comparisons of four socio-economically equated areas." American Sociological Review 12:411–20.

Mott, Jay
1973 "London juvenile drug offenders." British Journal of Criminology 13:209–17.

Mueller, John, Karl Schuessler, and Herbert Costner
1977 Statistical Reasoning in Sociology (3rd ed.). Boston: Houghton Mifflin.

Nagel, S., and L. J. Weitzman
1972 "Double standard of American justice." Society 9:18–63.

Newberg, Paul M.
1967 "The female of the species: a systematic review of theories, evaluations and studies of the female criminal." Interdiscipline 4:29–42.

Noblit, George W., and Janie M. Burcart
1976 "Women and crime: 1960–1970." Social Science Quarterly 56:650–7.

Norland, Stephen, and Neal Shover
1977 "Gender roles and female criminality." Criminology 15:87–103.

Nye, F. Ivan
1958 Family Relationships and Delinquent Behavior. New York: Wiley.

Oakley, A.
1972 Sex, Gender and Society. New York: Harper and Row.

Palmore, Erdman, B., and Phillip Hammond
1964 "Factors in juvenile delinquency." American Sociological Review 29:848–54.

Pollack, Otto
1950 The Criminality of Women. Philadelphia: University Press.

Porterfield, Austin
1946 Youth in Trouble. Fort Worth: Leo Potishman Foundation.

Przeworski, Adam, and Henry Teune
1970 The Logic of Comparative Social Inquiry. New York: Wiley.

Radzinowicz, L.
1937 "Variability of the sex-ratio of criminality." American Sociological Review 29:76–102.

Reige, Mary
1972 "Parental affection and juvenile delinquency in girls." British Journal of Criminology 12:55–73.

Rhodes, Albert, and Albert Reiss, Jr.
1970 "The 'religious factor' and delinquent behavior." Journal of Research in Crime and Delinquency 7:83–98.

Robin, G. D.
1963 "Patterns of department store shoplifting." Crime and Delinquency 9:163–72.

Short, James F., and F. Ivan Nye
1958 "Extent of unrecorded juvenile delinquency: tentative conclusions." Journal of Criminal Law, Criminology and Police Science 42:296–302.

Simon, Rita J.
1975 Women and Crime. Lexington: Lexington Books.
1976 "Women and crime revisited." Social Science Quarterly 56:658–63.

Simon, Rita J., and Navin Sharma
1979 "Women and crime: does the American experience generalize?" in Freda Adler and Rita J. Simon (eds.), The Criminology of Deviant Women. Boston: Houghton Mifflin.

Slocum, Walter, and Carol Stone
1963 "Family culture patterns and delinquent type behavior." Journal of Marriage and the Family 25:202–8.

Steffensmeier, Darrell J.
1978 "Crime and the contemporary woman: an analysis of changing levels of female property crime; 1960–75." Social Forces 57:566–84.

Steffensmeier, Darrell J., R. Steffensmeier, and A. Rosenthal
1979 "Trends in female violence." Sociological Focus 12:217–27.

Stewart, Cyrus, M. Senger, A. Vener, and L. Krupka
n.d. "Patterns of delinquency among adolescent girls." Unpublished manuscript. Michigan State University.

Stinchcombe, Arthur
1964 Rebellion in a High School. Chicago: Quadrangle.

Suval, E. M., and R. C. Brisson
1974 "Neither beauty nor beast: female criminal homicide offenders." International Journal of Criminology and Penology 2:23–34.

Tittle, Charles R., and Wayne J. Villemez
1977 "Social class and criminality." Social Forces 56:474–502.

Tittle, Charles R., Wayne J. Villemez, and Douglas A. Smith
1978 "The myth of social class and criminality." American Sociological Review 43:643–56.

Treiman, Donald, and Kermit Terrell
1975 "Sex and the process of status attainment." American Sociological Review 40:174–201.

Vedder, C. B., and D. B. Somerville
1970 The Delinquent Girl. Springfield: Charles C Thomas.

Voss, Harwin L.
1963 "Ethnic differentials in delinquency in Honolulu." Journal of Criminal Law, Criminology and Police Science 54:322–7.
1964 "Differential association and reported delinquent behavior: a replication." Social Problems 12:78–85.

Voss, Harwin, and John Hepburn
1968 "Patterns of criminal homicide in Chicago." Journal of Criminal Law, Criminology and Police Science 59:499–508.

Walberg, Herbert, Elain G. Yeh, and Stephen M. Patton
1975 "Family background, ethnicity and urban delinquency." Journal of Research in Crime and Delinquency 11:80–7.

Wallerstein, James, and Clement Wyle
1947 "Our law-abiding law-breakers." Probation 25:107–12.

Ward, David A., Maurice Jackson, and Renee E. Ward
1969 "Crimes of violence by women." Pp. 864–89, 906–9 in Donald Mulvihill et al. (eds.), Crimes of Violence. Washington: U.S. Government Printing Office.

Weis, Joseph
1976 "Liberation and crime: the invention of the new female criminal." Crime and Social Justice 6:17–27.

Williams, Jay R., and Martin Gold
1972 "From delinquent behavior to official delinquency." Social Problems 20:209–29.

Wise, Nancy B.
1967 "Juvenile delinquency among middle-class girls." Pp. 179–88 in Edmund W. Vaz (ed.), Middle-Class Juvenile Delinquency. New York: Harper and Row.

Wolfgang, Marvin
1958 Patterns in Criminal Homicide. New York: Wiley.

COMMENTS

ON "STRATIFICATION IN A DUAL ECONOMY"*

(COMMENT ON BECK ET AL., *ASR*, OCTOBER, 1978)

Beck et al. (1978) contrast what they term "individualistic" and "structural" theories of the distribution of earnings in an analysis of data from the 1975 and 1976 General Social Surveys (GSS) of the National Opinion Research Center (NORC) (1977). The purposes and results of their study are aptly summarized in their conclusion (Beck et al., 1978: 717):

> Our analysis examines the existence and the importance of industrial sectors, as hypothesized by the dual economy literature, on the process of earnings determination. Since the status attainment and neoclassical income models rest on the assumption of labor market homogeneity, our efforts to test a theoretically-derived model of nonhomogeneity are of direct relevance for those research traditions. Using a distinction between core and periphery industrial sectors derived from Bluestone et al. (1973), we address our analysis to three basic questions: (1) Are there significant differences in the economic status and composition of the core and periphery labor forces? (2) Can sectoral differences in economic status be reduced to differences in the quality of their respective labor forces? And, (3) to what degree are the economic returns to sex, race, human capital, and occupation-labor force variables the same in both sectors?
>
> The analysis provides clear answers to all three questions. The core and periphery sectors *do* exhibit significant differences in both earnings levels and in labor force composition. The sectoral differentials in earnings *cannot* be explained away by differences in labor force quality. The relationships between earnings and human capital as well as occupation-labor force variables *do* differ significantly between core and periphery sectors. Whether considering the biologically-fixed attributes of race and sex, the human capital variables, or the occupation-labor force variables, we find that the real dollar returns on these worker characteristics are greater in core industries than in periphery industries.
>
> The importance of these findings for the neoclassical research tradition in stratification should be clear. In face of strong empirical evidence contrary to the implicit tenet of labor market homogeneity, parameter estimates which are conditional on this assumption can no longer be treated as appropriate bases for the construction of sociological theory or social policy. Such simplistic models lead to a serious misspecification and misrepresentation of the social processes underlying individual earnings determination. In contrast, the notion of economic sectors appears to hold substantial promise as a theoretical concept and as an exemplar for a research program aimed at identifying structural aspects of the socioeconomic order.

There is no reason to question the first two of Beck et al.'s (1978) three major findings: industrial differentials in labor force composition and remuneration are well known. Their analysis and its conclusions focus primarily upon the third finding, "the relationships between earnings and human capital as well as occupation-labor force variables *do* differ significantly between core and periphery sectors." For example, Beck et al. (p. 706) state, "the present analysis will focus on the issue of fixed returns—the assumption that economic returns to worker characteristics are uniform." On this point their analysis is wrong. Beck et al. incorrectly classify a small number of GSS respondents as zero earners. These few observations dominate their regression analyses. As a result, Beck et al. "detect" several nonexistent interaction effects; they also fail to detect one important interaction effect. Their regression analyses are wrong in virtually every detail.

Beck et al. (1978) begin their analysis by imputing "individualistic" assumptions of perfect competition and labor market

---

* Direct all communications to: Robert M. Hauser; Department of Sociology; University of Wisconsin–Madison; 1180 Observatory Drive; Madison, WI 53706.

This research was supported by the National Institute of Mental Health (Grant No. MH-06275). NORC-GSS data were obtained from the Data and Program Library Service of the University of Wisconsin–Madison. Computations were carried out using facilities of the Center for Demography and Ecology, provided with core support from the National Institute of Child Health and Human Development (Grant No. HD-05876). I thank Shu-Ling Tsai for extensive and careful research assistance. E. M. Beck has kindly responded to my requests for additional information about his research. I thank Glen Cain, O. D. Duncan, Neil Fligstein, Arthur S. Goldberger, Charles Halaby, Susan Janssen, Arne Kalleberg, Robert Kaufman, Robert D. Mare, William Mason, Rachel Rosenfeld, Aage Sørensen, Seymour Spilerman, Bruce Stephenson, and Ross M. Stolzenberg for their helpful reactions to an earlier draft of this paper.

homogeneity to research in the "neo-classical" human capital and status attainment traditions. The economists can speak for themselves; for an enlightening statement, see Cain (1976). I think that the theoretical assertions about status attainment research are patently inaccurate. For example, in the sociological works cited by Beck et al. (p. 705) as "individualistic in orientation and execution"—Hauser and Featherman (1977), Sewell and Hauser (1975), and Duncan et al. (1972)—one need not look far to find analyses of heterogeneity in the attainment process across populations defined by such variables as race, sex, age, calendar year, city size, and migration status, whose sociological explanation is not "tied directly to the characteristics brought into the marketplace by the individual workers" (Beck et al., p. 705). Far from assuming fully competitive and homogeneous labor market processes, a major thrust of recent stratification research has been to demonstrate the variety of social and psychological mechanisms (extending over the life cycle and affecting major social groups in different ways) which tend to complicate and to invalidate such narrow theoretical frameworks.

The representation of labor market sectors in earnings functions is a straightforward theoretical and empirical elaboration of previous status attainment models (Featherman and Hauser, 1978: chap. 9). In fact, the Duncan SEI (Reiss, 1961) incorporates a much larger number of distinctions of socioeconomic standing by industry within occupation than does the prestige scale employed by Beck et al. (1978). However, in attempting to elucidate the effects on earnings of industrial sectors as defined by Beck et al., I do not want to imply my agreement with their concept that there are just two economic sectors, with their assignment of each industry to "core" or "periphery" sectors, nor with their use of industries, rather than firms, to designate labor market sectors. I suspect that important interfirm and interindustry differences in earnings are obscured in the analysis of Beck et al.

To avoid confusion, I use "sectoral interaction effects" to refer to differences in slopes in semilogarithmic earnings functions, like those in the left-hand panel of Table 3 of Beck et al. (1978:713). I do not refer to "real dollar" differences in the effects of variables as interaction effects. In this respect I believe that I am following the intent of Beck et al. (see p. 712, par. 3 and 4, and p. 714, par. 3 and 4). However, they also take note of "real dollar" differences in effects at some points in the text (pp. 714, 715, 717). In the semilog earnings function, each unit change in a regressor leads to a proportionate change in earnings, so the effect of a regressor in dollars depends on the value of the regressor at which its effect is evaluated (and on the values of other variables in the function). In the dollar earnings function, each unit change in a regressor leads to the same change in dollar earnings, regardless of the value of the regressor (or of other variables in the function). In the population, so long as sectors differ in intercepts of the earnings function, constant nonzero slopes in the semilog functions imply differences in real dollar effects, and constant nonzero dollar effects imply different slopes in the semilog functions. If one accepts both forms of interaction as evidence of heterogeneity in the labor market process, it is not logically possible to reject the hypothesis of heterogeneity.

There are good reasons to choose the semilog form of the earnings function, which is widely applied in economics and sociology. First, it has a convenient interpretation—that variables have proportionate effects on earnings. Under some circumstances, such effects can be interpreted directly as rates of return (Mincer, 1974). Moreover, one may expect proportionate effects to be invariant with respect to global price and productivity changes (or even to changes in units of currency across societies), while effects in raw units of currency will not be invariant. Second, the semilog form of the earnings function has desirable statistical properties relative to the linear (dollar) form. The residuals (deviations from the regression line) are typically less skewed and less heteroscedastic when the semilog function is applied. There is also evidence that the semilog function fits earnings data better than the linear (dollar) function or other power functions of earnings (Heckman and Polachek, 1974).

It is unfortunate that Beck et al. identify heterogeneity in labor market processes with heterogeneity in regression slopes, for that identity holds only where there is a strong theoretical justification for the functional form in which heterogeneity appears. For example, the statisticians Mosteller and Tukey (1977) invoke parsimony as a criterion in the choice of functional forms; a researcher may radically transform a dependent variable in order to obtain good fit with a simple function of explanatory variables. In the context of such a flexible statistical analysis, heterogeneity of slopes is but a methodological nuisance. Obversely, one might infer substantive heterogeneity from functions with homogeneous slopes.

In my view, Beck et al. (1978) offer no substantial theoretical rationale to predict the occurrence or form of a specific statistical interaction in their regression analysis. Moreover, they provide no theoretical rationale for semilog or linear forms of the earnings function that would support the inference of heterogeneity in the earnings process, regardless of empirical findings. Indeed, while Beck et al. use a semilog function to argue for sectoral heterogeneity, the same authors elsewhere use a linear function to make the same argument (Tolbert et al., 1980).

Beck et al. (1978:711) report that the mean logarithm of earnings is 8.5993 in the core with a standard deviation of 2.1148, and the mean is 7.7706 in the periphery with a standard deviation of 2.5628. These numbers are not credible. For example, they imply that 1975 dollar earnings at the mean in the core are $5,428 (= $e^{8.5993}$); earnings at the mean in the periphery are $2,370. As a rough approximation, the log of earnings is normally distributed; thus, one would expect dollar earnings at the mean of the distribution of the log of earnings to fall near the median of the distribution of dollar earnings. In the cumulative codebook of the GSS, the median current dollar earnings of respondents are roughly $7,400 in the 1975 and 1976 GSS (NORC, 1977:45). Even in the core the figure reported by Beck et al. is about $2,000 less than one would expect from the assumption of lognormality. That is not all. At one standard deviation above the mean, their figures imply annual earnings of $44,986 in the core and $30,742 in the periphery; the highest category in the NORC (1977) earnings reports is "$25,000 or more." At one standard deviation below the mean they imply annual earnings of only $655 in the core and $183 in the periphery. Something is wrong.

What accounts for the anomalously low means and large standard deviations in the distributions of the log of earnings? One reason to take the log of earnings is to reduce the positive skew of the distribution. In compressing the high end of the dollar metric, the logarithmic transformation also stretches out the low end of the distribution (Mosteller and Tukey, 1977:chap. 4). Thus, the anomalies might be explained by observations of no earnings or very low earnings. Since the log of zero is undefined, it is common practice to assign $1 (or some other nominal sum) to zero earners before taking logs. When $1 is assigned, the log is 0 by construction. This rule of thumb may generate extreme outliers (see Jencks et al., 1979:283–4, 374–5). There are a number of better practices: add a larger nominal value (for example, $500 or $1,000) to each observation; use a functional form which gives special treatment to zeros (Tobin, 1958; Sewell and Hauser, 1975:chap. 6; Goldberger, 1964:253–5); ignore the outlying observations (Featherman and Hauser, 1978:chap. 5); or choose a less radical transformation of earnings (Jencks et al., 1979:283–4).

The GSS (NORC, 1977) uses a closed-ended question to ascertain earnings in the calendar year before the survey date. Each respondent chooses a broad income category from a card offered by the interviewer. The lowest category on the card is "under $1,000." Even if one assumed a uniform earnings distribution from $0 to $1,000, the midpoint of the interval would be $500. If log(500) = 6.21 were the minimum of the distribution of the log of earnings, it could not account for the anomalous observations.

There are 99 assigned values of zero in the distributions of the log of earnings used by Beck et al. (Beck has acknowledged this in a private communication.) Moreover, these assigned values of zero do not occur primarily (or even secondarily) among persons holding low-paying jobs in the year before the survey date. They occur among persons who did not work at all in the year before the survey date or who changed occupations between the end of that year and the survey date (usually in March or April). Not only do the values of zero on the log of earnings give the distribution extreme negative skew, but the outliers have no clear connection with the occurrence of low pay.

The documentation of the GSS (NORC, 1977) suggests how Beck et al. (1978) made this error. By assigning zeros to the log of earnings in certain cases from the GSS, I have been able to approximate their results rather closely. When the observations with assigned zeros are removed from my analysis, the anomalies disappear. So do major findings of Beck et al.

In the 1975 and 1976 GSS (NORC, 1977), respondents were classified as in the labor force if they (1) were employed full or part-time, (2) had a job but were not at work, or (3) were unemployed (NORC, 1977:13, Q. 1). These and other persons with work experience were asked about their present or past occupation: "What kind of work do you (did you) normally do? That is, what (is/was) your job called?" (NORC, 1977: Q. 2-A, p. 15). At Q. 35, respondents were asked (NORC, 1977:44–5):

> 35. Did you earn *any* income from (OCCUPATION DESCRIBED IN Q. 2) in [1972/73/74/75/76]?
>
> Yes . . . . (ASK A)
> No . . . . [See *REMARKS*]
>
> A. *IF YES:* In which of these groups did your earnings from (OCCUPATION IN Q. 2) for last year—[1972–1976]—fall?

> That is, before taxes or other decuctions. Just tell me the letter.
>
> [HAND CARD E]
>
> *REMARKS*: Card E contained responses for punches 01–12 only. Q. 35 responses are not in these data, but are contained in Q. 35-A.

Question 35 states that persons with no earnings from the occupation described in question 2 were not asked to report personal earnings in the previous year; their earnings were coded as "blanks" for "not applicable" in the GSS file. Limitation of the personal earnings question to earnings in the named occupation is reinforced by the interviewer instructions (NORC, 1977:179):

> *1976*
>
> Note instruction: SEE Q. 2. IF ANY OCCUPATION RECORDED, ASK Q. 35. Read job title from Q. 2. Record R's income only from job described in Q. 2, not from any other jobs or sources. This question is concerned with OCCUPATION, rather than specific job or employer. EXAMPLE: If R's occupation was "waitress," you would ask for total income in 1975 as a waitress . . . even if she worked in several different places during the year.
>
> *1974–75*
>
> NOTE INSTRUCTIONS. Read job title from Q. 2. This question is concerned *only* with any income R. earned from occupation described in Q. 2, not any other income from other sources or income of other family members.

Beck et al. (1978) treated current members of the labor force who had no earnings in the named occupation in the previous year as if they were zero earners by replacing "blanks" in the dollar earnings variable with zeros in the logarithmic transformation of earnings. This procedure implies that persons who were not in the labor force at all in the previous year were treated as extreme low earners in that year. It implies that persons who changed occupations after the beginning of the survey year were treated as extreme low earners in the previous year. It implies that persons who changed occupations during the previous year had only part of that year's earnings covered by the GSS personal earnings question. To be sure, some persons may have been unemployed throughout the previous year and thus have been classified legitimately as in the labor force but without earnings in that year. I think that such persons are small in number, relative to those improperly classified as nonearners, and it would in any event be questionable to assign zeros to such persons on the log of earnings.

Unfortunately, the GSS (NORC, 1977) data lack the information required to enumerate persons properly and improperly classified as nonearners. Given the broad and heterogeneous make-up of the sample selected by Beck et al. (1978), which included all women in the labor force at the survey date and did not impose an upper age limit, I think it probable that most nonearners were improperly classified. Of 1,695 GSS respondents in the labor force at the survey date, Beck et al. (p. 708) excluded 12 for whom industry was not reported. Of the remaining 1,683 persons in their sample, 99 were assigned "blanks" on personal earnings in the previous year—53 in the core and 46 in the periphery.

Some light is shed on the validity of the classification of nonearners by the GSS item on work stability (NORC, 1977:123, Q. 132):

> At any time during the last ten years, have you been unemployed and looking for work for as long as a month?

Among the 99 current members of the labor force with "blanks" on earnings in the previous year, 53% (53 cases) reported a spell of unemployment as long as one month in the previous decade. In the unlikely case that *all* of these "unstable" workers had been unemployed throughout the previous year, only 53 cases would properly have been classified as unemployed nonearners. I believe this is an unreasonably high upper bound on the number of persons properly classified as nonearners, for 32% of earners also reported a one-month spell of unemployment in the past decade.

The treatment of the 99 zero earners in the labor force, who make up less than 6% of the sample used by Beck et al. (1978), has a critical effect on the results. For example, Table 1 shows the means and standard deviations of variables used in my reanalysis of the GSS data. LNY1 is my approximation of the dependent variable employed by Beck et al. That is, I adjusted the 1974 data to 1975 dollars, placed missing observations at the mode, assigned each person the log of the midpoint of his earnings interval, and used a Pareto approximation to estimate the mean of the upper, open-ended interval; the 99 observations with zero earnings were assigned zero values on LNY1. In the case of LNY2, the procedures were the same, except that the 99 observations with zero earnings were ignored. The characteristics of LNY1 are very similar, though not identical to those of the earnings measure used by Beck et al. (p. 711). I have found other discrepancies in my reanalysis, but I think their effects are minor. The means and standard deviations of LNY2 are vastly different from

Table 1. Means and Standard Deviations of Characteristics of Workers in the Civilian Labor Force by Industrial Sectors: 1975 and 1976 NORC General Social Surveys

			Industrial Sector	
Worker Characteristic		Total N = 1683	Core N = 1125	Periphery N = 558
LN annual earnings	LNY1[1]	8.349	8.616	7.809
		(2.295)	(2.103)	(2.558)
	LNY2[2]	8.870	9.042	8.511
		(.982)	(0.887)	(1.071)
Sex (1 = female)	X_1	0.403	0.370	0.471
		(0.491)	(0.483)	(0.500)
Race (1 = nonwhite)	X_2	0.102	0.092	0.122
		(0.302)	(0.289)	(0.327)
Age of males	X_3[3]	39.39	39.632	38.810
		(13.871)	(13.290)	(15.187)
Age of females	X_4[4]	38.507	37.911	39.449
		(13.543)	(12.920)	(14.451)
Years of schooling	X_5[5]	12.385	12.853	11.443
		(2.988)	(2.975)	(2.788)
Highest degree				
high school or junior college	X_6	0.553	0.552	0.554
		(0.497)	(0.498)	(0.498)
Bachelor's degree	X_7	0.131	0.158	0.075
		(0.337)	(0.365)	(0.264)
Postgraduate degree	X_8	0.051	0.073	0.007
		(0.220)	(0.260)	(0.084)
Occupational prestige	X_9[6]	39.298	42.255	33.358
		(13.777)	(13.808)	(11.640)
Union member (1 = yes)	X_{10}	0.244	0.297	0.138
		(0.430)	(0.457)	(0.345)
Unemployed (1 = yes)	X_{11}	0.071	0.073	0.068
		(0.257)	(0.260)	(0.252)
Work stability (0 = stable)	X_{12}[7]	0.336	0.332	0.344
		(0.472)	(0.471)	(0.475)
Years parental schooling	X_{13}[8]	9.409	9.630	8.966
		(3.895)	(3.930)	(3.787)
Father's prestige	X_{14}[9]	38.906	39.404	37.871
		(12.216)	(12.478)	(11.599)

[1] Cases with "zero" earnings are assigned LNY1 = 0, and missing earnings data are assigned the modal value, $12,500 ($N_T$ = 1683, N_C = 1125, N_P = 558).

[2] Missing earnings data are coded as $12,500, but cases with "zero" earnings are excluded (N_T = 1584, N_C = 1072, N_P = 512).

[3] 2 missing data for male ages are coded as 27 or 25 for core or periphery sectors, respectively (N_T = 1004, N_C = 709, N_P = 295).

[4] 3 missing data for female ages are coded as 24 or 25 for core or periphery sectors, respectively (N_T = 679, N_C = 416, N_P = 263).

[5] 4 missing data excluded (N_T = 1679, N_C = 1122, N_P = 557).

[6] 4 missing data excluded (N_T = 1679, N_C = 1121, N_p = 558).

[7] 7 missing data excluded (N_T = 1676, N_c = 1120, N_p = 556).

[8] Parental education is taken as the years of schooling for the respondent's father. If this information is not available, data for mother's education or modal values were substituted, using an algorithm provided by Beck et al.

[9] 200 missing data excluded (N_T = 1483, N_C = 1001, N_P = 482).

those of LNY1 in both the core and the periphery. For example, in the core sector, dollar earnings at the mean of LNY2 are $8,451, compared to $5,519 in the case of LNY1. At one standard deviation above the mean, earnings based on LNY2 are $20,517, but they are $45,207 when based on LNY1. At one standard deviation below the mean, earnings based on LNY2 are $3,481, but they are $674 when based on LNY1. Similar results are obtained in the periphery, where earnings at the mean of LNY2 are $4,969, and they are $2,463 at the mean of LNY1. At one standard deviation above the mean in the periphery, earnings based on LNY2 are $14,501, and those based on LNY1 are $31,793. At one standard deviation below the mean, earnings based on LNY2 are $1,703, and those based on LNY1 are $191.

In core and periphery, the standard deviation of LNY2 is less than half as large as that of

LNY1. In the total sample, earnings at the mean of LNY2 are $7,115, a figure reassuringly close to the median current dollar earnings of $7,400. In the total sample, earnings at the mean of LNY1 are only $4,226. Thus, the presence of (inappropriate) zero observations accounts for anomalous characteristics of the earnings measure used by Beck et al. (1978).

To answer the question, "How do the zero observations affect the regression analysis?" I carried out new regression analyses of earnings in the core and the periphery, first, using all observations with LNY1 as the dependent variable, and, second, using observations with nonzero earnings and LNY2 as the dependent variable. I did not reanalyze the earnings of the subsample of full-time employed white males (Beck et al. 1978:716), nor did I reanalyze the linear probability function that Beck et al. used to estimate the incidence of poverty-level earnings (pp. 713, 716). I do not think a reanalysis of earnings in the male subgroup would add anything of substance to the present analysis; Beck et al. have provided their tabulations for that subgroup that show the same anomalies as their published tabulations for the full sample (p. 716). The incidence of poverty-level earnings, like the overall distribution of the log of earnings, is affected substantially by the inappropriate classification of job changers and persons outside the labor force as nonearners or low earners. Also, statistical defects in the linear probability function are well documented, and these are likely to be important in analyses of a highly skewed variable like the incidence of poverty.

Table 1 reports the means and standard deviations of variables used in my reanalysis of the GSS (NORC, 1977) data; the reader should compare it with Table 2 of Beck et al. (1978:711). For most variables the two analyses yield the same estimates. As noted in the table, I reproduced Beck et al.'s results for age by assigning modal values to a small number of missing observations; this procedure was not mentioned by Beck et al. I was unable to resolve a minor discrepancy in the proportion of persons with a high school or junior college degree. In the core sector my estimate of mean occupational prestige is larger, and its standard deviation is smaller than that of Beck et al. That discrepancy apparently occurred because the prestige distribution used by Beck et al. included four zero values, which are out of its legitimate range, and I dropped these from the analysis.[1] In the case of work stability I excluded nonresponses, which Beck et al. apparently did not do.

"Father's occupational prestige" deserves special mention. My estimates of its mean are smaller and my estimates of its standard deviation are larger than those of Beck et al. (1978). They randomly assigned one of three modes of the prestige distribution to substitute for missing data, so their result cannot be reproduced independently (p. 708).[2] My estimates were obtained by dropping the 200 cases where father's prestige was missing. Aside from the large number of missing observations, parental prestige had trivial negative effects and did not interact with economic sector in the earnings regressions (p. 715). For these reasons, I dropped it from the regression analysis.

Beyond the differences just noted, I have tried to repeat the analysis of Beck et al. (1978) for each version of the dependent variable (LNY1 and LNY2). In particular, I have not tried to improve the specification of the earnings function in other ways. I believe that several defects in specification remain, so my results are of interest mainly because of their differences from those of Beck et al. For example, there is ample theoretical and empirical evidence that a linear term in age does not adequately represent the effect of work experience on earnings, especially in a sample with no upper age limit.

Aside from lifetime work experience, it is also important to include more proximate measures of labor supply (that is, weeks and hours worked) in the earnings function. No such measures were obtained for the previous year in the GSS, yet it is reasonable to expect that differences in labor supply are intricately connected with sectoral differentials in earnings, as they are with sex and race differentials in earnings. This is not to say that differences in labor supply between labor market sectors or between population subgroups are either voluntary or exogenous; rather, the several mechanisms affecting labor supply are quite different from those to which Beck et al. (pp. 706–8) allude in predicting sectoral interaction effects and earnings differentials.

It is well established, also, that occupational socioeconomic status is superior to occupational prestige in representing occupational differences in earnings. Last, heterogeneity in earnings functions across major social groups (race, sex, age, and geographic location) has been a major focus of research. While Beck et al. do attempt to show sectoral heterogeneity among full-time white male workers, it is curious that they do not specify heterogeneity in the regressions by race or sex within labor market sectors. Indeed, were there not other reasons to question the validity of their regression analysis, it would be pertinent to ask whether differences in the race and sex composition of the core and periphery sectors may help to explain the observed heterogeneity across sectors in the function specified by Beck et al.

---

[1] According to Bruce Stephenson of NORC, these four cases were persons who reported jobs whose descriptions could not be coded.

[2] In order to base my analysis on an exact replication, I had hoped to work with the data file used by Beck et al. (1978). While Beck expressed his willingness to share that file with me, it had accidentally been destroyed some time before I asked for it.

Table 2 shows the results of my efforts to reproduce the earnings regressions of Beck et al. (1978) using LNY1. Columns (a) and (b) of Table 2 show the earnings regressions in the core and periphery, respectively. Despite the differences between our analyses, my estimates are very similar to those reported by Beck et al. (p. 713). Column (c) reports estimated slopes in a pooled equation, where intercepts differ in the core and periphery, but common (homogeneous) slopes are estimated. This equation is not reported by Beck et al., but it is reassuring that the coefficients of male age, occupational prestige, union membership, and work stability do resemble the common slopes for those variables reported by Beck et al.

I also estimated a single pooled equation in which each sector has unique slopes and intercepts. Of course, the estimated slopes from the equation merely recapitulate the findings reported in columns (a) and (b). Column (d) shows F-statistics for the heterogeneity in

Table 2. Regression Analysis of Earnings (LNY1) by Industrial Sector in the Civilian Labor Force: LNY1 = 0 for "Zero" Earners, and Missing Earnings Placed at the Mode

		Sector-specific				
Worker Characteristic		Core (a)	Periphery (b)	Pooled Equation (c)	F-Statistic for Sectoral Interaction (d)	BHT Finding (e)
Intercept		7.433	5.183	6.671		
Sector (1 = periphery)	X_0	—	—	−.3238**		
				(.1180)		
Sex (1 = female)	X_1	−.9006**	−.1782	−.6064*	1.259	sig
		(.3724)[a]	(.5626)	(.3112)		
Race (1 = nonwhite)	X_2	−.4696**	.3228	−.2127	4.831**	sig
		(.2047)	(.3188)	(.1737)		
Age of males	X_3	.0192**	.0183*	.0192**	.008	nonsig
		(.0058)	(.0102)	(.0051)		
Age of females	X_4	.0187**	−.0015	.0108*	2.761*	sig
		(.0075)	(.0101)	(.0060)		
Years of schooling	X_5	−.0606	.1538**	.0230	6.625**	sig
		(.0452)	(.0663)	(.0374)		
Highest degree						
high school or	X_6	.4513**	.2810	.3830**	.124	sig
junior college		(.2099)	(.3175)	(.1765)		
Bachelor's degree	X_7	1.0106**	−.7354	.4871	5.645**	sig
		(.3664)	(.6232)	(.3161)		
Postgraduate degree	X_8	1.4160**	1.2456	.9015**	.001	sig
		(.4753)	(1.3147)	(.4222)		
Occupational prestige	X_9	.0207**	.0253**	.0205**	.065	nonsig
		(.0055)	(.0099)	(.0048)		
Union member (1 = yes)	X_{10}	.5310**	.7440**	.5636**	.466	nonsig
		(.1348)	(.2927)	(.1260)		
Unemployed (1 = yes)	X_{11}	−.6715**	−2.0206**	−1.0836**	8.874**	sig
		(.2389)	(.4216)	(.2122)		
Work stability	X_{12}	.3855**	.5749**	.4619**	.636	nonsig
(1 = stable)		(.1347)	(.2321)	(.1187)		
Years parental	X_{13}	−.0172	−.0816**	−.0369**	3.444*	sig
schooling		(.0173)	(.0314)	(.0155)		
Standard error of estimate		1.939	2.337	2.094	2.079	
R^2		.160	.185	.174	.192	
N[b]		1116	555	1672	1672	

[a] Approximate standard errors of coefficients in parentheses.

[b] These are minimum pairwise-present counts, so those for the sectors need not add to that for the total sample.

* Significant at the 0.10 level.

** Significant at the 0.05 level.

slope of each variable in the pooled equation with unique slopes. Also, the base of column (d) reports summary measures of fit of the equation with unique slopes.

While I think it fair and accurate to say that the results in columns (a), (b), and (c) reproduce the major features of the analysis by Beck et al. (1978), there are significant departures from their findings about sectoral interaction and about the fit of the regression equation. (I am discussing my effort to reproduce their findings, not the effect of including misclassified nonearners in the regression analysis.) Column (e) of Table 2 reports the findings of Beck et al. with respect to sectoral interaction in their semilogarithmic earnings function. Beck et al. say that they reject the null hypothesis of homogeneity when interactions are significant at or beyond the $p = .10$ level (pp. 712–3), assuming the GSS is as efficient as a simple random sample (NORC, 1977:157). My analysis follows the same generous rule, but in three of the nine cases where Beck et al. report sectoral interactions (sex, high school, and postgraduate degree) my test statistics fall well below their critical values. These discrepancies also appear when I construct a t-statistic from the point estimates and standard errors of the slopes in equations (a) and (b). I have been unable to resolve them.[3] Incidentally, Beck et al. (p. 713) report $R^2 = .2962$ in an equation containing only the significant sectoral interaction effects, while I find $R^2 = .192$ in an equation containing all sectoral interaction effects. Beck has informed me that $R^2 = .2008$ in his analysis; the larger figure was obtained by correlating observed dollar earnings with the antilog of predicted log earnings.

One other anomaly in the findings reported by Beck et al. (1978:712–3) is that their F-ratios for the Chow test, purporting to be a global test for 14 possible sectoral differences in slopes, also include the effects of differences in intercepts between sectors. Thus, while there are only 14 independent slopes in each of the sectoral regressions, Beck et al. (p. 713) report 15 degrees of freedom for the numerator of each of the F-ratios. Beck et al. (p. 714) wrongly interpret the results of these tests as if they reflected only sectoral interaction effects.

Table 3 shows a regression analysis of the log of GSS earnings from which the 99 observations assigned zero values have been dropped. In every other respect the analysis is unchanged, but the results differ dramatically from those reported earlier in Table 2 and from those reported by Beck et al. (p. 713).

First, the fit is vastly improved. As shown by the standard errors of estimate and coefficients of determination, the predictive value of the simple earnings function is substantially greater in the core, in the periphery, and in the total sample. For example, the pooled equation with interaction effects accounts for 19.2% of the variance in LNY1 and for 38.6% of the variance in LNY2; the unexplained variance of earnings is more than seven times larger in the case of LNY1 than in that of LNY2 ($2.079^2/.776^2 = 7.2$).

Second, when the outliers are dropped, there remain only two statistically significant interaction effects. The (negative) effect of parental schooling is significantly larger in the periphery than in the core. While this negative effect is statistically significant in the periphery and in the total sample, the sign of the coefficient makes no theoretical sense. I am inclined to regard that finding as evidence of measurement error—possibly the treatment of missing data—in parental schooling, or as evidence of misspecification elsewhere in the earnings function. Beck et al. (1978) also have little to say about a parallel finding in their analysis.

More interestingly, the estimated effect of occupational prestige is more than twice as large in the periphery as in the core. This is the only interaction effect in my Table 3 which may be worth interpreting, and it was not detected in the analysis by Beck et al. (1978:713, 715) nor in that of my Table 2. Both in the core and in the periphery the effects of prestige on earnings are substantial. Earnings increase by 1.1% for each unit of prestige (on the NORC scale) in the core, and they increase by 2.5% for each unit of prestige in the periphery. These different returns may be a result of nonlinearity rather than of heterogeneity, for jobs in the core and in the periphery cover different ranges of the prestige scale. Mean prestige is 42.3 in the core and 33.4 in the periphery; the two means are nearly a standard deviation apart. I have not investigated this possibility, that the sectoral interaction effect of prestige is an artifact of nonlinearity.

In the theoretical context developed by Beck et al. (1978), I think it may be difficult to explain why the effects of prestige are twice as large in the periphery as in the core. For example, Beck et al. write (p. 707):

> Theories of dual economy suggest that these sectoral differences have important implications for the opportunity structures and experiences faced by individual workers. In the core sector, workers move within job structures characterized by differentiated task and wage schedules with often well-defined career patterns, i.e., internal labor markets (Doeringer and Piore, 1971; Spilerman, 1977). Formal education is widely used to mediate

---

[3] Beck has not responded to my inquiries about these discrepancies.

Table 3. Regression Analysis of Earnings (LNY2) by Industrial Sector in the Civilian Labor Force: Data for "Zero" Earners Excluded and Missing Earnings Placed at the Mode

		Sector-specific			F-Statistic for	
Worker Characteristic	Core (a)	Periphery (b)	Equation (c)	Pooled Interaction (d)	Sectoral Finding (e)	BHT
Intercept		7.844	7.316	7.716		
Sector (1 = periphery)	X_0			−.1889**		
				(.0453)		
Sex (1 = female)	X_1	−.5241**	−.6847**	−.5902**	.363	sig
		(.1436)[a]	(.2155)	(.1195)		
Race (1 = nonwhite)	X_2	−.0037	.0786	.0110	.332	sig
		(.0790)	(.1221)	(.0667)		
Age of males	X_3	.0099**	.0071*	.0094**	.672	nonsig
		(.0022)	(.0039)	(.0020)		
Age of females	X_4	.0067**	.0061	.0069**	.079	sig
		(.0029)	(.0039)	(.0023)		
Years of schooling	X_5	.0028	.0435	.0157	.010	sig
		(.0174)	(.0254)	(.0144)		
Highest degree						
high school or	X_6	.2708**	.1989	.2302**	.128	sig
junior college		(.0810)	(.1216)	(.0678)		
Bachelor's degree	X_7	.4511**	.1276	.3344**	.038	sig
		(.1413)	(.2387)	(.1214)		
Postgraduate degree	X_8	.6490**	1.0978**	.5139**	2.281	sig
		(.1833)	(.5035)	(.1621)		
Occupational prestige	X_9	.0113**	.0247**	.0158**	11.503**	nonsig
		(.0021)	(.0038)	(.0019)		
Union member (1 = yes)	X_{10}	.3292**	.3600**	.3169**	.014	nonsig
		(.0520)	(.1121)	(.0484)		
Unemployed (1 = yes)	X_{11}	−.2208**	−.1009	−.1588*	.608	sig
		(.0921)	(.1615)	(.0815)		
Work stability	X_{12}	.2909**	.3170**	.2915**	.082	nonsig
(1 = stable)		(.0519)	(.0889)	(.0456)		
Years parental	X_{13}	−.0010	−.0484**	−.0150**	11.653**	sig
schooling		(.0067)	(.0120)	(.0059)		
Standard error of estimate		.731	.858	.781	.776	
R^2		.329	.375	.374	.386	
N[b]		1067	510	1577	1577	

[a] Approximate standard errors of coefficients in parentheses.

[b] These are minimum pairwise-present counts, so those for the sectors need not add to that for the total sample.

* Significant at the 0.10 level.

** Significant at the 0.05 level.

> individual access to job ladders, and workers' wages "are largely determined by their respective access to different job clusters, by the relatively rigid pattern of wages attached to the job structures through which they respectively move, and by the speed with which they pass through those structures" (Gordon, 1972:50). In the peripheral sector, occupational opportunity structure is more restricted [sic] with a consequent dampening of task and wage variations. Gordon (1972:51) suggests that in this sector, "variation in individual hourly wages will depend very little on variations in individual 'capacities' like aptitude, reasoning and vocational skill."

Whatever the theoretical ramifications of the differential effects of prestige, I think it would be premature to draw global conclusions about heterogeneity in labor markets from my finding that occupational prestige affects earnings more in the periphery than in the core.

Again, aside from the effects of occupational prestige and of parental schooling, the analysis in my Table 3 detects no statistically significant interaction effects. This may be contrasted with nine sectoral interactions reported by Beck et al. (of which I was able to confirm only six). Some of the noneffects are worth mentioning. Beck et al. (p. 715) suggest that "earnings return to education in the core sector rests on the acquisition of a formal degree, whereas in the peripheral sector economic benefits are derived from additional years of schooling, not from increases in formal levels of certification." In the analysis of my Table 3, the linear term in schooling is not significant in core or periphery, and the effects of levels of certification do not differ across sectors. Beck et al. (pp. 717–8) also write:

> We further contend that the sectoral model employed here constitutes an essential element in understanding the process of discrimination against minority groups in that it includes not only individual factors but also the organization of the economic structure. Specifically, despite the differential representation of females and nonwhites in the peripheral sector, there is no statistically significant evidence of earnings discrimination in that sector after controlling on the predetermining variables. In the core sector, however, there is evidence of significant adverse race and sex main effects on earnings even after controlling on human capital and occupational variables.

In the analysis of my Table 3, women suffer a very large economic disadvantage, which is not significantly different in the core and in the periphery. Furthermore, with the other variables controlled, nonwhites have no significant advantage or disadvantage in the core or in the periphery, nor do the estimates of the effect of race differ significantly between the core and the periphery.

Not only are most of the revised estimates in my Table 3 similar in the two economic sectors, but they are very different from those estimated in my Table 2 and, similarly, from those estimated by Beck et al. (1978:713). I offer a few illustrations constructed by taking antilogs of coefficients from the revised equation with common slopes (col. [c] of my Table 3) and from the unique sectoral equations estimated by Beck et al. (p. 713). Beck et al. estimate that women make 44% ($e^{-.8232} \times 100$) as much as men in the core and 87% as much in the periphery; in Table 3 I find that in both sectors women make 55% of the earnings of men. Beck et al. estimate that nonwhites make 63% as much as whites in the core and 38% more than whites in the periphery. In Table 3 I find no differences between the earnings of whites and nonwhites—a seemingly anomalous finding which may be a result of the inclusion of both men and women in the analysis without a measure of labor supply. Beck et al. estimate that the currently unemployed made 50% as much in the previous year as the currently employed in the core, and the currently unemployed made 14% as much in the previous year as the currently employed in the periphery. In Table 3 I find that the currently unemployed made 85% as much in the previous year as the currently employed. My reading of these comparisons is that the estimates based on Table 3 are reasonably consistent with other social scientific findings, but those of Beck et al. are not.

Conclusion

Beck et al.'s analysis of sectoral differences in earnings, based upon data from the 1975 and 1976 NORC General Social Surveys, is fatally flawed. Because of an error in sample selection, many job changers and persons out of the labor force were treated as if they worked but had no earnings. This error was compounded by using a logarithmic transformation of earnings, which gives a great deal of importance to observations at or near the zero point of the transformed variable. Because of this error, their analyses of earnings (but not of labor force composition) are wrong in important respects. In this reanalysis of the GSS data I have emphasized the effect of the error upon Beck et al.'s major conclusion, that the earnings process is heterogeneous across industrial sectors. When the error is corrected, most of the evidence of heterogeneity disappears.

To be sure, there remain differentials in earnings between core and periphery sectors. In the corrected equation of my Table 3 (col. [c]), where common slopes are estimated in the two industrial sectors, I estimate that workers in the periphery make 83% as much as workers in the core; the differential is $1,455 when evaluated at the mean of the earnings distribution in the core, and it is $1,033 when evaluated at the mean of the earnings distribution in the periphery. Any resemblance between these dollar figures and the estimates of Beck et al. (1978:713, Table 4) is purely coincidental. The equations used by Beck et al. to standardize earnings between sectors vastly overestimate the sectoral earnings differential in relative terms, but this error is largely compensated by erroneously low estimates of average earnings in both sectors.

Another respect in which the analysis of Beck et al. (1978) is roughly correct is the share of the total sectoral earnings differential which is explained by the variables included in their regression equation. In my revised estimates the difference in the log of earnings between periphery and core is −.531; evaluated at the means in the two sectors, the difference is $3,482. Since the coefficient of sector in the pooled equation of my Table 3 is −.1889, differences in social composition between sectors explain about two-thirds of the initial difference in earnings.

These points of agreement are conditional upon the specification of the earnings functions in my Table 3. While I believe they do not share the flaws of the equations estimated by Beck et al. (1978), there remain several specification errors in those equations. In particular, I think that the absence of a proximate measure of labor supply (the amount of time at work in the previous year) explains part of the remaining sectoral difference in earnings.

I doubt that any commentary can counter the impact of the broad and sweeping conclusion of Beck et al. (1978:717):

> In face of strong empirical evidence contrary to the implicit tenet of labor market homogeneity, parameter estimates which are conditional on this assumption can no longer be treated as appropriate bases for the construction of sociological theory or social policy.

It is most regrettable that their proposition finds so little empirical support.

Robert M. Hauser
University of Wisconsin–Madison

REFERENCES

Beck, E. M., Patrick M. Horan, and Charles M. Tolbert II
1978 "Stratification in a dual economy: a sectoral model of earnings determination." American Sociological Review 43:704–20.

Cain, Glen G.
1976 "The challenge of segmented labor market theories to orthodox theory." Journal of Economic Literature 14:1215–57.

Duncan, Otis Dudley, David L. Featherman, and Beverly Duncan
1972 Socioeconomic Background and Achievement. New York: Seminar.

Featherman, David L., and Robert M. Hauser
1978 Opportunity and Change. New York: Academic.

Goldberger, Arthur S.
1964 Econometric Theory. New York: Wiley.

Hauser, Robert M., and David L. Featherman
1977 The Process of Stratification: Trends and Analyses. New York: Academic.

Heckman, James, and Solomon Polachek
1974 "Empirical evidence on the functional form of the earnings-schooling relationship." Journal of the American Statistical Association 69:350–4.

Jencks, Christopher, Susan Bartlett, Mary Corcoran, James Crouse, David Eaglesfield, Gregory Jackson, Kent McClelland, Peter Mueser, Michael Olneck, Joseph Schwartz, Sherry Ward, and Jill Williams
1979 Who Gets Ahead? The Determinants of Economic Success in America. New York: Basic.

Mincer, Jacob
1974 Schooling, Experience, and Earnings. New York: National Bureau of Economic Research and Columbia University Press.

Mosteller, Frederick, and John W. Tukey
1977 Data Analysis and Regression: A Second Course in Statistics. Reading, Mass.: Addison-Wesley.

National Opinion Research Center (NORC)
1977 National Data Program for the Social Sciences: Cumulative Codebook for the 1972–1977 General Social Surveys. Chicago: National Opinion Research Center.

Reiss, Albert J., Jr.
1961 Occupations and Social Status. New York: Free Press of Glencoe.

Sewell, William H., and Robert M. Hauser
1975 Education, Occupation and Earnings: Achievement in the Early Career. New York: Academic.

Tobin, James
1958 "Estimation of relationships for limited dependent variables." Econometrics 26:24–36.

Tolbert, Charles, Patrick M. Horan, and E. M. Beck
1980 "The structure of economic segmentation: a dual economy approach." American Journal of Sociology 85:1095–116.

SOCIAL STRATIFICATION IN INDUSTRIAL SOCIETY: FURTHER EVIDENCE FOR A STRUCTURAL ALTERNATIVE*

(REPLY TO HAUSER)

In 1950 two young researchers characterized the dominant community stratification research tradition as follows:

> There are numerous indications that the principal motivation of many crucial points of the analysis is more a matter of the imposition of a set of categories and the manipulation of a technique than the understanding of what is at stake—the social structure. This point is evidenced by a general tendency to regard any anomalies encountered as being problematic only in a methodological light, rather than from the standpoint of the theory of class structure. There is often a failure to consider the meaning of negative instances, or to consider alternative hypotheses. (Pfautz and Duncan, 1950:211)

Now, some thirty years later, Professor Hauser's defense of the dominant status attainment research tradition founded by Duncan completes the circle.

In attempting to counter our theoretical and empirical criticisms of the status attainment tradition, Hauser reinterprets our work from within that neoclassical theoretical perspective which status attainment presumes. From that vantage, Hauser sees no basic conceptual or theoretical issues, on y methodological errors and empirical anomalies. In our response we will examine both the conceptual foundations of Hauser's assertions and their empirical validity.

---

* Direct all communications to: E. M. Beck; Department of Sociology; University of Georgia; Athens, GA 30602.

Because confusion between conceptual and methodological issues permeates Hauser's comment, we must give some initial attention to the rather different theoretical foundations for his status attainment research and for our research on industrial segmentation. Status attainment rests on a neoclassical theoretical perspective (Horan, 1978) in which the socioeconomic structure of industrial society derives from rational interactions of individual workers and individual employers within a competitive market setting. Social stratification is represented as an individual "attainment" process and that process is analyzed in terms of the socioeconomic returns to individual characteristics, such as family background and schooling. Hauser defends this tradition by acknowledging heterogeneity in status attainment processes by race, gender, and age. Such an acknowledgement misses the mark. The important issue is how such differences are conceptualized and interpreted. What is the theory of stratification in industrial society which would lead one to expect such differences? Our position is not only that labor market heterogeneity exists, but that it derives from the organization of production in industrial capitalism. It is this conceptual grounding which distinguishes industrial segmentation theory from simple demographic observation by providing an alternative interpretative scheme for understanding the operation of the stratification system.

Our research derives from a very different conception of the social and economic order than that which underlies status attainment. From our perspective, the placement of individuals within the social order cannot be understood solely in terms of the personal characteristics of the individuals themselves. It is the organizational structure of industrial capitalism which shapes the American stratification system, not the psychological dispositions or individual predilections of workers. Accordingly, the fundamental issue in social stratification is not individual "attainment," but the arrangement of distinct social positions within the social and economic organization of production, and the impact such arrangements have on the experiences of individuals.

These different perspectives can lead to divergent interpretations of the same empirical findings. When faced with evidence that individual attainment processes are different for blacks and females, status attainment researchers acknowledge these differences and proceed with a research tradition which concentrates primarily on white males. When faced with the existence of low earnings, poverty, and underemployment, status attainment researchers tend to characterize these as representing individual "outliers" and to exclude them from analysis. From our view, the differential experiences of females and blacks, and the existence of poverty and underemployment are not anomalies but regularities which can be better understood through a focus on the social and economic structure of industrial capitalism.

Given the fundamental intellectual differences between the status attainment perspective and our own, we find Hauser's systematic misapprehension of our work to be predictable. In our response to his specific criticisms we will focus on two separate themes: (1) What are the theoretical implications of Hauser's "methodological" criticisms? (2) Do the data manipulations proposed by Hauser eliminate the existence of substantial sectoral differences in individual-level earnings determination processes?

The NORC Income Question

Hauser identifies an important issue concerning the use of the NORC General Social Survey for analysis of earnings determination models; namely, the income question is keyed to "normal" occupation ("What kind of work do you [did you] normally do?") and thus may not accurately reflect earnings. Specifically, individuals classified as nonearners by the GSS and treated as such in our analysis may in fact have earned income in some other occupation. Hauser "think[s] it probable" that respondents who report no earnings should not be treated as nonearners. We think it probable that our treatment of these individuals as nonearners was accurate. However, as Hauser (1980:705) notes, "the GSS data lack the information required to enumerate persons properly and improperly classified as nonearners." Hauser proposes to resolve the issue by deleting the nearly 6% of the sample who fell into the nonearner category.[1] This is a conceptually and empirically inappropriate truncation of the sample which renders his subsequent analysis meaningless.

It should be clear that this dispute about the classification of nonearners cannot be settled through further manipulations of the NORC data, since any such manipulation must impose some prior solution to the classification issue. In order to evaluate the empirical validity of Hauser's criticisms, it is necessary to employ a national data set which does not have the problems associated with the NORC income questions: the 1976 Current Population Survey (March Supplement). From this CPS we selected all workers who were members of the recently experienced civilian labor force in 1975.[2] This produced a sample of over 62,000 respondents, representing almost 100.3 million workers. We will use these data to evaluate each of Hauser's claims about "errors" in our analysis, errors which Hauser claims are responsible for our findings of significant differences in the earnings determination process between core and periphery economic sectors.

---

[1] Although Hauser claims his study is a replication, this is not accurate. He redefines the sample and deletes one of the variables included in our original analysis, all of which results in the loss of a substantial number of degrees of freedom. We do not know to what degree his conclusions were affected by these departures.

In order to evaluate these claims we estimate, under a variety of conditions, an augmented Mincer-type human capital function by industrial sector. This human capital function consists of linear and quadratic terms for years of schooling and years of work experience, a schooling/work experience interaction term, and an occupational prestige variable.[3] To determine the robustness of sector differences we computed a Chow test for sectoral interactions under each of the conditions proposed by Hauser. The results of these analyses, presented in Tables 1 and 2, demonstrate that none of the "methodological" issues proposed by Hauser eliminate the sectoral differences in earnings determination processes which we noted in our original paper.

In discussion below we consider each of Hauser's specific criticisms, focusing first on the theoretical implications of the criticism and second on its empirical validity.

Labor Supply vs. Labor Demand.[4] Hauser offers that the earnings equation should contain annual hours worked as a labor supply variable and that its omission is a serious flaw. While such a specification *may* be reasonable from a human capital perspective, it represents a serious conceptual misspecification from our perspective. Specifically, there is no reason to assume that annual hours worked reflects labor supply as opposed to labor demand factors. If each individual labor force participant elects how many hours to work (that is, if time worked is a consequence of individual decision making), then a control for this as a supply factor might be appropriate. However, if the number of hours worked is largely determined by factors beyond the individual worker—determined by the same elements of the social and economic organization that structure socioeconomic rewards—then, time worked is not a labor supply factor but a characteristic of labor which is endogenous to the organization of production.

Furthermore, the control on time worked which Hauser advocates would change the focus of analysis from earnings determination to wage determination.[5] That is, since earnings is the product of a wage-rate factor and a time-worked component, the elimination of differences in time worked leaves only one source of variation in earnings: wage rates.[6] Earnings determination models which include time worked as a control variable, therefore, amount to implicit wage determination models. While such a shift from earnings to wage determination is consistent with the neoclassical human capital theory of Becker and Mincer, it is not the correct specification for the theory we are employing.

Not only does Hauser's labor supply approach do violence to segmentation theory, but it leads to the blind dismissal of the effects of unemployment and underemployment on socioeconomic outcomes. By concentrating on models of wage determination, or restricting models of earnings determination to full-time year-round workers, much research in the status attainment tradition has naively ignored the very real problems of underemployment, especially for minorities, and the critical adverse effects these have on socioeconomic rewards.[7] While it is possible to conceptualize

---

[2] The recently experienced civilian labor force is defined as workers who in 1975 were civilians 16 years old or older and were employed, either full time or part time, or were a member of the recently experienced unemployed. The recently experienced unemployed are those who did not work in 1975 but who sought employment and who had been employed for two consecutive weeks or more during the previous four years.

[3] A complete discussion of the variables, as well as a justification of the functional form, is detailed in Beck et al. (1980).

[4] For a more elaborate discussion of these issues, see Beck (1980).

[5] Blinder (1976) makes an important point by noting that such a transition to a wage determination model implies theoretical interest in potential earnings rather than actual earnings. Our interest was in the latter.

[6] One strategy which we are exploring is to decompose log earnings into its additive elements, a log wage-rate component and a log time-worked component, and then treat these as jointly dependent endogenous variables in a simultaneous equations system. This approach permits an assessment of the effects of exogenous factors on both components of annual earnings and avoids the limited scope of pure wage determination models.

[7] For a discussion of these issues see Fusfeld (1973), Gordon (1972), Harrison (1972), Hiestand

Table 1. F-Tests for Segmented Market Model[1]

Labor Income	White Males	White Females	Nonwhite Males	Nonwhite Females
Unadjusted for Labor Force Participation				
Dollar Earnings	187.76*	153.52	29.23	20.46
Ln Earnings	129.33	108.20	10.27	7.08
Adjusted for Labor Force Participation				
Dollar Earnings	142.52	89.99	19.62	15.57
Ln Earnings	127.36	85.05	9.89	4.56

[1] Labor incomes were set equal to zero if less than zero. For the semilog regressions, zero labor incomes were incremented by one dollar so that Ln earnings was defined as equal to zero.

* All F-ratios significant beyond the 0.001 level.

these employment issues as questions of "labor supply," such a view is deficient because it ignores organizational features of industrial capitalism which determine and shape the demand for labor.

In later work, we made a concession to the labor supply critics by adjusting annual earnings proportionally for the amount of labor force participation.[8] Following that convention, we adjusted the earnings in our CPS sample to eliminate differences in labor force participation, yet as the F-ratios reported in Table 1 demonstrate, regardless of whether earnings are adjusted or left unadjusted, there are significant sectoral interactions.[9] This is evidence that the sectoral effects are not reflections of simple differences in labor supply.

Choice of functional form. Hauser criticizes our use of a semilogarithmic earnings function arguing that, although there are good reasons for employing such a function, our analysis is flawed by failure to endorse his preferred justification. While Hauser chooses a statistical justification for selecting the semilogarithmic function, we prefer a justification resting on conceptual grounds.[10] Industrial segmentation theory highlights the critical role of the organization of production in shaping the demand for labor and the effects which these demand factors have on individual labor market outcomes. Becker (1964:53) argues that relative differences in earnings are more important where labor demand is the orientation while absolute differences in dollar earnings are appropriate for studies of labor supply.[11] Given the demand orientation of segmentation theory, our use of a semilogarithmic function is conceptually appropriate. Further, Mincer (1974:18) suggests that studies of income inequality should concentrate on relative differences among workers rather than worker differences in absolute dollars. This argument can be based on the declining marginal utility of labor income. If the marginal utility of income diminishes as income increases, then income transfers are relatively more important among low income workers than among work-

---

(1970), Killingsworth (1968), Levine (1972), Tsuchigane and Dodge (1974), and Sullivan (1978) as well as various papers in Gordon (1965), Wallace and LaMond (1977), and Wiesbrod (1965). See also Robert Solow's Presidential address to the American Economic Association (Solow, 1980).

[8] The adjustment involves estimating the mean earnings per week in the labor force during 1975, then multiplying the result by 52 (see Beck et al., 1980). Even this adjustment is not totally satisfactory, however, since it ignores problems of the "discouraged" worker and other forms of hidden underemployment (see Rees, 1979:10–14).

[9] All F-ratios are significant beyond the 0.001 level and were computed on the basis of the number of unweighted cases. It should be noted that the Chow tests reported in Tables 1 and 2 reflect sectoral differences in intercepts as well as sectoral differences in slopes. In a private communication, Hauser has argued that the F-ratios should be constructed to contain only slope differences. We believe this is incorrect and would lead to misleading and inconclusive results. Given the limited range of variables for such earnings models in national data sets, it is imperative to include intercept differences as measures of the sectoral affects of variables not included in the model. If variables not explicitly included in the equation have significantly different sectoral effects, their collective effects will be reflected in sectoral differences in the intercepts. Thus intercept differences as well as slope differences reflect heterogeneity in earnings determination between industrial sectors, and it would be inappropriate to exclude the intercepts from F-ratios. We note, however, that of the 32 contrasts presented in Tables 1 and 2, in only 7 instances, all involving nonwhites, were there no significant differences between slopes at the 0.05 level.

[10] Since there is ample conceptual justification for our specification, Hauser's invocation of the principle of parsimony, with its implicit ceteris paribus clause, is inappropriate and misdirected.

[11] Becker's point was cited by Featherman and Hauser (1978:288).

ers of high income.[12] From this social welfare perspective, therefore, we are guided to consider the effects of social forces on the relative differences among workers rather than absolute differences.[13] It follows that the appropriate dependent variable be log earnings, not dollar earnings.

Hauser notes correctly that the choice of an earnings function will affect the empirical findings from analyses of the earnings determination process. Specifically, sectoral homogeneity of effects in an analysis of log earnings does require sectoral heterogeneity of dollar earnings, and vice versa. However, heterogeneity in one does not imply homogeneity in the other. For reasons outlined above we chose to focus on heterogeneity in rates of return although the literature on industrial segmentation is not sufficiently well developed to specify unambiguously whether heterogeneity should be expected in dollar returns, in rates of returns, or in both.[14]

In the CPS regressions we estimated both the dollar returns and the rates of return by industrial sector and as the F-ratios in Table 1 show, there are significant sectoral interactions in both.[15] These results reinforce the basic market heterogeneity tenet of industrial segmentation theory.

Race and gender interaction. Hauser further suggests that our failure to include race and gender interactions is "curious" and may be responsible for our findings of sectoral differences in earnings determination processes. We find these assertions curious on a number of counts. (1) Hauser must be aware that the small size of the GSS data set would not support inclusion of race and sex interactions. (2) Hauser ignores our analysis in that same article which demonstrates the existence of sector differences in earnings determination among white males. (3) Hauser likewise ignores our later analysis where we addressed directly the issue of sectoral differences in earnings processes by race and gender (Beck et al., 1980).[16]

Although this issue is explored in more detail in this later paper, in the present analysis we estimated the human capital function separately by race and gender using the CPS data. The race- and gender-specific F-ratios presented in Table 1 demonstrate that there are significant sectoral interactions within each group. Thus the sectoral interactions cannot be dismissed as being an artifact of excluding race and gender interactions.

Truncating labor income. Hauser suggests that we should not have included zero earners in the earnings equation, and that their inclusion produces biased results. However, we argue that it is his exclusion of low earners which is the major source of bias. The actual distribution of labor income in the population not only contains zero earners but also a non-negligible negative tail. Many surveys, such as the GSS, ignore this negative tail and lump negative earners and zero earners into the lowest income category thus producing a censored sample distribution of labor income. Hauser's procedure of deleting zero earners (and presumably negative earners as well) is particularly ill-advised because it further distorts an already censored sample distribution by truncating its lower tail, further attentuating its dispersion and biasing upward its central tendency.[17]

---

[12] Hauser errs in suggesting that the log transformation gives undue weight to low income workers. How much weight to be given observations of differing income is a normative question which assumes some underlying specification of a social welfare function. See Allison (1978), Atkinson (1970), and Sen (1973) for discussion of this point.

[13] For a somewhat different conceptual development, see Rainwater (1974) and Coleman et al. (1978).

[14] In most of our work we have concentrated on sectoral interactions in rates of return (Beck et al., 1978; Horan et al., 1980). In Tolbert et al. (1980) a model for sectoral interactions in dollar returns was presented for purposes of a simple illustration of sectoral measurement. This does not represent the intellectual inconsistency Hauser implies.

[15] It could be argued, however, that these effects are due to the limited nature of the human capital function. In work not presented here we utilized a much more complex function (very similar to that employed by Featherman and Hauser [1978:293]) and the OCGII data to test for sectoral interactions. This analysis demonstrates that there are significant sectoral interactions in both dollar returns and rates of return even with this more complex specification. See Horan et al. (1980) for a report on a subset of these additional analyses using the OCGII data.

[16] Hauser was provided a copy of this paper at his request.

[17] If the point of interest is estimating dollar returns, and if the data have not been censored by permitting only positive values of earnings, then no particular problem exists. If, however, interest centers on estimating the rates of return an immediate difficulty is encountered: the natural log of income is undefined for zero and negative earners. There are three possible solutions to this dilemma: (1) follow Hauser's advice and truncate the distribution by deleting the offending observations; (2) censor the distribution by assigning negative and zero earners the value of one dollar so that the log of earnings is zero; or (3) recast the issue in terms of a three parameter Lognormal distribution where the displacement parameter is equal to the largest negative income (Aitchison and Brown, 1969:14–16). The first alternative has only methodological simplicity to recommend it, the second strategy is the one we have followed in past research, and the third tactic is one we are currently exploring.

Table 2. F-Tests for Segmented Market Model: Truncated Earnings[1]

	White		Nonwhite	
Labor Income	Males	Females	Males	Females
Unadjusted for Labor Force Participation				
Dollar Earnings	152.31*	96.55	22.80	16.71
Ln Earnings	170.33	145.66	19.68	16.53
Adjusted for Labor Force Participation				
Dollar Earnings	121.27	59.19	15.87	14.14
Ln Earnings	216.74	173.23	24.55	15.52

[1] Regressions computed only for workers with adjusted or unadjusted earnings greater than $1,000.
* All F-ratios significant beyond the 0.001 level.

It is interesting to note that several years ago Glen Cain (1976) argued that much of the segmentation research was methodologically questionable because empirical observations were selected on the basis of dependent variable:

> However, these tests [of market segmentation] almost invariably suffer from a methodological flaw—that of fitting the regression to a sample that is truncated on the values of the dependent variable—with the result that the estimated coefficients (effects) of the independent variable are biased. (Cain, 1976:1246)

Cain's criticism would seem to apply as well to Hauser's exclusion of low earners. Moreover, we are not inclined to adopt Hauser's unique view that lower tail of the distribution of labor income constitutes an anomaly in the stratification system of industrial capitalism. It is the exclusion of the lower tail of the income distribution which constitutes bias, not its inclusion.

Turning to the empirical question, Hauser argues that sectoral interactions are due to the inclusion of these zero earners and that when these "anomalies" are removed, the interactions disappear along with them. While it is contestable whether such workers constitute "anomalies," we explored Hauser's empirical hypothesis by replicating the regressions in Table 1 with the additional restriction that workers with labor incomes less than $1,001 were deleted from the analysis.[18] The results of the Chow tests for these truncated regressions are presented in Table 2. Again we find that regardless of race or gender, regardless of whether income is adjusted or unadjusted, and regardless of whether attention is directed toward dollar returns or rates of return, all F-ratios are statistically significant beyond the 0.001 level. This further demonstrates that even if it were conceptually reasonable to truncate the distribution of labor income, there are still significant sectoral interactions which cannot be attributed to the inclusion of low income earners.

Conclusions

In the most general terms our research examined the impact of placement within the industrial order on the earnings determination process. Beginning with the conventional neoclassical conception of the earnings process as an exchange of worker resources for economic rewards, we demonstrated that the distinction between oligopolistic and competitive modes of industrial organization serves to condition this exchange in a manner which is both substantively and statistically significant. Contrary to Hauser's assertion that there is "no substantial theoretical rationale" for such an inquiry, both our research design and empirical findings are subject to straightforward theoretical derivation and interpretation from the industrial segmentation literature in general and from the dual economy perspective in particular. Briefly, that interpretation holds that differential location of employers along the oligopoly/competitive dimension of industrial organization leads to very different strategies for the structuring of production, and the management, control, and evaluation of the workforce. (See Edwards, 1979 for a clear explanation of these issues.) One consequence of this differential organization of production is a pattern of worker experiences which varies according to placement within the industrial order. And this is precisely what the empirical research reported here and elsewhere has demonstrated: there are theoretically interpretable relationships between divisions within the industrial economy and individual labor market outcomes.

Using data unfettered by the limitations of the GSS, we have shown that the market het-

---

[18] We truncated the distribution at $1,000 for two reasons: (1) it provides a stringent test of the sectoral hypothesis, and (2) it is consistent with the procedure Hauser has followed in past research (Featherman and Hauser, 1978).

erogeneity hypothesis of segmentation theory continues to have strong empirical support throughout the range of statistical manipulations suggested by Hauser. Regardless of controls for race and gender, of whether or not income is adjusted for labor force participation, of whether effects are estimated in dollar returns or rates of return, and of whether low earners are included or excluded, basic sectoral differences in earnings processes remain. Yet we have absolutely no doubt that if one is sufficiently diligent, it would be possible to redefine the population, or the problem for that matter, so as to eliminate sectoral interactions in the earnings determination process. In the absence of an explicit theoretical model, such findings would be neither surprising nor even particularly interesting, except perhaps as a methodological footnote.

We do not suggest that our work on organizational constraints on earnings determination provides a totally adequate and complete explanation of the findings we have reported nor does it represent the only viable specification of the organizational structure of industrial capitalism. Our findings of sectoral differences in earnings represent not so much a complete explanation of underlying processes, as an identification of basic empirical regularities which need to be elaborated through further systematic inquiry. Industrial segmentation theory is but one of several structuralist perspectives which could be applied here. Wright (1979), for example, used a similar research approach to demonstrate the importance of class categories in mediating earnings determination processes.

In sum, it is our view that problems such as underemployment, poverty wages, job instability, race and gender inequalities, and regional or subregional disparities in wealth do not represent mere anomalies or market imperfections in an otherwise orderly system, but, rather, reflect an integral component of the economy of industrial capitalism. Any satisfactory paradigm for stratification research must be sufficiently robust so as to account for these features without dismissing them as anomalies and outliers. In this sense, stratification research, focusing on the positions of individuals within the social and economic organization of production, provides important advantages over the neoclassical research traditions of status attainment and human capital.

E. M. Beck
Patrick Horan
University of Georgia
Charles M. Tolbert, II
Emory University

REFERENCES

Aitchison, J. and J. A. C. Brown
1969 The Lognormal Distribution. Cambridge: Cambridge University Press.

Allison, Paul D.
1978 "Measures of inequality." American Sociological Review 43:865–80.

Atkinson, Anthony B.
1970 "On the measurement of inequality." Journal of Economic Theory 2:244–63.

Beck, E. M.
1980 "Toward a better theory of earnings determination: some slighted issues in sociological approaches." Working Paper, Department of Sociology, University of Georgia.

Beck, E. M., Patrick M. Horan, and Charles M. Tolbert II
1978 "Stratification in a dual economy: a sectoral model of earnings determination." American Sociological Review 43:704–20.
1980 "Industrial segmentation and labor market discrimination." Forthcoming, Social Problems.

Becker, Gary S.
1964 Human Capital. New York: National Bureau of Economic Research.

Blinder, Alan S.
1976 "On dogmatism in human capital theory." Journal of Human Resources 11:8–22.

Cain, Glen G.
1976 "The challenge of segmented labor market theories to orthodox theory: a survey." Journal of Economic Literature 14:1215–57.

Coleman, Richard P., and Lee Rainwater with Kent A. McClelland
1978 Social Standing in America: New Dimensions of Class. New York: Basic Books.

Edwards, Richard C.
1979 Contested Terrain: The Transformation of the Workplace in the Twentieth Century. New York: Basic Books.

Featherman, David L., and Robert M. Hauser
1978 Opportunity and Change. New York: Academic Press.

Fusfield, Daniel R.
1973 The Basic Economics of the Urban Racial Crisis. New York: Holt, Rinehart and Winston.

Gordon, Margaret S. (ed.)
1965 Poverty in America. San Francisco: Chandler.

Gordon, David M.
1972 Theories of Poverty and Underemployment. Lexington: Heath.

Harrison, Bennett
1972 Education, Training, and the Urban Ghetto. Baltimore: Johns Hopkins University Press.

Hiestand, Dale L.
1970 Discrimination in Employment. Ann Arbor: Institute of Labor and Industrial Relations, University of Michigan.

Horan, Patrick M.
1978 "Is status attainment research atheoretical?" American Sociological Review 43:334–41.

Horan, Patrick M., E. M. Beck, and Charles M. Tolbert II
Forthcoming "The market homogeneity assumption: on the theoretical foundations of empirical knowledge." Social Science Quarterly.

Killingsworth, Charles C.
1968 Jobs and Income for Negroes. Ann Arbor: Institute of Labor and Industrial Relations, University of Michigan.

Levine, Marvin J.
1972 The Untapped Human Resource: The Urban Negro and Employment Equality. Morristown: General Learning Corporation.

Mincer, Jacob
1974 Schooling, Experience, and Earnings. New York: National Bureau of Economic Research.

Pfautz, Harold W. and Otis Dudley Duncan
1950 "A critical evaluation of Warner's work in community stratification." American Sociological Review 15:205–15.

Rainwater, Lee
1974 What Money Buys: Inequality and the Social Meanings of Income. New York: Basic.

Rees, Albert
1979 The Economics of Work and Pay. New York: Harper and Row.

Sen, Amartya
1973 On Economic Inequality. New York: Horton.

Solow, Robert M.
1980 "On theories of unemployment." American Economic Review 43:1–11.

Sullivan, Teresa A.
1978 Marginal Workers, Marginal Jobs: The Underutilization of American Workers. Austin: University of Texas Press.

Tolbert, Charles M., Patrick M. Horan, and E. M. Beck
1980 "The dual economy in American industrial structure: toward the specification of economic sectors." American Journal of Sociology 85:1095–116.

Tsuchigane, Robert and Norton Dodge
1974 Economic Discrimination Against Women in the United States. Lexington: Lexington Books.

Wallace, Phyllis A. and Annette M. LaMond
1977 Women, Minorities, and Employment Discrimination. Lexington: Lexington Books.

Weisbrod, Burton A. (ed.)
1965 The Economics of Poverty: An American Paradox. Englewood Cliffs: Prentice-Hall.

Wright, Erik Olin
1979 Class Structure and Income Determination. New York: Academic Press.

COMMENT ON NEWPORT ARTICLE*

(*ASR*, AUGUST, 1979)

I would like to make a comment, in the form of three questions, on the Newport article in the August, 1979, issue of the *American Sociological Review*.

I will not dispute Newport's findings. I have published an analysis of the same data elsewhere (Greeley, 1979: *Crisis in the Church*, Chicago: Thomas More Press). The interested reader can compare my conclusions with Newport's if he/she so desires.

(1) Why did Newport limit himself to the 1975–1976 General Social Survey data? The same questions were asked from 1973 to 1978 with a massive N of almost 9,000 cases. Why did not Newport use this larger data set since it would have enabled him to look much more closely at subpopulations—young people, for example, or Irish Catholics, a nonswitching ethnic group?

(2) Why did Newport ignore the moral, doctrinal, devotional, and religious organizational questions which are in the General Social Survey and which might explain religious switching? Thus, might not one switch from affiliation to no affiliation because, for example, one does not believe in life after death? Or might not one switch to the religion of one's spouse (as Newport concedes) because the spouse is more devout than even the typical member of such a spouse's denomination?

(3) Why does Newport pay no attention to the research done on religious change in the last two decades by scholars affiliated with NORC; scholars whose work led to the inclusion of the very questions he analyzed in the National Opinion Research Center (NORC) General Social Survey? The books by Caplowitz (1977, *The Religious Dropout: Apostacy Among College Graduates*, Beverly Hills: Sage); Kotre (1971, *View From the Border: A Social-Psychological Study of Current Catholicism*, Chicago: Aldine); and Greeley (1963, *Religion and Career*, Sheed and Ward: New York); and papers by Zelan (1968, "Religious Apostacy, Higher Education, and Occupational Choice," *Sociology of Education* 41:370–9, No. 4, Fall); and Greeley (1968, "Religious Changers," in Martin Marty, Andrew M. Greeley, and Stuart Rosenberg (eds.), *What We Believe*, New York: Meredith Press) are a fairly substantial body of literature. Either they are terrible and ought to be refuted or they are a contribution and ought to be acknowledged.

Andrew M. Greeley
National Opinion Research Center

---

* Direct all communications to: Andrew M. Greeley; Center for the Study of American Pluralism; National Opinion Research Center (NORC); University of Chicago; 6030 South Ellis; Chicago IL 60637.

"REPLY TO 'COMMENT ON NEWPORT ARTICLE, *ASR*, AUGUST, 1979' "*

I appreciate Professor Greeley's "Comment." Any one article is limited, of course. Professor Greeley is certainly correct in pointing out that there are additional data, variables, and studies in the literature—which I did not choose to use. Perhaps scholars who continue to investigate religious switching will want to take advantage of them.

Frank Newport
University of Missouri–St. Louis

---

* Direct all communications to: Frank Newport; Department of Sociology, Anthropology, and Social Work; University of Missouri–St. Louis; St. Louis, MO 63105.

ITEMS

■ PETER B. EVANS (Dependence, Inequality, and Growth in Less Developed Countries) is Associate Professor, Department of Sociology, Brown University. The petrochemical industry in Brazil is the subject of his current research. In 1979, his book, *Dependent Development: The Alliance of Multinational, State, and Local Capital in Brazil,* was published by Princeton University Press. MICHAEL TIMBERLAKE will be on leave from the Department of Sociology, Memphis State University (where he is Assistant Professor) in 1980–1981. He will participate in an NIMH-sponsored postdoctoral research training program in the Department of Social Relations at Johns Hopkins University. His current research continues examination of Third World countries' economic dependence and, also, of the effects of city size upon social problems.

■ RICHARD E. RATCLIFF (Capitalist Class Impact on Lending Behavior of Banks) is Associate Professor, Department of Sociology, Syracuse University. The topic, "Banks and Corporate Lending"; a study of patterns of geographic discrimination in mortgage lending ("mortgage redlining"); and research on determinants of business contributions supporting an electoral campaign to add an anti-labor union "right-to-work" amendment to the Missouri constitution are his present projects. Based on his "Banks and Corporate Lending" research, he has published an article (pages 107–32) in Maurice Zeitlin (ed.), *Classes, Class Conflict and the State* (Winthrop: 1980) as well as another (pages 115–38) in G. William Domhoff (ed.), *Power Structure Research* (Sage: 1980).

■ LALA CARR STEELMAN (Sibship Size and Birth-Order Effects on Intelligence) is a Ph.D. Candidate (and A.B.D.) in the Department of Sociology at Emory University. She is writing her dissertation on the confluence model and other explanations used to explain cognitive differences; she expects to complete, very soon, her work toward the Ph.D. JAMES A. MERCY is also a Ph.D. candidate at Emory, in the same department. He is currently analyzing time series data on police expenditures in Chicago, along with pursuing topics related to intelligence test performance.

■ RACHEL A. ROSENFELD (Race and Sex Differences in Career Dynamics) is Assistant Professor, on leave from McGill University, and Senior Study Director, National Opinion Research Center (NORC), Chicago. She continues research by: her study of careers and of the models for them; a study of farm women, funded by USDA; a study (with Chris Winship), funded by NSF, of youth unemployment and job mobility; as well as several other specific-area studies. Her basic area is stratification, with emphasis on educational choices and careers.

■ HAGEN KOO (Class and Income Inequality in Korea) is Associate Professor, Department of Sociology, Memphis State University. His ongoing research study is on Korean class structure and he is also involved in a comparative study of migration and urbanization patterns in Asia, being conducted at the East-West Center, Population Institute. He is also analyzing Asian immigration to the United States and the migrant-labor absorption in the United States labor market, from the world economic-system perspective. DOO-SEUNG HONG is Assistant Professor, Department of Sociology, Seoul National University, Korea. He has just finished his Ph.D. dissertation, "Social Stratification and Social Values in Contemporary Korea," in the Department of Sociology, University of Chicago. Social stratification, social mobility, and military sociology in developing societies are three areas of his research interest.

■ CHARLES M. JUDD (Structure of Attitude Systems in the General Public) is Assistant Professor, Department of Psychology and Social Relations, Harvard University. Methods of Social Research—particularly in applied settings—and Attitudes and Public Opinion are the two general subjects of his present research. He is the author (with R. G. Bridge and P. Moock) of *The Determinants of Educational Outcomes* (Cambridge: Ballinger, 1979) and (with D. A. Kenny) of *The Design and Analysis of Social Research in Applied Settings* (New York: Cambridge University Press), in press at this writing. MICHAEL A. MILBURN is Assistant Professor, Psychology Department, University of Massachusetts–Boston. His present research regards mass-media effects, public opinion, and health promotion. He has written a chapter in Selltiz et al. (eds.), *Research Methods in Social Relations.*

■ PHILIP E. CONVERSE (Rejoinder to Judd and Milburn) is Robert C. Angell Professor of Political Science and Sociology, as well as Program Director, Institute for Social Research—both at the University of Michigan. His current research concentrates on "Political Representation" and "Quality of Life." Those subjects were treated in his books, *Political Representation in France* (forthcoming, 1981) and, written with Campbell and Rodgers, *The Quality of American Life* (New York: Russell Sage, 1976).

■ DOUGLAS A. SMITH (Sex and Involvement in Deviance/Crime Literature) is a graduate student in the Department of Sociology at Indiana University. He is currently making an empirical examination of police-citizen encounters. Recently published articles which he has authored are "Sex and Theories of Deviance," Sociological Quarterly, Vol. 20, No. 2, and "The Myth of Social Class and Criminality" (co-authored by Charles R. Tittle and Wayne J. Villemez), *American Sociological Review,* October, 1978. CHRISTY A. VISHER is also a graduate student in Sociology at Indiana University. She is currently involved in an NIMH-funded research project to study the determinants of verdicts in jury-tried sexual assault cases in a large midwestern city. Specifically, the study explores the impact on their verdicts of the jurors' sex-role attitudes and of their attitudes toward rape.

■ BARRY GLASSNER (How Jews Avoid Alcohol Problems), Associate Professor of Sociology, Syracuse University, is continuing his research on ethnicity and alcohol abuse with a new project on Italian Americans. Other research in progress includes a study of sociological aspects of manic depression and a theoretical monograph on strategies for translating between theories of mental illness. He is the author of *Essential Interactionism: On the Intelligibility of Prejudice*, published this year by Routledge and Kegan Paul, and co-author (1979, with Jonathan A. Freedman) of *Clinical Sociology*, published by Longman. His co-author in this issue of *ASR* is BRUCE BERG, a doctoral student in Sociology at Syracuse University, who is interested in deviance, religion, and death and dying.

■ MICHAEL SWAFFORD (Parametric Techniques for Contingency Table Analysis) is Assistant Professor, Department of Sociology, Vanderbilt University. He is completing a book on Soviet education, based on a year's research in the USSR and on extensive interviews with recent émigrés; studying the relationship between education and jobs in the United States, and participating in the development of a large-scale survey of recent Soviet émigrés to the United States, Germany, Canada, and Israel.

■ ROBERT M. HAUSER (On Stratification in a Dual Economy, Comment on Beck et al.) is Professor in both the Department of Sociology and the Institute for Research on Poverty, the University of Wisconsin–Madison. His book, *Opportunity and Change*, written with David L. Featherman, was published in 1978. He is currently researching the determinants of young men's earnings, the validity of survey reports of social and economic variables, historic changes in the hierarchy of occupational status, sex differences in socioeconomic achievement, and siblings' similarities and differences in achievement.

■ ANDREW M. GREELEY (Comment on Newport) is Professor, Department of Sociology, University of Arizona. His research currently centers on correlates of the religious imagination and on ethnic alcohol subcultures. He has (with Philip Morgan) recently published *The Structures of Prayer*. Another recent book, *Explaining Ethnic Subcultures*, was written with William McCready and Gary Theisen.

CAMBRIDGE

Who Should Know What?
Social Science, Privacy and Ethics
J. A. Barnes

Barnes looks at changes in the conduct of social science research since the early nineteenth century, discussing the ethical and political obligations of social scientists in collecting data and publishing their findings. His incisive study reveals the precariousness of the survival of a truly democratic social science. Hardcover $17.50 Paper $4.95

Measurement in the Social Sciences
The Link Between Theory and Data
Richard A. Zeller and Edward G. Carmines

In this theoretical and methodological introduction to the measurement techniques used in social science research, Zeller and Carmines thoroughly explain the assumptions, limitations, and interpretations of these techniques as they discuss the ways in which measurement principles can be effectively applied in research. Hardcover $26.95 Paper $7.95

Comrades and Christians
Religion and Political Struggle in Communist Italy
David I. Kertzer

Drawing from his own participant-observation research conducted in a working-class quarter of Bologna, Kertzer effectively blends individual portraits, case studies, and sociopolitical analysis in his examination of the ways in which the people actually experience their divided loyalties—torn between a tradition of anticlericalism and a Church that decries Communism as incompatible with Catholicism. Hardcover $24.95 Paper $6.95

Sociological Explanation as Translation
Stephen P. Turner

Using well-known examples, Turner presents a new account of the grounds for and validation of "interpretive" sociological claims, arguing that there is a "rationality" to interpretation. He offers a view of the relationship between statistical and qualitative claims that shows their complementary character. His significant study breaks new ground in understanding the conceptual bases for "understanding" social behavior. *ASA Rose Monograph Series*
Hardcover $14.95 Paper $5.95

other titles of interest

Elmdon: Continuity and Change in a North-West Essex Village
Jean Robin $00.00

Race Relations in Colonial Trinidad, 1870–1900
Bridget Brereton $34.50

Traditional Romanian Village Communities
The Transition from the Communal to the Capitalist Mode of Production in the Danube Region
Henri Stahl
translated by Daniel Chirot and Holley Coulter Chirot
Studies in Modern Capitalism/Etudes sur le Capitalisme Moderne. Co-published with *Les Editions de la Maison des Sciences de l'Homme* $34.50

On Marriage in Norway
Eilert Sundt
translated by Michael Drake *forthcoming*

A Class Against Itself
Power and the Nationalisation of the British Steel Industry
Doug McEachern *forthcoming*

Action and Interpretation
Studies in the Philosophy of the Social Sciences
Christopher Hookway and Philip Pettit, Editors
Now in Paperback $8.50

Pitmen, Preachers and Politics
Robert Moore *Now in Paperback* $10.95

Cambridge University Press
32 East 57th Street, New York, N.Y. 10022

New Books for Sociologists

Now available

PRINTERS FACE AUTOMATION
The Impact of Technology on Work and Retirement among Skilled Craftsmen
Theresa Rogers and **Nathalie S. Friedman**, Columbia University and Barnard College
Foreword by **A.H. Raskin**
208pp. ISBN 0-669-03310-3 $21.95

UNDERSTANDING ABUSIVE FAMILIES
James Garbarino and **Gwen Gilliam**, Boys Town Center for the Study of Youth Development
288 pp. ISBN 0-669-03621-8 $19.95

CHILDREN IN COOPERATION AND COMPETITION
Toward a Developmental Social Psychology
Emmy A. Pepitone, Bryn Mawr College
480pp. ISBN 0-669-02842-8 Now available

POPULATION DISPERSAL
A National Imperative
John Oosterbaan
160pp. ISBN 0-669-03615-3 Now available

PUBLIC POLICY AND THE FAMILY
Wives and Mothers in the Labor Force
Z.I. Giraldo, Duke University
ISBN 0-669-03762-1 Available in September

DEVIANT STREET NETWORKS
A Study of Heterosexual Prostitution in New York City
Bernard Cohen, City University of New York — Queens College
ISBN 0-669-03949-7 Available in fall 1980

THE SUNSHINE WIDOWS
Adapting to Sudden Bereavement
Carol D.H. Harvey, Boise State University, and **Howard M. Bahr**, Brigham Young University
176pp. ISBN 0-669-03375-8 $18.95

BOOKS AND PUBLISHERS
Commerce Against Culture in Postwar Britain
Michael Lane, University of Essex, with **Jeremy Booth**, Hull College of Higher Education
160pp. ISBN 0-669-03383-9 $16.95

THIRD-WORLD POVERTY
New Strategies for Measuring Development Progress
edited by **William Paul McGreevy**, Battelle Human Affairs Research Centers
ISBN 0-669-02839-8 Now available

NUTRITION POLICY IN TRANSITION
Jurgen Schmandt, **RoseAnn Shorey**, **Lilas Kinch**, and members of the Nutrition Policy Research Project, The University of Texas
320pp. ISBN 0-669-03596-3 $22.95

SOCIAL POLICIES AND PROGRAMS ON AGING
Louis Lowy, Boston University
ISBN 0-669-02342-6 Available in October

THE LOVE CANAL
Adeline Levine, State University of New York at Buffalo
ISBN 0-669-04034-7 Available in fall 1980

. . . and Watch for

Lexington Books

HEATH

LexingtonBooks
D. C. Heath and Company
125 Spring Street
Lexington, MA 02173
(617) 862-6650
(212) 924-6460

Call our toll-free number (800) 428-8071

The Journal of
Applied Behavioral
Science

Bureaucracy in the Eighties

Volume 16, Number 3 of THE JOURNAL OF APPLIED BEHAVIORAL SCIENCE is comprised of original articles that examine the emerging characteristics and policies of large organizations that promise to shape the 1980s. Some contributors examine power and authority in organizations in an international context. Articles on multinational corporations, bureaucracy in Britain, Africa, Sweden, and the U.S. identify major forces that confront and shape organizations. Other contributors examine the impact upon persons and roles of power and authority within organizations. Quality of work life, the bureaucratic personality, the changing nature of supervision, prospects for OD, wage and grading policies, and employee participation are all examined. The articles will benefit readers with applied and with theoretical concerns. The Special Issue editors are William Boyd Littrell, Chair, Department of Sociology, The University of Nebraska and Gideon Sjoberg, a professor in the Department of Sociology, The University of Texas at Austin.

CONTENTS

Part I. Bureaucracy and Power in International Perspective—R. Kelly Hancock; Sandra L. Albrecht; Gwyn Harries-Jenkins; Peter Blunt; Sara McLanahan

Part II. Bureaucracy, Authority, and the Person—Barry Stein and Rosabeth Moss Kanter; Norma M. Williams, Gideon Sjoberg, and Andrée F. Sjoberg; Paul M. Roman; W. Warner Burke; Elliott Jaques

Publication date: August 1980
Single copies available prepaid: $6.50
Order from: NTL Institute, Dept. X.

NTL®
INSTITUTE

P.O. Box 9155, Arlington, Virginia 22209 (703) 527-1500

CHARLES C THOMAS • PUBLISHER

New! THE SEXUAL VICTIMOLOGY OF YOUTH

Edited by **LeRoy G. Schultz,** *West Virginia Univ., Morgantown. (21 Contributors)* This broad-based collection of articles is concerned with both the protection of children and the protection of children's rights. It is a paragon of realism in an area fraught with inaccuracies, generalizations and stereotypes of victims and offenders.

The legal control of the sexual abuse of children is considered first, with a short history of efforts in this area and discussions of the sexual victimization and molestation of children. Sections also are included on medical and psychological diagnosis and treatment, incest, the victim and the justice system, the child sex industry, and sexual emancipation. The last of these constitutes a critical evaluation of age-of-consent and statutory rape laws.

Although this text is the product of many contributors, the editor has deftly drawn the material together into a cohesive overview of the sexual victimology of youth. A definitive bibliography is included. *'80, 432 pp., 1 il., 6 tables, $19.75*

New! **POLICE-COMMUNITY RELATIONS: Crisis in Our Time (3rd Ed.)** *by* **Howard H. Earle,** *California State Univ., Los Angeles. Foreword by* **William M. Conroy.** This expanded and updated edition begins by reviewing general problems of police-community relations (PCR), including police image and crisis areas. The author explores organizational factors and, in a section on the sociology of PCR, he evaluates the import and impact of civil disobedience and riots. The final segment explores PCR programming, the individual officer, and the future of PCR. *'80, 304 pp., 5 il., 1 table, $16.75*

New! **SOCIAL PSYCHOLOGICAL PERSPECTIVES ON LEISURE AND RECREATION** *edited by* **Seppo E. Iso-Ahola,** *Univ. of Iowa, Iowa City. (18 Contributors) '80, 448 pp., 6 il., 10 tables, $19.75*

INVOLUNTARY TREATMENT OF THE MENTALLY ILL: The Problem of Autonomy *by* **Michael Alfred Peszke,** *Univ. of Connecticut School of Medicine, Hartford.* The author of this book objectively examines and analyzes the issue of involuntary treatment of the mentally ill. He suggests program designs and proposes a system in which no one is deprived of appropriate treatment while no one is deprived of personal freedom in an arbitrary manner. *'75, 176 pp., 1 il., $13.75*

New! **BURGLARY AND THEFT** *by* **John M. Macdonald,** *Univ. of Colorado School of Medicine, Denver. Contributions by* **C. Donald Brannan** *and* **Robert E. Nicoletti.** *'80, 320 pp., $19.75*

New! **URBAN COMMUNITY CARE FOR THE DEVELOPMENTALLY DISABLED** *edited by* **Herbert J. Cohen,** *Albert Eistein College of Medicine, Bronx, New York, and* **David Kligler;** *with the assistance of* **Judith Eisler.** *(21 Contributors)* In this volume, actively involved professionals focus on issues and problems in caring for developmentally disabled persons in the urban setting. Impact of the courts and consumerism on services, available residential alternatives, family services, and specific models of care are among the areas scrutinized. *'80, 360 pp., 12 il., 13 tables, $25.50*

GAMBLING AND SOCIETY: Interdisciplinary Studies on the Subject of Gambling *edited by* **William R. Eadington,** *Univ. of Nevada, Reno. (34 Contributors)* All facets of the gambling phenomena are examined in this text. Topics covered include socioeconomic aspects, the behavior and motives of both normal and compulsive gamblers, attitudes toward risk-taking, game strategies, and legalization. *'76, 488 pp., 27 il., 40 tables, $44.00*

New! **SEX, MOTIVATION, AND THE CRIMINAL OFFENDER** *by* **Robert H. Morneau, Jr.,** *California State Univ., Los Angeles, and* **Robert R. Rockwell,** *Burns International Security Services, Inc., Oakland, California.* This authoritative volume explains the role that sex plays in the psychology of the criminal offender. Following a general definition of sexuality and a review of those factors that influence the way the sexual instinct is manifested, chapters draw a complete picture of the psychosexual underpinnings of criminality. *'80, 416 pp., 46 il., $29.75*

New! **WOMEN IN LAW ENFORCEMENT (2nd Ed.)** *by* **Peter Horne,** *St. Louis Community College at Meramec, St. Louis. Foreword by* **Lewis J. Sherman.** *'80, 288 pp., 7 il., cloth-$15.50, paper-$10.50*

PLURALISM IN EDUCATION: Conflict, Clarity and Commitment *by* **Richard Pratte,** *Ohio State Univ., Columbus.* In this series of essays, the author exposes the negative influence of pluralistic ideologies that mask outmoded educational policies and practices. *'79, 232 pp., 2 il., cloth-$15.75, paper-$9.75*

GRACIAN'S MANUAL: A Truthtelling Manual and the Art of Worldly Wisdom (2nd Ed., 6th Ptg.) *translated by* **Martin Fischer.** *'79, 324 pp. (3 1/2 x 5 1/4), $7.95*

RAPE IN PRISON *by* **Anthony M. Scacco, Jr.,** *Waterbury, Connecticut.* This book employs the author's personal experiences, unpublished criminological papers, and the testimony of inmates to present a realistic view of sexual aggression in correctional institutions. The text covers such topics as the institutional setting and its contribution to sexual aggression, the sexual acts *per se*, the victims, the internal sexual violence as a microcosm of society as a whole, and the possibilities for change. *'75, 144 pp. (5 3/8 x 8 1/2), $13.50*

New! **THE OLDER WOMAN: Lavender Rose or Gray Panther** *edited by* **Marie Marschall Fuller,** *Texas Woman's Univ., and* **Cora Ann Martin,** *North Texas State Univ., both of Denton. (41 Contributors)* The lives of older women, their unique problems and their needs are described in this anthology. A broad range of articles examines such aspects as socialization, biological facts and fallacies, social bonds, institutions, alternative lifestyles, and the future of older women in society. *'80, 368 pp., 19 tables, $16.75, paper*

New! **THE NEW POLICE TECHNOLOGY: Impact of the Computer and Automation on Police Staff and Line Performance** *by* **V. A. Leonard,** *Denton, Texas. '80, 360 pp., 48 il., 4 tables, $29.75*

New! **THE ROLE OF THE FORENSIC PSYCHOLOGIST** *edited by* **Gerald Cooke,** *Norristown State Hospital, Norristown, Pennsylvania. Foreword by* **David L. Bazelon.** *(41 Contributors) '80, 432 pp., 10 tables, $26.50*

New! **TRANSSEXUALITY IN THE MALE: The Spectrum of Gender Dysphoria** *by* **Erwin K. Koranyi,** *Univ. of Ottawa, Ottawa, Ontario, Canada. Foreword by* **Ralph Slovenko.** *(3 Contributors) '80, 192 pp., 31 il., 1 table, $17.50*

OUR CHILDREN SHOULD BE WORKING *by* **William N. Stephens,** *Dalhousie Univ., Halifax, Nova Scotia, Canada. '79, 228 pp., 2 il., 1 table, $12.75*

ACUTE GRIEF AND THE FUNERAL *edited by* **Vanderlyn R. Pine, Austin H. Kutscher, David Peretz, Robert C. Slater, Robert DeBellis, Robert J. Volk** *and* **Daniel J. Cherico.** *(47 Contributors)* The articles in this book describe the psychological and social aspects of bereavement during the period of acute grief. *'76, 328 pp., 1 il., 26 tables, $21.00*

Orders with remittance sent, on approval, postpaid

301-327 East Lawrence Avenue • Springfield • Illinois • 62717

New
from
Columbia

In Quest

Journal of an Unquiet Pilgrimage

SIGMUND DIAMOND. As he relates impressions of his journey—from London, through Eastern Europe and the Soviet Union, culminating in a year's stay in Israel—historian-sociologist Sigmund Diamond reveals a personal consciousness of history at work on the experience that was his own and on the experience he shared with others—Jews and non-Jews—in this turbulent century. 240 pages, illus., $10.95

Man in the Age of Technology

ARNOLD GEHLEN. Translated by Patricia Lipscomb; with a Foreword by Peter L. Berger. Probing the crisis of human personality in our century, *Man in the Age of Technology* traces the dizzying social after-effects resulting from the loss of the institutions that grew out of and sustained an earlier, agrarian age. Published in Germany in 1949, this is the first English translation of Arnold Gehlen's seminal work. $15.00

The Age of Structuralism

Lévi-Strauss to Foucault

EDITH KURZWELL. This lucid study of the sociological, political, and philosophic currents of structuralism offers introductions to eight prominent French thinkers—five who fueled the movement and three who provided intellectual opposition. Examining the work of Claude Lévi-Strauss, Louis Althusser, Roland Barthes, Jacques Lacan, Michel Foucault, Henri Lefèbvre, Alain Touraine, and Paul Ricoeur, *The Age of Structuralism* provides a lively and clear portrait of a movement whose impact still reverberates today. $20.00 cloth, $6.95 paper

Behind Every Successful Man

Wives of Medicine and Academe

MARTHA R. FOWLKES. The interconnectedness of the worlds of family and work, and the discovery that marriages are made in the image of the occupational world provide the focus for Martha R. Fowlkes' *Behind Every Successful Man*. Based on 40 in-depth interviews with the wives of physicians and university professors, the book links specific features of a wife's role to her husband's career structure and emphasizes how a wife enables her husband to work. 240 pages, $17.50

Now in paperback:

Socialism, Politics, and Equality

Hierarchy and Change in Eastern Europe and the USSR

WALTER D. CONNOR. "Connor's discussion of social structure and social change in Eastern Europe is a comprehensive and magisterial treatment of a difficult subject."—*Library Journal* 488 pages, $10.00 paper

Chinese Family and Kinship

HUGH D. R. BAKER. This work is the first synthesis in more than 30 years of what is known about traditional Chinese family and kinship relations. 272 pages, $6.00 paper

From Ritual to Record

The Nature of Modern Sports

ALLEN GUTTMANN. "A pioneering work in a field remarkably little touched, except by specialists writing about their own particular sport. For any future writer about the philosophy or the sociology of sport in general, this book will be required reading."—*Times Literary Supplement*
198 pages, $5.00 paper

Order any of the books advertsied by sending check or money order to **Dept. JN** at the address below, including $1.30 per order for postage and handling.

COLUMBIA UNIVERSITY PRESS
Address for orders:
136 South Broadway, Irvington, New York 10533

Machine Politics in Transition

Party and Community in Chicago

Thomas M. Guterbock

For three years, Thomas M. Guterbock participated in the daily activities of the Regular Democratic Organization in one North Side Chicago ward in order to discover how political machines win the support of the urban electorate.

Cloth 352 pages $20.00

Before the Convention

A Theory of Presidential Nomination Campaigns

John A. Aldrich

Aldrich seeks to determine why and how candidates choose to run, why some succeed and others fail, and to determine what consequences the nomination process has for the general election and, later, for the President in office.

Paper 312 pages $7.95 Also available in cloth.

The Gray Lobby

Henry J. Pratt

"*The Gray Lobby* is a success because it goes beyond the sphere of old age and provides an excellent model, based on the senior citizen movement, to explain the mass character of social movements in general, as well as policy systems and strategies for successful advocation and implementation of legislation."—Hedy Weinberg, *Social Service Review*

Paper 264 pages $5.95

The Black Extended Family

Elmer P. Martin and Joanne Mitchell Martin

The Martins point out that what appear to be fatherless, broken homes in our cities may really be vital parts of strong and flexible extended families based hundreds of miles away—usually in rural areas.

Paper 144 pages Illus. $3.95

The Joyful Community

An Account of the Bruderhof—A Communal Movement Now in Its Third Generation

With a new Preface

Benjamin Zablocki

"Ben Zablocki's book belongs on the shelves and reading lists of every social scientist concerned with communities and 'community.' "—Herbert J. Gans, Columbia University

Paper 362 pages $3.95

The University of Chicago Press • Chicago 60637

ASA-SPONSORED PUBLICATIONS

A.S.A. PRESIDENTIAL SERIES

(Published by The Free Press)

These annual volumes, published jointly by The Free Press and the American Sociological Association, are collections of original essays derived from presentations at ASA's annual meetings. Titles include...

Approaches to the Study of Social Structure (Peter M. Blau, Editor)	($12.95)*
Conflict and Dissensus (Lewis A. Coser & Otto N. Larsen, Editors)	($14.95)*
Major Social Issues: A Multidisciplinary View (J. Milton Yinger, Editor)	($16.95)*
Societal Growth: Processes and Implications (Amos H. Hawley, Editor)	($16.50)*

Order from: THE FREE PRESS, 866 Third Avenue, New York, New York 10022

ANNUAL REVIEW OF SOCIOLOGY ***(Alex Inkeles, Editor)***

The publication of this *Annual Review* provides the sociological community with a special opportunity for taking stock at intervals of progress in its various subfields. The articles identify the critical issues, sort out the research evidence bearing on them, and facilitate systematic assessment.

- **Volumes 1-5 (1975-1979)**

U.S. price	$17.00 per volume*
Elsewhere	$17.50 per volume*

(Add 6% of net price for volumes to be shipped to California residents.)

Order from: ANNUAL REVIEW, INC., 4139 El Camino Way, Palo Alto, CA 94306

*** Individual members of the ASA are entitled to a special discount. Details available from the American Sociological Association • 1722 N Street, N.W. • Washington, D.C. 20036**

JOURNAL OF

HEALTH & SOCIAL BEHAVIOR

(Howard B. Kaplan, Editor)

(Quarterly—ISSN 0022-1465)

JHSB publishes reports of empirical studies, theoretical analyses, and synthesizing reviews that employ a sociological perspective to clarify aspects of social life bearing on human health and illness, both physical and mental. Its scope includes studies of the organizations, institutions, and occupations devoted to health services, as well as studies of the behavior of actual and potential recipients of these services.

Recent issues contain reports on:

Social Class and the Use of Professional Help for Personal Problems: 1957 and 1976, RICHARD A. KULKA, JOSEPH VEROFF & ELIZABETH DOUVAN

Health Occupations in Israel: Comparative Patterns of Change During Socialization, JUDITH T. SHUVAL AND ISRAEL ADLER

Occupational Stress and Health Among Factory Workers, JAMES S. HOUSE, ANTHONY J. McMICHAEL, JAMES A. WELLS, BERTON H. KAPLAN, & LAWRENCE R. LANDERMAN

The Functional Importance of Sex Roles and Sex-Related Behavior in Societal Reactions to Residual Deviants, WILLIAM A. RUSHING

Stress, Social Status, and Psychological Distress, RONALD C. KESSLER

ASA members, $8 **Non-members, $12** **Institutions, $16**
Single issues, $4

Published by THE AMERICAN SOCIOLOGICAL ASSOCIATION
1722 N Street, N.W. • Washington, D.C. 20036

ROSE MONOGRAPH SERIES

(Published by Cambridge University Press)

(Suzanne Keller, Editor)

The ASA Rose Monograph Series, established in 1968 by a fund set up by Arnold and Caroline Rose, was developed to publish high quality monographs and theoretical studies in sociology. Titles include:

Middle Start: An Experiment in the Educational Enrichment of Young Adolescents
(Yinger, Ikeda, Laycock, Cutler) **(Cloth—$13.95; Paper—$4.95)***

Education, Employment and Migration: Israel in Comparative Perspective
(Ritterband) **(Cloth—$13.95; Paper—$4.95)***

Class, Race and Worker Insurgency: The League of Revolutionary Black Workers
(Geschwender) **(Cloth—$13.95; Paper—$4.95)***

Protest and Participation: The New Working Class in Italy
(Low-Beer) **(Cloth—$16.95; Paper—$4.95)***

Continuity and Change: A Study of Two Ethnic Communities in Israel
(Simon) **(Cloth—$13.95; Paper—$4.95)***

Cities with Little Crime: The Case of Switzerland
(Clinard) **(Cloth—$13.50; Paper—$4.95)***

Opening and Closing: Strategies of Information Adaptation in Society
(Klapp) **(Cloth—$13.95; Paper—$4.95)***

Tasks and Social Relationships in Classrooms: A Study of Instructional Organization and Its Consequences
(Bossert) **(Cloth—$14.95; Paper—$4.95)***

Juvenile Delinquency and Its Origins: An Integrated Theoretical Approach
(Johnson) **(Cloth—$14.95; Paper—$4.95)***

Understanding Events: Affect and the Construction of Social Action
(Heise) **(Cloth—$14.95; Paper—$4.95)***

From Student to Nurse: A Longitudinal Study of Socialization
(Simpson) **(Cloth—$14.95; Paper—$4.95)***

Order the above monographs from: CAMBRIDGE UNIVERSITY PRESS, 32 East 57th Street, New York, New York 10022

* * *

Books previously published in the series . . . *Available only in paperback at $5.00 each from the ASA, 1722 N Street, N.W., Washington, D.C. 20036:*

Socioeconomic Background and Educational Performance (Hauser) • ***Black and White Self-Esteem: The Urban School Child*** (Rosenberg & Simmons) • ***Looking Ahead: Self-Conceptions, Race and Family as Determinants of Adolescent Orientation to Achievement*** (Gordon) • ***Black Students in Protest: A Study of the Origins of the Black Student Movement*** (Orum) • ***Attitudes and Facilitation in the Attainment of Status*** (Gasson, Haller & Sewell) • ***Patterns of Contact with Relatives*** (Klatzky) • ***Interorganizational Activation in Urban Communities: Deducations from the Concept of System*** (Turk) • ***The Study of Political Commitment*** (DeLamater) • ***Ambition and Attainment: A Study of Four Samples of American Boys*** (Kerckhoff) • ***The Greek Peasant*** (McNall) • ***Patterns of Scientific Research: A Comparative Analysis of Research in Three Scientific Fields*** (Hargens) • ***Ethnic Stratification in Peninsular Malaysia*** (Hirschman) • ***Deviance, Selves and Others*** (Schwartz & Stryker)

***Individual members of the A.S.A. are entitled to a special discount. Contact:**

AMERICAN SOCIOLOGICAL ASSOCIATION
1722 N Street, N.W.
Washington, D.C. 20036

SOCIOLOGY OF EDUCATION

A Journal of Research in Socialization & Social Structure

(Alan C. Kerckhoff, *Editor*)

(Quarterly—ISSN 0038-0407)

Sociology of Education publishes papers on educational processes, and on human development. The research may focus on the individual, institutions, and structural arrangements among institutions bearing on education and human development.

◆

Recent issues have included studies on:

Interactive Influences in Spady and Tinto's Conceptual Models of College Attrition—PASCARELLA & TERENZINI

Loose Structure, Retrenchment and Adaptability in the University—RUBIN

School SES Influences—Composition or Context?—ALEXANDER, FENNESSEY, McDILL & D'AMICO

Academic Sponsorship and Scientists' Careers—RESKIN

Three Perspectives on IQ and Social Differentiation—GUTERMAN; GORDON & RUDERT, ECKLAND

◆

ASA members, $8 **Non-members, $12** **Institutions, $16**
Single issues, $4

Order from: **American Sociological Association**
1722 N Street, N.W.
Washington, D.C. 20036

Journals of the
American Sociological Association

AMERICAN SOCIOLOGICAL REVIEW
(Rita J. Simon, *Editor* Bi-monthly—ISSN 0003-1224)

Official Journal of the A.S.A. Articles of major concern to social scientists. New trends and developments in theory and research.

Members, $10 Non-members, $15 Institutions, $30
Non-member students, $10 Single issues, $4

CONTEMPORARY SOCIOLOGY: A Journal of Reviews
(Norval D. Glenn, *Editor* Bi-monthly—ISSN 0094-3061)

A journal of reviews, criticism, and debate designed to give new thrust and style to reviewing and intellectual exchange within the field.

Members, $10 Non-members, $15 Institutions, $20
Non-member students, $10 Single issues, $4

JOURNAL OF HEALTH & SOCIAL BEHAVIOR
(Howard B. Kaplan, *Editor* Quarterly—ISSN 0022-1465)

Distinctive for a sociological approach to the definition and analysis of problems bearing on human health and illness.

Members, $8 Non-members, $12 Institutions, $16 Single issues, $4

SOCIOLOGY OF EDUCATION
(Alan C. Kerckhoff, *Editor* Quarterly—ISSN 0038-0407)

A forum for social scientists concerned with research on education and human social development.

Members, $8 Non-members, $12 Institutions, $16 Single issues, $4

SOCIAL PSYCHOLOGY QUARTERLY
(George W. Bohrnstedt, *Editor* Quarterly—ISSN 0190-2725)

A journal of research and theory in social psychology. Genuinely interdisciplinary in the publication of works by both sociologists and psychologists.

Members, $8 Non-members, $12 Institutions, $16 Single issues, $4

THE AMERICAN SOCIOLOGIST
(James L. McCartney, *Editor* Quarterly—ISSN 0003-1232)

Contains major articles analyzing sociology as a profession and as a discipline. Included are reports on standards and practices in teaching, research, publication and the application of sociological knowledge.

Members, $8 Non-members, $12 Institutions, $16 Single issues, $4

AMERICAN SOCIOLOGICAL ASSOCIATION
1722 N Street, N.W. • Washington, D.C. 20036

ROUTLEDGE & KEGAN PAUL

9 Park Street, Boston, Mass. 02108

Elitism

G. LOWELL FIELD and
JOHN HIGLEY
University of Connecticut and
Australian National University

This book presents an important critique of prevailing political doctrine in Western societies at a time of major change in the circumstances of Western civilization. It pleads for a more realistic appraisal of elite and mass roles in politics, arguing that political stability and any meaningful degree of representative democracy depend fundamentally on the existence of specific kinds of elites. The authors analyse where and how Western political thought became unrealistic, and show how a restated version of elitist ideas greatly clarifies the policy and moral implication of present world conflicts. $18.50

Contemporary Hermeneutics

Hermeneutics as Method, Philosophy and Critique
JOSEF BLEICHER
Glasgow College of Technology

Contemporary Hermeneutics reveals the profound impact this philosophy is having on a wide range of intellectual pursuits, and analyses selections from the works of five major writers in the field–Betti, Gadamer, Ricoeur, Apel and Hebermas–which present the main debates going on within this new and challenging field. August $28.00, paper $14.00

Talcott Parsons and the Conceptual Dilemma

HANS P.M. ADRIAANSENS
University of Tilburg, Netherlands

This analysis of the nature and development of Talcott Parsons' theory of action offers an introduction to the conceptual paradigm upon which this theory is based, and gives an explanation of the developments which the action theory has undergone during the half-century of Parsons' career. *International Library of Sociology* August $27.50

Rousseau and Weber

A Study in the Theory of Legitimacy
J.G. MERQUIOR
Until recently, King's College, London

Rousseau and Weber are central figures in the discussion of political legitimacy; this book describes and assesses their contribution to the understanding of the subject. Dr Merquior asserts the adequacy of Rousseau's ideal of participatory democracy to the modern experience of democracy, and shows that Weber's theory of legitimacy is handicapped by the culturalism of its major premises. *International Library of Sociology* August $37.50

Essays in Law and Society

Edited by GEOFF MUNGHAM and
ZENON BANKOWSKI
University College, Cardiff and
University of Edinburgh
Contributors: P. Fennel, C. May, H. Williamson, R. Pearson, P. Young, M. Stephens, R. Ellis, D. Dixon, T. Keenoy, G. Mungham, Z. Bankowski

Covering a wide range of empirical and theoretical material, these essays provide an important introduction to some of the contemporary work being done by those who represent the 'newest generation' in the field. Some areas covered are plea bargaining, the role of the lawyer in juvenile courts, and lay participation in the legal process. September Original paperback about $17.50

Founding Theory of American Sociology 1881-1915

ROSCOE C. HINKLE
Ohio State University, Columbus

Professor Hinkle shows that early American sociological theory focused on social ontological interests, and by contrasting earlier and later theories, he provides a basis for interpreting one period in American theory in relation to other periods.

The price of this book was previously erroneously announced at $135.00

STATE OF KUWAIT
KUWAIT UNIVERSITY

Applications are invited for Academic Posts in Faculty of Arts & Education, Kuwait University in the following departments:

Department of Sociology & Social Work: (Prof., Assoc. Prof., Assist. Prof.)

1. Anthropology: General Anthropology

2. Sociology: Methodology, Statistics, Social Psychology, Social Organization, Sociological Theories

3. Social Work: Social Welfare Policy, Social Work Practice, Human Behaviour & Social Environment, Field Instruction

4. Department of Psychology: (Assist. Prof.) Organizational Psychology

For all of the above positions:

Contracts for two years, renewable for a further period of four eyars by mutual agreement of the two parties, in accordance with the following:

Teaching Arabic is a must.
Method of teaching is based on the credit system.
Holding of Ph.D. degree or its official equivalent.
Applications which will not be considered are:

☐ Non holders of Ph.D. degrees or its equivalent, even if they are expected to hold it later on.
☐ Applications submitted for other specializations.

Early applications will be given priority.

Completed applications including full details together with a non-returnable copy of the Ph.D. degree, other academic qualifications, and candidate's publications, should be sent directly to Department of Administrative Affairs, P. O. Box 5969 (Safat) Kuwait.

Additional information required by the candidate can be requested on the course in subject, from:

Chairman, Dept. of Sociology & Social Work
or
Chairman, Dept. of Psychology
Faculty of Arts & Education
Kuwait University
P. O. Box 23558
(Safat)
Kuwait

ONLY THE BEST.
Harper & Row sociology texts.

COLEMAN & CRESSEY
SOCIAL PROBLEMS
583 pages. 1/80. ISBN 0-06-041326-3.
Instructor's Manual.

GORDON & JOHNSON
READINGS IN HUMAN SEXUALITY
Contemporary Perspectives,
Second Edition
400 pages. 4/80. ISBN 0-06-042399-4.

GRIFFIN & GRIFFIN
JUVENILE DELINQUENCY IN PERSPECTIVE
452 pages. 1978. ISBN 0-06-042512-1.

KENKEL
SOCIETY IN ACTION
Introduction to Sociology,
Second Edition
600 pages. 2/80. ISBN 0-06-384486-9.
Instructor's Manual. Study Guide. Test Bank.

MILLER & FORM
INDUSTRIAL SOCIOLOGY
Work in Organizational Life,
Third Edition
834 pages. '79. ISBN 0-06-044471-1.

ROSENGREN
SOCIOLOGY OF MEDICINE
Diversity, Conflict, and Change
446 pages. '79. ISBN 0-06-045582-9.

SCHUR
INTERPRETING DEVIANCE
A Sociological Introduction
539 pages. '79. ISBN 0-06-045811-9.

SHERMAN & WOOD
SOCIOLOGY
Traditional and Radical Perspectives
413 pages. '79. ISBN 0-06-046107-1.
Instructor's Manual.

WHITTEN
READINGS IN SOCIOLOGY
Contemporary Perspectives, ***Second Edition***
302 pages. '79. ISBN 0-06-045503-9.

Harper & Row
10 East 53d Street, New York, N.Y. 10022

To request examination copies, write to Suite 5D. Please include course title, enrollment, and present text.

10 MAR 1981

American Sociological Review

Heredity, Environment, and Public Policy Reconsidered 723
Christopher Jencks

Bringing the Firms Back In 737
James N. Baron and William T. Bielby

Crime and Poverty 766
Richard A. Berk, Kenneth J. Lenihan, and Peter H. Rossi

Social Networks and Social Movements 787
David A. Snow, Louis A. Zurcher, Jr., and Sheldon Ekland-Olson

Differential Sentencing of White-Collar Offenders 802
John Hagan, Ilene H. Nagel (Bernstein), and Celesta Albonetti

Testing a Structural Theory of Corporate Cooptation 821
Ronald S. Burt, Kenneth P. Christman, and Harold C. Kilburn, Jr.

Official Reactions to Rape 842
Gary D. LaFree

Comments 855
Marcia Whicker Taylor; Walter R. Gove and Michael Hughes; Alan Booth, David R. Johnson, and John N. Edwards

Volume 45 October 1980 Number 5

Notice to Contributors

A processing fee of $10 is required for each paper submitted, except for comments; such fees will be waived for student members of ASA. A check or money order, made payable to the American Sociological Association, should accompany each submission. The fee must be paid in order for the review process to begin.

The length of an *ASR* paper is generally ten (10) printed pages or thirty (30) typed pages. However, the *ASR* is happy to accept manuscripts up to fifty (50) typed pages in length for review. We also are interested in receiving manuscripts for a Research and Notes Section.

To permit anonymity in the review of manuscripts, keep identifying material out of the manuscript. Attach a cover page giving authorship, institutional affiliation and acknowledgments, and provide only the title as identification on the manuscript and abstract.

ASA Multiple Submissions Policy: Submission of a manuscript to a professional journal clearly implies commitment to publish in that journal. The competition for journal space requires a great deal of time and effort on the part of editorial readers whose only compensation for this service is the opportunity to read papers prior to publication and the gratification associated with discharge of professional obligation. For these reasons, *the American Sociological Association regards submission of a manuscript to a professional journal while that paper is under review by another journal as unacceptable.*

SUBMISSION AND PREPARATION OF MANUSCRIPTS

1. Submit four (4) copies and retain the original for your files. Copies may be Xerox, mimeograph or multilith, but not carbons.
2. Enclose a stamped self-addressed postcard for acknowledgment of manuscript receipt. *Manuscripts will not be returned.* Please do not send your original copy.
3. All copy must be typed, doublespaced (including indented material, footnotes and references) on 8½ by 11 inch white opaque paper. Lines must not exceed six (6) inches. Margins must be a minimum of one inch.
4. Include four (4) copies of an abstract of no more than 150 words.
5. Type each table on a separate page. Insert a location note at the appropriate place in the text, e.g., "Table 2 about here." Send copies, retain originals.
6. Figures must be drawn in India ink on white paper. Send copies, retain originals.
7. Clarify all symbols with notes in the margins of the manuscript. Circle these and all other explanatory notes not intended for printing.
8. Footnotes should not be placed in the body of the text. Type them (doublespaced) and attach them as a separate appendix to the text. Number them consecutively throughout the text. Footnotes are to be kept to a minimum and used only for substantive observations. Source citations are made within the text rather than in footnotes.
9. Acknowledgments, credits and grant numbers are placed on the title page with an asterisk.

REFERENCE FORMAT

A. *In the text:* All source references are to be identified at the appropriate point in the text by the last name of the author, year of publication and pagination where needed. Identify subsequent citations of the same source in the same way as the first, not using *"ibid.," "op. cit.," or "loc. cit."* Examples follow:

1. If author's name is in the text, follow it with year in parentheses. [". . . Duncan (1959) . . ."]
2. If author's name is not in the text, insert, in parentheses, the last name and year, separated by a comma. [". . . (cf. Gouldner, 1963) . . ."]
3. Pagination (without "p." or "pp.") follows year of publication after a colon. [". . . Kuhn (1970:71)."]
4. Give both last names for dual authors; for more than two use "et al." in the text. When two authors have the same last name, use identifying initials in the text. For institutional authorship, supply minimum identification from the beginning of the complete citation. [". . . (U.S. Bureau of the Census, 1963:117) . . ."]
5. Separate a series of references with semicolons and enclose them within a single pair of parentheses. [". . . (Burgess, 1968; Marwell et al., 1971. Cohen, 1962) . . ."]

B. *In the appendix:* List all source citations by author, and within author by year of publication, in an appendix titled "References." The reference appendix must be complete and include all references in the text. The use of "et al." is not acceptable in the appendix; list the names of all authors. (See A. 4. for text format.)

If there is more than one reference to the same author and year, distinguish them by the letters, a, b, etc. added to the year. [". . . Levy (1965a:331) . . ."] Give the publisher's name in as brief a form as is fully intelligible. For example, John A. Wiley and Sons should be "Wiley." If the cited material is unpublished, use "forthcoming" with name of journal or publisher; otherwise use "unpublished."

Use no underlining, italics or abbreviations.

Examples follow:

1. *Books:*	Jud, Gerald J., Edgar W. Mills, Jr. and Genevieve Walters Burch	
	1970	Ex-Pastors. Philadelphia: Pilgrim Press.
	U.S. Bureau of the Census	
	1960	Characteristics of Population. Volume 1. Washington, D.C.: U.S. Government Printing Office.
	Bernard, Claude	
	[1865] 1957	An Introduction to the Study of Experimental Medicine. Tr. Henry Copley Greene, New York: Dover.
2. *Periodicals:*	Conger, Rand	
	Forthcoming	"The effects of positive feedback on direction and amount of verbalization in a social setting." Pacific Sociological Review.
	Merton, Robert K.	
	1963a	"The ambivalence of scientists." Bulletin of The Johns Hopkins Hospital 112:77–97.
	1963b	"Resistance to the systematic study of multiple discoveries in science." European Journal of Sociology 4:237–82.
3. *Collections:*	Davie, M.	
	1938	"The pattern of urban growth." Pp. 133–61 in G. Murdock (ed.), Studies in the Science of Society. New Haven: Yale University Press.

See recent issues for further examples.

Revised 1980 ASR

AMERICAN SOCIOLOGICAL REVIEW

Official Journal of the American Sociological Association

OCTOBER, 1980 VOLUME 45, NUMBER 5

ARTICLES

Heredity, Environment, and Public Policy Reconsidered .... Christopher Jencks 723

Bringing the Firms Back In: Stratification, Segmentation, and the Organization of Work .... James N. Baron and William T. Bielby 737

Crime and Poverty: Some Experimental Evidence from Ex-Offenders .... Richard A. Berk, Kenneth J. Lenihan, and Peter H. Rossi 766

Social Networks and Social Movements: A Microstructural Approach to Differential Recruitment .... David A. Snow, Louis A. Zurcher, Jr., and Sheldon Ekland-Olson 787

The Differential Sentencing of White-Collar Offenders in Ten Federal District Courts .... John Hagan, Ilene H. Nagel (Bernstein), and Celesta Albonetti 802

Testing a Structural Theory of Corporate Cooptation: Interorganizational Directorate Ties As a Strategy for Avoiding Market Constraints on Profits .... Ronald S. Burt, Kenneth P. Christman, Harold C. Kilburn, Jr. 821

The Effect of Sexual Stratification by Race on Official Reactions to Rape .... Gary D. LaFree 842

COMMENTS AND REPLIES

The Noninfluence of "Family Background" on Intellectual Attainment (A Critique of Scarr and Weinberg, *ASR*, October, 1978) .... Marcia Whicker Taylor 855

Calling All Camps! The War Is Over! (Reply to Taylor) .... Sandra Scarr and Richard A. Weinberg 859

The Effects of Crowding Found in the Toronto Study: Some Methodological and Empirical Questions (A Comment on Booth and Edwards, *ASR*, 1976) .... Walter R. Gove and Michael Hughes 864

Reply to Gove and Hughes .... Alan Booth, David R. Johnson, and John N. Edwards 870

In Pursuit of Pathology: The Effects of Human Crowding (Comment on Gove, Hughes, and Galle, *ASR*, February, 1979) ..... Alan Booth, David R. Johnson, and John N. Edwards 873

In Pursuit of Preconceptions: A Reply to the Claim of Booth and His Colleagues That Household Crowding Is Not an Important Variable (Reply to Booth, Johnson, and Edwards) .... Walter R. Gove and Michael Hughes 878

OTHER

The Editor's Items 887

Editor: RITA J. SIMON

NEW EDITOR

Effective with the February 1981 issue, William H. Form will become the editor of the American Sociological Review. Beginning July 1, 1980, send all manuscripts to Prof. William H. Form, Department of Sociology, 326 Lincoln Hall, University of Illinois, Urbana, IL 61801.

Deputy Editors: CLARK McPHAIL AND JOE L. SPAETH

Associate Editors: PAUL BURSTEIN, ROBERT EVAN COLE, THOMAS J. FARARO, BERNARD FARBER, DANIEL GLASER, MICHAEL J. HINDELANG, GISELA HINKLE, ROSABETH KANTER, JAMES R. KLUEGEL, JOHN LOFLAND, MURRAY MELBIN, MICHAEL MOCH, ANTHONY OBERSCHALL, VALERIE K. OPPENHEIMER, JEFFREY PFEFFER, ALEJANDRO PORTES, SAMUEL PRESTON, BARBARA RESKIN, RICHARD RUBINSON, CHARLES W. TUCKER, NANCY BRANDON TUMA, JOHN WILSON, MAYER N. ZALD, LYNNE G. ZUCKER.

Copy Editor: ELIZABETH ROBINSON NEUMANN

Managing Editor: SHARON GUNASON

Editorial Assistant: HELEN CURLEY

Executive Officer: RUSSELL R. DYNES

SCOPE AND MISSION OF AMERICAN SOCIOLOGICAL REVIEW: The *American Sociological Review* publishes work of interest to the discipline in general, new theoretical developments, results of research that advance our understanding of the most fundamental social processes, and important methodological innovations. Like other publications of the Association, emphasis is given to exceptional quality. Unlike the more specialized journals of the Association, the *American Sociological Review's* primary focus is on whatever has the most general bearing on the knowledge of society.

Concerning manuscripts, address: Editor, *American Sociological Review*, Department of Sociology, University of Illinois, Urbana, Illinois 61801. (217) 333-6515.

The *American Sociological Review* (ISSN 0003–1224) is published at 49 Sheridan Avenue Albany, New York, bimonthly in February, April, June, August, October, and December.

Applications for permission to quote from this journal should be addressed to the Executive Office

Copyright © 1980 American Sociological Association:

Annual membership dues of the American Sociological Association, $20–$120 ($10 of this amount may cover a subscription to the *American Sociological Review*); Associate, $25; International Associate, $25; Student Associate, $12.

Application for membership and payment of dues should be made to the Executive Office. Subscription rate for members, $10; nonmembers, $15; institutions and libraries, $30; nonmember students, $10. Single issue, $4.

New subscriptions and renewals will be entered on a calendar year basis only.

Change of address: Six weeks advance notice to the Executive Office and old address as well as new are necessary for change of subscriber's address.

Concerning advertising, changes of address, and subscriptions, address: Executive Office, American Sociological Association, 1722 N Street, N.W., Washington, D.C. 20036.

Claims for undelivered copies must be made within the month following the regular month of publication. The publishers will supply the missing copies when losses have been sustained in transit and when the reserve stock will permit.

Second class postage paid at Washington, D.C. and additional mailing offices.

MEMBERS OF THE COUNCIL, 1981

Officers of the Association

WILLIAM FOOTE WHYTE, President
Cornell University
RENÉE C. FOX, Vice President
University of Pennsylvania
ERVING GOFFMAN, President-Elect
University of Pennsylvania
JOAN HUBER, Vice President-Elect
University of Illinois–Urbana-Champaign
HERBERT L. COSTNER, Secretary
University of Washington
PETER H. ROSSI, Past President
University of Massachusetts, Amherst
RUSSELL R. DYNES, Executive Officer
American Sociological Association

Elected-at-Large

PAULINE B. BART
University of Illinois, Chicago Circle
NORMAN BIRNBAUM
Amherst College
ERNEST Q. CAMPBELL
Vanderbilt University
ARLENE K. DANIELS
Northwestern University
IRWIN DEUTSCHER
University of Akron
WILLIAM GAMSON
University of Michigan
HELENA Z. LOPATA
Loyola University of Chicago
THOMAS F. PETTIGREW
Harvard University
MORRIS ROSENBERG
University of Maryland
IMMANUEL WALLERSTEIN
SUNY, Binghamton
CHARLES V. WILLIE
Harvard University
MAURICE ZEITLIN
University of Cal., Los Angeles

(Articles in the REVIEW are indexed in the Social Sciences Index, Psychological Abstracts, Sociological Abstracts, Ayer's Guide, University Microfilms, Abstracts for Social Workers, International Political Science Abstracts, and United States Political Science Documents.)

AMERICAN SOCIOLOGICAL REVIEW

HEREDITY, ENVIRONMENT, AND PUBLIC POLICY RECONSIDERED*

CHRISTOPHER JENCKS
Northwestern University

American Sociological Review 1980, Vol. 45 (October):723–736

Given suitable data, we can estimate the degree to which genetic variation explains observed variation in human behavior (h^2). But, contrary to what most investigators assume, the amount of nongenetic variation ($1 - h^2$) sets no upper limit on the explanatory power of environmental variation. This is because genetic variation often causes environmental variation, so the two categories are not mutually exclusive. The assumption that the two categories are mutually exclusive derives from a narrowly physiological model of the way in which genes affect behavior. This model is inconsistent with all the most familiar examples of genes affecting behavior, especially the examples of race and sex. Heritability estimates set no upper limit on the potential effect of reducing or eliminating variation in environmental factors that currently vary in response to genotype, as many do. Nor do they set an upper limit on the effect of creating new environments. Heritability estimates *do* set an upper limit on the effect of reducing or eliminating environmental variations that are independent of genotype, but other statistics usually provide even better estimates of these effects. There is no evidence that genetically based inequalities are harder to eliminate than other inequalities. Until we know *how* genes affect specific forms of behavior, heritability estimates will tell us almost nothing of importance.

INTRODUCTION

Both social and natural scientists have been quarreling about the relative importance of heredity and environment for more than half a century. So far as I can discover, this seemingly endless debate has not led to a single substantive insight into the causes of specific forms of human behavior. When debates prove persistently unfruitful, one must at least entertain the hypothesis that the participants are asking the wrong questions.

This paper is divided into four parts. Part I examines the traditional terms of the heredity-environment debate, illustrating their meaning with concrete examples. It shows that while we can, in principle, estimate the fraction of the variance in human behavior currently traceable to genetic variation, we cannot ordinarily estimate the fraction of the total variance attributable to environmental factors. Part II argues that we can understand the effects of heredity and environment more easily if we think in terms of recursive equations than if we think in terms of a traditional analysis of variance. It shows that while we can, in principle, estimate certain parameters of the reduced-form equations that include both genotype and exogenous environmental factors, we cannot estimate the parameters of structural equations that also include endogenous environmental factors which depend partly on genotype. Part III analyzes the widespread but fallacious assumption that genetic explanations of human variation are likely to be exclusively physical explanations while environmental explanations are likely to be social, illustrating the argument with a discussion of racial and sexual differences. Part IV discusses the kinds of policy implications that can be drawn

---

* Direct all correspondence to: Christopher Jencks; Department of Sociology; Northwestern University; Evanston, IL 60201.

I am indebted to Miriam Flacks, Arthur Goldberger, Robert Hamblin, John Loehlin, and Sandra Scarr for helpful criticisms of earlier versions of this paper, one of which was presented at the August, 1979, meeting of the American Sociological Association.

from heritability estimates, arguing that they are very limited.

I. *The Traditional Approach*

The traditional approach to the "heredity-environment" question uses language in a way that is easily misunderstood. If we want to understand what this approach can and cannot tell us, an example is helpful.

Suppose that a farmer alternates two genetically pure strains of corn in the same field over many years, that Strain A's average yield is 1,120 bushels, that Strain B's average yield is 880 bushels, and that the standard deviation of the annual yields over the entire period is 200 bushels. Population geneticists usually describe such findings using the following terms:

(1) Each strain's expected yield under this range of environmental conditions is called its *genotypic value* (or simply its *genotype*). In the absence of a change in environmental conditions, each strain's expected yield is simply the mean of past yields. The genotypic values are thus 1,120 bushels for Strain A and 880 bushels for Strain B.

(2) The variance of the genotypic values under this range of environmental conditions is called the *genetic variance*. Since Strains A and B have equal frequencies in this example, the genetic variance is $(1{,}120-1{,}000)^2 (0.50) + (880-1{,}000)^2 (0.50) = 14{,}400 = 120^2$.

(3) The observed yield in a given year is the *phenotypic value* (or simply the *phenotype*).

(4) The observed variance of the annual yields is the *phenotypic variance*. In this instance the phenotypic variance is $200^2 = 40{,}000$.

(5) The ratio of the genetic variance to the phenotypic variance is the *broad heritability* (or simply the *heritability*, usually denoted as h^2). In this instance $h^2 = 14{,}400/40{,}000 = 0.36$. Note that we can also think of heritability as the ratio of the between-strains variance to the total variance.

(6) The variance not attributable to genetic variation—that is, the within-strains variance—is usually called the *environmental variance*. In this case the environmental variance is supposed to be $40{,}000 - 14{,}400 = 25{,}600 = 160^2$. Using this definition, the ratio of the environmental variance to the total variance is necessarily equal to $1 - h^2$—that is, 0.64.

The first five definitions pose no serious problems. The sixth is seriously misleading, as it uses the term "environment" in a way quite different from the way in which social scientists and lay persons use it.

Suppose, for example, that the yield in our hypothetical field depends entirely on the number of grasshoppers attacking the crop and that Strain A yields more than Strain B solely because Strain A is less attractive to grasshoppers. Since most of us think of grasshoppers as part of the environment, most of us would describe this situation by saying that environmental variation explained 100% of the variance in yield. But because the environment depends partly on what strain has been planted, we would also say that genetic variation explained 36% of the environmental variation. The conventional language of population genetics, in contrast, asserts that environmental variation explains only 64% of the variance in yield. Some grasshoppers are thus "environmental"; others, "genetic."

Thus, what population geneticists and their followers call "environmental variance" is only that part of the total environmental variance not traceable to genetic variation. In technical terms, this is the "exogenous" environmental variance.

The theoretical problems we encounter when we study people are much the same as those which appear when we study corn. Among people, of course, we have no genetically "pure" strains—that is, no large groups of individuals who all have exactly the same genes. We do have pairs of identical twins, however; so in principle we can conduct experiments rather like the one just described with corn.

Suppose, for example, that we were to identify pairs of identical twins, separate them at birth, and assign them to random homes. Any correlation between such twins' phenotypic values on traits such as height, IQ, mental health, or income would have to be due either to their common genes or to their common prenatal environment. There is little evidence that variation in prenatal environment explains a significant fraction of the variation in any of these phenotypes. If prenatal environments are not important, the correlation between separated twins is, as we shall see in Part II, equal to the ratio of the genetic variance to the total phenotypic variance—that is, h^2.

In practice, of course, separated identical twins are never assigned to new homes on a random basis. But our concern at this point is not with practical problems of estimation, to which I return below, but with what inferences we could draw assuming we had "ideal" data. Suppose, for example, that some adoption agency *had* separated large numbers of identical twins and assigned them to random homes. And suppose that, after correcting for measurement error, we found a correlation of 0.40 between separated pairs' reading scores at the age of ten. What could we infer?

First, we could infer that genetic variation accounted for roughly 40% of the variation in ten-year-olds' reading scores.

Second, we could infer that environmental factors plus interactions between environmental factors and genes accounted for at least 60% of the variation in ten-year-olds' reading scores.

But we could not rule out the possibility that the environment accounted for *more* than 60% of the total variance in reading scores. Nor could we infer that equalizing children's environments would reduce variation in reading scores by only 60%. If children's genes affect their environment, separated identical twins' overall environments could be positively correlated even though the exogenous features of their environments were uncorrelated. Environment as a whole could then account for more than 60% of the variation in reading scores, even though genes accounted for 40%. In the extreme case, equalizing children's environments might eliminate *all* the variance in reading scores, not just 60%.

An individual's genes can influence his or her environment in at least two distinct ways.[1] First, genes can affect the way in which the environment reacts to the individual. Every society treats males differently from females, for example. Every society also treats those who conform to its standards of beauty differently from those who do not. Such variation in the way society treats individuals with different genes can obviously lead to variation in individual behavior, affecting traits as diverse as reading scores, mental health, and income.

Second, even when the environment is blind to a given set of genes, these genes can affect the environment individuals choose for themselves. Those with unusually light skin, for example, tend to burn more easily when exposed to the sun. Not surprisingly, they tend to avoid the sun, accentuating genetic differences in skin color. Likewise, genes seem to affect the ease with which people read, and those who read easily also read more than those who read with difficulty. This not only accentuates initial differences in reading comprehension, but leads to differences in vocabulary, general information, attitudes, and values.

Once stated, the idea that our genes affect our environment is so obvious as hardly to require demonstration. The real puzzle is why neither population geneticists nor social scientists concerned with genetic influences have taken seriously the implications of this obvious fact. The answer seems to be that these investigators have been almost exclusively concerned with ascertaining the importance of genetic variation and, for this purpose, as Roberts (1967:218) has argued, "it matters not one whit whether the effects of the genes are mediated through the external environment or directly through, say, the ribosomes." But if one is interested in the effects of *environmental* variation, it is absolutely crucial to distinguish the effects of genes "mediated through the external environment" from those mediated "through, say, the ribosomes."

---

[1] The following analysis is analogous in certain respects to that of Plomin et al. (1977), although the conclusions are very different. Plomin et al. distinguish three sources of correlation between genotype and environment: passive, reactive, and active. A "passive" correlation arises when a parent's genes affect both a child's genes and its environment. In principle, one can estimate the magnitude of this effect empirically (Jencks et al., 1972; Loehlin, 1978). The present paper is not concerned with this problem, which involves a correlation between genotype and what I here call "exogenous" environment. What Plomin et al. label a "reactive" correlation between genotype and environment arises when the environment responds differently to individuals with different genes. What they label an "active" correlation arises when individuals with different genes seek out or create different environments. This paper is concerned with "reactive" and "active" sources of correlation between genotype and environment.

Several social scientists have recognized this problem (e.g., Jencks et al., 1972; Jensen, 1976; Plomin et al., 1977), but none has tried to solve it. Instead, they have tried to define it away. Jencks and Brown (1977), for example, define the "environment" as including only those elements that vary independently of an organism's genes. This is equivalent to partitioning the variance attributable to environment into endogenous and exogenous components and restricting the label "environment" to the latter. This shorthand might be acceptable if we knew which specific elements of the environment varied for exogenous reasons and which varied for endogenous reasons. But few specific elements of the environment fall unambiguously into either category, as the case of the grasshoppers illustrated. *Estimating the explanatory power of the exogenous environment therefore sets neither a ceiling nor a floor on the explanatory power of specific features of the environment.*

The conventional, generally understood meaning of the term "environment" is extremely broad. That is precisely why both social and natural scientists find it useful. Narrowing the definition (while retaining the term itself) is certain to mislead all but the most attentive and sophisticated readers. Indeed, it is only a slight exaggeration to say that narrowing the definition of a term that has traditionally had a very broad meaning is *meant* to mislead—that is, it is meant to make one's results sound more significant than they really are. If an investigator wants a simple, descriptive label for those influences that are independent of genotype, the term "nongenetic" is ready at hand. Equating these unexplained residual influences with "environment" can only generate confusion and misunderstanding.

II. A Recursive Model

The verbal argument of the previous section suggests that we need two recursive equations to describe the relationships among heredity, environment, and phenotype. Phenotype (P) depends on Genotype (G^*) and Environment (E). Since the metrics of G^* and E are arbitrary, let us scale them so that their coefficients in an equation predicting P are both 1.00. Our first structural equation is then:

$$P = G^* + E + u_P \tag{1}$$

where u_P is an error term uncorrelated with G^* and E, attributable partly to interactions between the elements of G^* and E and partly to random errors in measuring P. Environment depends on both genotype and other factors that we label "exogenous environment" (X). Since the genetic factors that influence the environment need not be identical to those that influence the phenotype with environment controlled, we need a second genetic variable, G^{**}, to denote these genetic factors.

Our second equation is then:

$$E = G^{**} + X + u_E \tag{2}$$

where u_E embodies interactions between the components of G^{**} and X.[2]

Substituting from Equation 2 into Equation 1 we obtain the reduced form estimate of P:

$$P = G^* + G^{**} + X + u \tag{3}$$

where $u = u_P + u_E$. Setting

$$G^* + G^{**} = G \tag{4}$$

---

[2] We can test for interactions between the overall values of G and X by examining the relationship between pair means and pair differences in samples of genetically related pairs reared apart. If we find such interactions we can usually eliminate them by rescaling G and E. Interactions between *specific* genes and *specific* environments (that is, between the components of G and X) may, however, also make $s^2_{u_E} > 0$. Such interactions need not produce any systematic relationship between pair means and pair differences (Jencks and Brown, 1977).

we can rewrite Equation 3 as:

$$P = G + X + u. \qquad (5)$$

As we shall see, heritability estimates are concerned with the relationship of G and X to P. Yet we habitually discuss them as if they were estimates of the relationship of G* and E to P. The two sets of relationships are obviously quite different.

Suppose, for example, that we want to estimate the amount of phenotypic variance among individuals who have the same environment—that is, the same value of E. The exact value may vary if there are interactions between the components of G and E, but if we ignore the error variance our best estimate is Var (G*). Conventional heritability estimates assume the value is Var (G)—that is, the phenotypic variance with only the exogenous environment controlled. Yet taking the variance of both sides of Equation 4, and rearranging, shows that:

$$\mathrm{Var}(G^*) = \mathrm{Var}(G) - \mathrm{Var}(G^{**}) - 2\mathrm{Cov}(G^*G^{**}). \qquad (6)$$

We ordinarily expect $r_{G^*G^{**}}$ to be positive. Equation 6 therefore indicates that as a general rule Var(G*) is smaller than Var(G), perhaps much smaller. Conventional heritability estimates thus overestimate the phenotypic variance among individuals with identical environments.

If we want to know how much phenotypic variance there is among individuals with identical genotypes, we are in somewhat better shape. The exact value may again vary if there are interactions between the components of G and X, but if we ignore the error variance our best estimate is Var(X). As we shall see, conventional heritability estimates provide fairly plausible estimates of Var(X).

In most instances, however, we do not want to know the phenotypic variance among individuals with identical genotypes. We want to know how much we could reduce the phenotypic variance if we equalized everyone's environment. The conventional estimate is Var(P) − Var(G). The correct estimate is Var(P) − Var(G*). Since Var(G*) is ordinarily smaller than Var(G), conventional procedures usually underestimate the impact of equalizing environments.

The magnitude of the foregoing biases depends on how much genotype affects environment. Since we have no working definition of "environment" and no way of saying when two individuals have the same environment, we cannot hope to devise general procedures for estimating the bias. The most we can hope to do is assess the impact of genes on those specific features of the environment we can actually define and measure.

While it is in principle possible to estimate the overall phenotypic variance due to genes (Var(G)), data limitations mean that we must treat existing estimates with extreme caution. To see why this is so, it helps to review the conventional procedures for making such estimates. Imagine a sample of collateral relatives: identical or fraternal twins, full siblings, half siblings, or cousins. Suppose we sort these relatives into pairs and record the data on each pair twice, reversing their order. Let us denote the phenotype for the first member of each pair as P and the phenotype for the second member as P′. This makes $s^2_{P'} = s^2_P$, $s^2_G = s^2_{G'}$, $s^2_E = s^2_{E'}$, $r_{GE} = r_{G'E'}$, $r_{GE'} = r_{G'E}$, and so forth. It also means that:

$$P' = G' + X' + u' \qquad (5')$$

Now suppose we measure all phenotypes as deviations from the sample mean. If we then multiply Equation 5 by Equation 5′, sum over all pairs, divide by the number of pairs, and assume that the relatives do not influence one another, so that the correlations involving u and u' are 0, we get:

$$\mathrm{Cov}(PP') = \mathrm{Cov}(GG') + \mathrm{Cov}(XX') + 2\mathrm{Cov}(GX') \qquad (7)$$

or in slightly different notation,

$$r_{PP'}\, s^2_P = r_{GG'}\, s^2_G + r_{XX'}\, s^2_X + 2r_{GX'}\, s_G s_X \qquad (8)$$

If we collect data on genetically related individuals reared apart, and if their exogenous environments prove uncorrelated, $r_{XX'} = r_{GX'} = 0$. Rearranging then gives us:

$$\frac{s^2_G}{s^2_P} = \frac{r_{PP'}}{r_{GG'}}. \qquad (9)$$

If the separated pairs are identical (MZ) twins, $r_{GG'} = 1$ and $s_G^2/s_P^2 = r_{PP'MZA}$, where the subscript MZA represents MZ twins reared apart. If the separated pairs are fraternal (DZ) twins or ordinary siblings (S), genetic theory tells us that $r_{GG'} \sim 0.5$ and $s_G^2/s_P^2 \sim 2r_{PP'DZA} \sim 2r_{PP'SA}$.[3] If the separated pairs are half-siblings (HS), $r_{GG'} \sim 0.25$ and $s_G^2/s_P^2 \sim 4r_{PP'HSA}$.

Unfortunately, we have no data on separated collateral relatives other than identical twins and full siblings, and even these data cover only one phenotype of interest to sociologists—namely, IQ scores. Furthermore, Taylor (in press) shows that, for existing samples of separated identical twins, exogenous environments tend to be positively correlated (that is, $r_{XX'} > 0$), though one can perhaps identify subsamples in which this is not true. Internal evidence also suggests that $r_{XX'}$ exceeds zero in the only well-documented study of separated siblings (Freeman et al., 1928). Finally, the phenotypic variance for separated relatives tends to be smaller than for the general population. This could be because of restrictions on the genetic variance, the exogenous environment variance, the covariance between genotype and exogenous environment, or some combination of the three.[4] Generalizing from these samples to any larger population is therefore difficult.

Because data on separated collateral relatives is so poor, most heritability estimates rely heavily on relatives reared together. To estimate s_G^2 or s_G^2/s_P^2 from relatives reared together, we need two kinds of relatives for whom the genetic correlation ($r_{GG'}$) differs while the exogenous environmental correlation ($r_{XX'}$) is the same. For illustrative purposes let us consider identical (MZ) and fraternal (DZ) twins. Using the subscripts *M* and *D* to denote the two types of twins, we can write Equation 8 as:

$$r_{PP'M}s_{PM}^2 = r_{GG'M}s_{GM}^2 + r_{XX'M}s_{XM}^2 + 2r_{GX'M}s_{GM}s_{XM} \qquad (8M)$$

and as

$$r_{PP'D}s_{PD}^2 = r_{GG'D}s^2_{GD} + r_{XX'D}s_{XD}^2 + 2r_{GX'D}s_{GD}s_{XD}. \qquad (8D)$$

If the phenotypic variance is the same for MZ and DZ twins, we can plausibly assume that the same is true for s_G^2 and s_X^2. Subtracting Equation 8*D* from Equation 8*M* then yields:

$$(r_{PP'M} - r_{PP'D})s_P^2 = (r_{GG'M} - r_{GG'D})s_G^2 + (r_{XX'M} - r_{XX'D})s_X^2 + 2(r_{GX'M} - r_{GX'D})s_G s_X. \qquad (10)$$

If $r_{XX'M} = r_{XX'D}$ and $r_{GX'M} = r_{GX'D}$ Equation 10 reduces to a variant of Jensen's (1967) formula:

$$\frac{s_G^2}{s_P^2} = \frac{r_{GG'M} - r_{GG'D}}{r_{PP'M} - r_{PP'D}}. \qquad (11)$$

While the assumption that $r_{XX'M} = r_{XX'D}$ is intuitively implausible, the scanty available evidence supports it (Plomin et al., 1976; Lytton, 1977; Scarr and Carter-Saltzman, in press). The assumption that $r_{GX'M} = r_{GX'D}$ may also seem problematic. But since G correlates with X′ only insofar as parental genes affect both G and X′, $r_{GX'} = r_{GX}$ for twins and full siblings. The assumption that $r_{GXM} = r_{GXD}$ seems quite reasonable. Thus, while a certain amount of scepticism about twin studies is certainly warranted (Jencks and Brown, 1977; Goldberger, 1979), they may nonetheless yield satisfactory estimates of s_G^2. If so, we can estimate s_G^2 for a relatively large number of traits that interest sociologists, including not only IQ but personality traits (e.g., Loehlin and Nichols, 1976), educational attainment (Taubman, 1976; Jencks and Brown, 1977), occupational status (Taubman,

---

[3] The value of $r_{GG'}$ is exactly 0.50 if genes have additive effects and there is no assortative mating. Assortative mating raises the expected value while interactions among genes lower it.

[4] The genetic variance is restricted to the extent that genetically advantaged parents are less likely than others to have children whom they later have to give up, or that genetically disadvantaged children are more likely than others to end up in orphanages, where mortality is extremely high. The environmental variance is restricted to the extent that adoption agencies avoid placing children in unfavorable environments. The covariance is restricted insofar as adoption agencies are less successful than nature in matching children's genotypes to their adoptive parents' phenotypes. (For evidence on this last point see Jencks et al., 1972.)

1976; but see Corcoran and Jencks, 1979), and earnings (Taubman, 1976).[5]

Since twin studies depend on the assumption that $r_{XX'_M} = r_{XX'_D}$, a cautious investigator would want to check such results using other kinds of collateral relatives. The most obvious possibility is to compare full siblings reared together to adopted siblings reared together. Adopted siblings are harder to locate than twins, and data on adopted siblings are not available for anything like as wide a range of phenotypes. Indeed, IQ is virtually the only phenotype for which we have data on adopted children.[6] Furthermore, the phenotypic variance of IQ among adopted pairs is usually lower than among natural pairs. Since it is not obvious whether this is attributable to restriction of the genetic variance, of the exogenous environmental variance, or of the covariance (see footnote 4), we cannot use the same procedure for comparing natural and adopted siblings that we used for identical and fraternal twins. Indeed, without a convincing explanation for the discrepancy in phenotypic variances, comparing covariances or correlations from samples of natural and adopted siblings cannot tell us either the genetic variance or the heritability for either group.

In theory, we might do better if we compared full siblings and half siblings reared together. In order to minimize discrepancies in $r_{XX'}$, we could look exclusively at pairs in which the fathers had been absent while the children were growing up. Such data are not currently available, but they would not be hard to collect, since such sibships are quite numerous.

For a still fuller picture, we would want to utilize data not only on collateral relatives but also on parents and offspring (see, e.g., Loehlin, 1978, and the sources cited there).

Despite the many practical difficulties that arise when we try to estimate the genetic variance of human traits, such estimates are clearly feasible, *in principle*. These estimates are only feasible *in practice*, however, when we use a reduced-form model that includes only genotype and exogenous environment. Models that include the environment as a whole are hopelessly underidentified. Thus, while we can, in principle, draw conclusions about the importance of genes in explaining variation in human attributes and behavior, we cannot draw comparable conclusions about the importance of environment. This is hardly surprising. We have a reasonably clear idea of what a gene is and how genetic correlations arise among relatives. We have no clear idea what "environment" is and no theory for predicting environmental correlations among relatives or anyone else.

III. The "Genetic=Physical and Environmental=Social" Fallacy

While population geneticists have ignored the implications of genes' affecting the environment partly because they are almost exclusively concerned with demonstrating the importance of genetic variation, regardless of the mechanisms involved, most have also assumed that genes were, in fact, far more likely to operate "directly through, say, the ribosomes" than to have effects "mediated through the environment."

---

[5] While twin samples usually yield IQ variances comparable to those for the general population, this is not necessarily true for other traits. Taubman's (1976) twin sample, for example, has less variable earnings than representative samples of white men in the same age range. This could be because their genes are less variable, because their environments are less variable, or because their genes are less correlated with their environments. The relationship of s_G^2 or h^2 in Taubman's sample to s_G^2 or h^2 for other populations is thus murky.

[6] The best recent studies are by Scarr and Weinberg (1978) and Horn et al. (1979). For reviews of earlier studies see Munsinger (1975) and Kamin (1978).

[7] Of course, neither racial nor sexual identity depends exclusively on genes. Individuals with male genes can define themselves as female, and vice versa, and in some cases others accept these definitions, especially if the individuals in question have undergone sex change operations so that their anatomy conforms relatively closely to the norm for the sex with which they identify. Likewise, skin color and facial features do not completely determine an individual's racial identity. Many an individual's physical appearance does not conform clearly to any prevalent racial stereotype. In these cases, observers usually infer racial identity from contextual evidence, such as the racial identity of the parents (if known), the individual's self-reported racial identity, and other factors. These ambiguities do not, however, affect the validity of my basic argument.

Such a bias is not surprising among physical scientists. More surprising is its almost universal acceptance among social scientists. Consider the case of IQ scores. Having assembled evidence that genetic variation contributes to phenotypic variation in IQ, psychologists have hardly even bothered to ask themselves *how* this comes about. Both hereditarians and environmentalists have simply assumed that if genes do, in fact, affect IQ, they do so by affecting the development of the central nervous system—particularly the brain. No one has spelled out this view in detail, since it would have to be built entirely on conjecture. But the physical form of the model is implicit in innumerable arguments, the most obvious example being the argument that high heritabilities "explain" the failure of environmental manipulations to alter IQ scores—a point to which I will return below. Most environmentalists have accepted this model. As a result, they have felt obliged to argue that genes do not affect IQ scores—or at least not much. It does not seem to have occurred to them to concede that genes might be important and then to argue that genes could exert their influence primarily by determining a child's environment. Given the environmentalists' extraordinary ingenuity in explaining away evidence that genes are important in the first place, their failure to argue that genes may exert their impact largely through the environment is puzzling.

Environmentalists do, of course, make this argument in specific instances, without recognizing the argument's general implications. Consider the case of sex differences. The debate about sex differences in mathematical achievement, for example, is usually portrayed as a debate about whether these differences are "genetic" or "environmental" in origin. From the viewpoint of population genetics, however, such differences are genetic *by definition*. Individuals who inherit two X chromosomes (females) differ from those who inherit only one (males). For the population geneticist, it "matters not one whit" whether this is because the chromosomal difference produces internal physiological differences that affect behavior or because the chromosomal difference produces visible external differences that lead society to treat the two sexes differently. Either way, the *ultimate* cause of sex differences is genetic. The "heredity-environment" debate is only about proximate causes or, if one prefers, about the nature of the intervening variables through which sex affects behavior. What we label the "hereditarian" position is the view that these intervening variables are physical. What we label the "environmental" position is the view that they are social.

Debate about racial differences uses these same misleading labels. The "hereditarian" explanation of racial differences in IQ scores, for example, assumes that the genes which influence skin color and facial features either exert a direct effect on the central nervous system and brain or, more plausibly, that they tend to be associated with other genes that exert such effects. The "environmental" explanation assumes that skin color and facial features determine an individual's racial identity which then affects the way society treats an individual and perhaps also the way an individual treats himself or herself. Just as in the case of sex differences, both sides agree that genes have important effects on test performance. Disagreement is about proximate causes and focuses on whether these proximate causes are physical or social.[7]

Why have environmentalists never generalized their explanations of sex and race differences to other genetic determinants of human behavior? Admittedly, it is hard to think of other physical traits that affect the way society treats an individual to the extent that race and sex do. (Physical beauty probably has comparable effects in extreme cases, but few cases are extreme.) Genes need not, however, affect appearance in order to affect an individual's environment. In an industrial society, for example, a student with an aptitude for reading will typically be assigned to quite different environments from a student who has trouble reading. These environmental differences will then generate differences in a wide range of phenotypes, ranging from political attitudes to personal income.

While environmentalists have been slow to recognize the general implications of their analysis of racial and sexual differences, many hereditarians have implicitly done so. Those who estimate the heritability of human traits almost invariably separate respondents by race. Many separate respondents by sex as well. They then estimate heritabilities within racially and sexually homogeneous subpopulations. The naive observer might wonder why hereditarians do this for race and sex but not for other characteristics that depend largely on genotype—such as height, physical appearance, weight, eye color, or blood type. The obvious answer is that if an investigator were to pursue this approach to its logical conclusion, he or she would end up with a series of genetically homogeneous subsamples. Within these subsamples, phenotypic variation would by definition be nongenetic in origin, and h^2 would be zero. But why, then, have hereditarians treated race and sex as "special?" Why not treat race and sex just like other genetic sources of phenotypic variance? If we were to draw a random sample of identical twins, separate them at birth, and assign them to random families, for example, all pairs would be of the same sex, and virtually all pairs would end up being treated as if they were of the same race. If sex or race affected a given phenotype, they would contribute to resemblance between separated pairs and increase the estimated value of h^2. Why, then, have hereditarians generally preferred to estimate h^2 within racially and sexually homogeneous samples?

One possible answer is that heritabilities could vary by race and sex. But heritabilities could also vary for many other subpopulations. They could be higher in urban than in rural environments, or higher in high SES homes than in low SES homes, or vice versa. There is no theoretical reason to expect the same heritability in *any* two subgroups. Indeed, heritabilities are likely to vary from family to family, depending on whether the family allocates its resources so as to accentuate genetic differences or offset them.

A second possible reason for estimating heritabilities separately by race and sex is the implicit recognition that pooling races and sexes will make the effects of race and sex genetic as a matter of definition. Many investigators are uncomfortable with this assumption. They also assume, however, that race and sex are atypical. The argument of this paper is that they may, in fact, be quite typical. Given our almost complete ignorance of how genes affect the phenotypes that interest social scientists, the assumption that the genes determining race and sex are different from all other genes is premature, to say the least. The assumption that race and sex operate in the same way as other genes—or in the same way as each other—is equally premature. We simply do not know enough at present to draw any sensible conclusions about how most genes exert their effects.

The foregoing argument suggests that, instead of unconsciously equating genetic effects with physical effects and nongenetic effects with social effects, we should recognize that these two dimensions are independent of one another and think in terms of a two-by-two table. Table 1 presents an illustrative classification of phenomena affecting test performance.

Cell I covers what have traditionally been regarded as "genetic" explanations of test score variance—that is, explanations that not only invoke genes as an exogenous cause of variation but also emphasize endogenous physical factors in accounting for genes' effects on test performance. Phenylketonuria (PKU) is an example. PKU is a genetic disorder that prevents an individual from metabolizing phenylalanine. If an individual consumes

Table 1. Sources of Variation in Test Performance

Proximate Endogenous Cause	Exogenous Cause	
	Genetic	Nongenetic
Physical	(I) PKU	(II) Lead Poisoning
Social	(III) Sexism	(IV) Language spoken at home

a normal diet, this disorder significantly lowers his or her expected IQ. If this were the whole story, PKU would fit neatly into Cell I. But because we know how to identify PKU and how to treat it—by drastically reducing the amount of phenylalanine in an affected individual's diet—the effect of the relevant gene now depends on the environment. If a child is born in a place that routinely screens for PKU, the gene's effect will often be minimal. Otherwise, the effect will usually be substantial. For today's children, then, the effects of the relevant gene interact (in the formal statistical sense) with socially determined variations in medical care.

Cell II covers sources of variation that have no genetic basis but involve variation in physical rather than social environment. Lead poisoning, which can lead to brain damage, is an example. Again, there is some ambiguity in the classification. While the immediate causes and effects of lead poisoning are physical, the probability of exposure depends on a wide range of social influences. It also depends on skin color. Thus, one could plausibly argue that lead poisoning belongs in Cell IV as well as Cell II.

PKU and lead poisoning illustrate an important general point about Table 1. While we habitually think of certain determinants of test performance as physical "rather than" social, virtually all physical determinants of test performance are themselves to some extent socially determined. Allocating a phenomenon to Cell I or II must therefore be understood not as a denial that social variation leads to phenotypic variation but as an assertion that social factors exert their effects physically.

Cell III covers test-score variations whose exogenous cause is genetic variation but whose proximate endogenous cause is variation in individuals' social environment. "Sexism" is an example. If parents thought it more appropriate for girls to spend their time reading books than for boys to do so, girls might well end up with larger vocabularies than boys. The exogenous cause of this difference would be genetic variation between the sexes. The immediate endogenous cause would be variation in time spent reading.

Cell IV covers sources of variation in test performance that are causally independent of both genes and physical conditions—though they will, of course, often be correlated with both. An individual's native language is an obvious, almost trivial, example. Such differences are the meat and potatoes of social science, so detailed comment should not be necessary.

Cells II, III, and IV cover source of variation in test performance that people commonly label "environmental." Yet these three sources of variation have very different effects on h^2. If most of the variance in a given phenotype derives from Cells II and IV, h^2 will be low. If most of the variance derives from Cells I and III, h^2 will be high. Yet neither hereditarians nor environmentalists are interested in the combined importance of Cells I and III vs. Cells II and IV. They are interested in the importance of Cell I relative to the combined importance of Cells II, III, and IV. What they care about, in other words, is the explanatory power of the "environment," broadly construed, not the explanatory power of "heredity," broadly construed. The two questions have only the most limited relationship to one another. Furthermore, the question we can answer, namely the explanatory power of heredity, is the question we seem not to care much about.

IV. Policy Implications of Heritability Estimates

Geneticists often argue that while genes do not "determine" phenotypes, genes do "set limits" on phenotypes. No sensible person would deny this: genes may not determine height, for example, but they do seem to ensure that nobody will be less than six inches high or more than sixteen feet high. The question, however, is whether these limits are narrower for traits with high heritabilities than for traits with low heritabilities. If the effects of genotype and environment are additive, as virtually all analysts assume, the answer is that h^2 tells us nothing about the relevant limits. In Equation 1, for example, the value of G sets no limit whatever on the value of P. For any value of G there is another value of E that will yield the desired value of P. If a \$100 increase in expenditures raises a pupil's IQ by one point, for example, the additive model in Equation 1 implies that a school board can raise any pupil's IQ to any level it deems desirable, simply by spending more money. This would remain true even if \$100 raised IQ by only 0.01 points.

Most readers will rightly reject models with such unlikely implications. The point, however, is that we know virtually nothing about the limits genes place on specific phenotypes and that knowing h^2 adds nothing to our knowledge of these limits. There is no empirical evidence that genes place narrower limits on traits with high heritabilities than on traits with low heritabilities. Indeed, it is not clear how one would go about comparing the limits genes place on different traits.

In practice, most hereditarians (e.g., Jensen, 1969) make a somewhat different argument. They claim that if a trait or behavior has a high heritability this means that environmental changes are unlikely to have much effect *relative to the existing variance*. This argument is sometimes correct—but only if two rather stringent conditions hold. First, the change in question must affect only exogenous environmental factors, not partially or completely endogenous ones. Second, the change in question must involve reducing or redistributing existing environmental variation rather than increasing such variation or changing conditions that are now uniform.

Since IQ and scholastic achievement have been the focus of most policy inferences based on h^2, they provide a useful illustration of these principles. First, suppose that a school board is trying to assess the likely impact on IQ of abolishing elementary school tracking, and that local studies have shown that IQ has a heritability of 0.80—the highest figure cited by most hereditarians. What can the board infer about the effects of tracking? The answer is, "Nothing." The reason is that children are assigned to elementary school tracks partly on the basis of IQ. Track assignment therefore depends on genotype as well as exogenous environmental influences, and variations in IQ that derive from track assignment contribute to the genetic as well as the phenotypic variance of IQ. Thus, $1 - h^2$ sets no limit on the proportion of variance due to tracking.

For simplicity, suppose that half of all entering first graders have IQ's of 90 while the other half have IQ's of 110, and that all high IQ children are assigned to the "fast" track while all low IQ children are assigned to the "slow" track. Suppose that at the end of six years all children in the fast track have IQ's of 115, while all those in the slow track have IQ's of 85. Abolishing tracking would then seem likely to reduce the variance of IQ by more than half. But the *ratio* of genetic to phenotypic variance—that is, h^2—would remain constant. This would be equally true if at the end of six years all children in the fast track still had IQ's of 110 while all those in the slow track had acquired IQ's of 105, or if all IQ's were the same at the end of school as at the start. An h^2 of 0.80 is thus compatible with tracking helping the advantaged, helping the disadvantaged, or having no effect whatever. Since changes in the effect of track assignment have no necessary effect on h^2, the value of h^2 tells us nothing about the likely effect of track assignment.

In practice, of course, initial IQ is not the only determinant of track assignment. Race, sex, SES, and other factors play a part. But all these factors are also likely to be proxies for an individual's genes. Unless we can determine the degree to which track assignment depends on genes, both directly and indirectly, we cannot say to what extent IQ variance generated by tracking is utilmately traceable to genetic factors. As a result, $1 - h^2$ sets no upper bound on the amount of variance generated by tracking.

In some cases, however, we may have reason to believe—or at least be willing to assume—that some feature of the environment does not vary in response to genotype. Suppose, for example, that a school system assigns pupils to schools on a random basis. Variations in school quality are then clearly exogenous. If $h^2 = 0.80$ in this community, variations in school quality cannot explain more than 20% of the total variance in IQ. Yet even this "fact" has relatively weak implications for policy.

Suppose, for example, that the school board is currently spending $2,000 per pupil and is considering a proposal to set up an experimental school that would cost $3,000 per pupil. Suppose, in addition, that the board knows the standard deviation of current expenditures per pupil is $335 and that it is willing to accept hereditarian claims that $h^2 = 0.80$ for IQ. The equation relating IQ to expenditures (Y), other exogenous environmental influences (X), and genotype (G) is:

$$\mathrm{IQ} = \mathrm{B_1Y} + \mathrm{X} + \mathrm{G} + u. \qquad (12)$$

Taking variances,

$$\mathrm{Var(IQ)} = \mathrm{B_1^2Var(Y)} + \mathrm{Var(X)} + \mathrm{Var(G)} + \mathrm{2Cov(GX)} + \mathrm{2Cov(YX)} + \mathrm{2Cov(YG)} + \mathrm{Var}(u). \qquad (13)$$

But Cov(YX) = Cov(YG) = 0 under random assignment, and by assumption Var(G) = 0.80 Var(IQ). It follows that:

$$\mathrm{B_1^2 Var(Y)} < \mathrm{0.20Var(IQ)}. \qquad (14)$$

Since $\mathrm{Var(IQ)} = 15^2$, and $\mathrm{Var(Y)} = 335^2$,

$$\mathrm{B_1}^2 < \frac{(0.20)(15^2)}{335^2} \qquad (15)$$

or

$$\mathrm{B_1} < 0.020. \qquad (16)$$

A $1,000 increase in expenditures per pupil cannot, in short, increase IQ by more than 20 points. Such an increase may, however, have no effect whatever. This range of possible effects is so wide as to be virtually useless. If the board were to assume a lower value of h^2, its uncertainty about the likely effect of spending more money would be even greater.

A heritability of 0.80 would provide slightly more guidance if the school board were considering eliminating an existing source of variation in IQ. If pupils are currently assigned to schools on a random basis, the school board can be reasonably certain that making all schools equally good will not reduce the variance of IQ by more than $1 - h^2 = 20\%$. Again, however, this is a fairly weak conclusion. If equalizing school expenditures reduced the variance of IQ by 20%, this would be appreciable. If the reduction were 2%, the results would be hard to detect.

Another statistic usually tells more about the likely effect of environmental changes than $1 - h^2$ does. This statistic, which is often denoted as e_c^2, measures the proportion of the phenotypic variance in a given trait attributable to "common" environment. The "common" environment includes those aspects of the environment that are common to all children reared in the same home—or sometimes to twins in the same home—regardless of genotype. Since such features of the environment are exogenous, e_c^2 is always less than $1 - h^2$.

Like $1 - h^2$, e_c^2 tells us nothing about the potential effects of new environments, such as a new school that might spend more than existing schools. It tells us only about the effect of reducing or eliminating existing environmental differences among families. But many policy proposals consist, in essence, of providing all families with advantages currently enjoyed by the privileged. If e_c^2 is initially large for a given phenotype, successful efforts along this line can be expected to substantially reduce the total variance of the relevant phenotype and greatly improve the relative position of the disadvantaged. If e_c^2 is small, such egalitarian policies are unlikely to have dramatic effects on the total variance, though the effects on the very worst-off may still be fairly sizable.

The simplest way to estimate the explanatory power of common environment is to calculate the covariance between pairs of adopted siblings reared together ($\mathrm{CovPP'}_A$). But since adoptive parents are more advantaged than the average parent in terms of SES and other unmeasured traits, they probably provide children with less-variable environments than natural parents do. The covariance between adopted siblings is therefore likely to be less than the variance explained by common environment among natural siblings, though not necessarily much less.

Another way of estimating the common environmental variance is from twin data. Using the notation of Section III, the common environmental variance is equal to Cov(XX′) and $e_c^2 = \mathrm{Cov}(XX')/\mathrm{Var}(P)$. Subtracting $\mathrm{Var}(G_M)$ from both sides of equation 8*M* gives us:

$$\mathrm{Cov}(PP'_M) - \mathrm{Var}(G) = \mathrm{Cov}(XX'_M) - \mathrm{Cov}\,(GX'_M). \tag{17}$$

If the variances for twins are comparable to that for the general population, dividing both sides of equation 17 by Var(P) yields:

$$r_{PP'M} - h^2 = e_c^2 - \frac{\mathrm{Cov}(GX'_M)}{\mathrm{Var}(P)}. \tag{18}$$

Since Cov(GX′) can hardly be negative,

$$e_c^2 \leq r_{PP'M} - h^2. \tag{19}$$

Equation 19 thus sets an upper bound on e_c^2, while $\mathrm{Cov}(PP'_A)/\mathrm{Var}(P)$ sets a lower bound. If Cov(GX′) is sizable (because parental genotype has a sizable effect on X as well as G), or if the restriction of range on X is sizable for adopted children, the discrepancy between these two estimates may be quite large. Nonetheless, for policy purposes e_c^2 is usually a more useful statistic than h^2.

The foregoing arguments underline the point made at the outset of this paper: until we know *how* genes affect human behavior, the mere fact that they account for a substantial fraction of the currently observed variance in such behavior is not very instructive. But suppose we could describe the way in which genes exert their influence in precise detail. Suppose, moreover, that we found genes exerted their effects on, say, IQ largely by affecting children's physical development which, in turn, affected their mental "capacity" in the traditional, mechanical sense. Would this be grounds for pessimism about raising the IQ's of those who now score low? The answer is no, for two reasons.

First, as already noted, if we can identify environmental variables that explain even a small fraction of the existing variance in IQ, we may well be able to increase the means of these variables so that they fall outside their current range. This can increase the IQ's of certain individuals—or of the whole population—substantially. It is true, of course, that most interventions aimed at raising IQ have negligible long-term effects. But this discouraging fact merely dramatizes our inability to identify—or perhaps change—the environmental determinants of IQ. If we *could* change the environmental determinants of IQ, there would be no necessary limit on our ability to raise IQ's, even if h^2 were 0.80. The earlier discussion of school expenditures illustrates why this is so.

Second, even if genes affect IQ by physical rather than social means, this does not necessarily imply that IQ is hard to change. On the contrary, physically determined traits are often *easier* to change than socially determined ones. The case of PKU is illustrative. Once we know how PKU works, we can reduce the damage relatively easily. Few social interventions are anything like as effective. Eliminating "cultural deprivation," for example, would require an egalitarian revolution that is opposed by a large fraction of the population. Limiting the effects of PKU requires only a change in diet—which is cheap, effective, and relatively easy to implement.

There is, of course, no ironclad guarantee that a full understanding of the way in which genes affect a given trait will allow us to intervene effectively in the process. Nevertheless, if we compare past medical and social interventions, it would be hard to argue that social interventions had generally been either easier to implement or more effective than medical ones. Thus, the idea that high heritabilities should be grounds for pessimism is doubly suspect, first because genes may currently exert much of their effect through the environment and, second, because even if genes do not now exert their effect through the environment, environmental changes may nonetheless alter genes' effects in the future.

REFERENCES

Corcoran, Mary, and Christopher Jencks
1979 "The effects of family background." Pp. 50–84 in Christopher Jencks et al., Who Gets Ahead? New York: Basic Books.

Freeman, Frank N., Karl J. Holzinger, and Blythe C. Mitchell
1928 "The influence of environment on the intelligence, school achievement, and conduct of foster children." 27th Yearbook of the National Society for the Study of Educa-

tion. Part I, pp. 103–217. Bloomington, IN: Public School Publishing Co.

Goldberger, Arthur
1979 "Heritability." Economica 46:327–47.

Horn, Joseph, John Loehlin, and Lee Willerman
1979 "Intellectual resemblance among adoptive and biological relatives: the Texas adoption project." Behavior Genetics 9:177–207.

Jencks, Christopher, Marshall Smith, Henry Acland, Mary Jo Bane, David Cohen, Herbert Gintis, Barbara Heyns, and Stephan Michelson
1972 Inequality. New York: Basic Books.

Jencks, Christopher, and Marsha Brown
1977 "Genes and social stratification." Pp. 169–233 in Paul Taubman (ed.), Kinometrics: The Determinants of Economic Success within and between Families. New York: North Holland.

Jensen, Arthur
1967 "Estimation of the limits of heritability of traits by comparison of monozygotic and dizygotic twins." Proceedings of the National Academy of Science 58:149–56.
1969 "How much can we boost IQ and scholastic achievement?" Harvard Educational Review 39:1–123.
1976 "The problem of genotype-environment correlation in the estimation of heritability from monozygotic and dizygotic twins." Acta Geneticae Medicae et Gemellologiae 25:85–99.

Kamin, Leon
1978 "Comment on Munsinger's review of adoption studies." Psychological Review 85:194–201.

Loehlin, John
1978 "Heredity-environment analyses of Jencks' IQ correlations." Behavior Genetics 8:415–36.

Loehlin, John, and Robert Nichols
1976 Heredity, Environment, and Personality: A Study of 850 Sets of Twins. Austin: University of Texas Press.

Lytton, H.
1977 "Do parents create, or respond to, differences in twins?" Developmental Psychology 13:456–59.

Munsinger, Harry
1975 "The adopted child's IQ: a critical review." Psychological Review 82:623–59.

Plomin, Robert, Lee Willerman, and John Loehlin
1976 "Resemblance in appearance and the equal environments assumption in twin studies of personality." Behavior Genetics 6:43–52.

Plomin, Robert, J. C. DeFries, and John Loehlin
1977 "Genotype-environment interaction and correlation in the analysis of human behavior." Psychological Bulletin 84:309–22.

Roberts, R. C.
1967 "Some concepts and methods in quantitative genetics." In Jerry Hirsch (ed.), Behavior Genetic Analysis. New York: McGraw-Hill.

Scarr, Sandra, and Richard Weinberg
1978 "The influence of 'family background' on intellectual attainment." American Sociological Review 43:674–92.

Scarr, Sandra, and L. Carter-Saltzman
In press "Twin method: defense of a critical assumption." Behavior Genetics.

Taubman, Paul
1976 "The determinants of earnings: genetics, family, and other environments: a study of white male twins." American Economic Review 66:858–70.

Taylor, Howard
In press The IQ Game: A Methodological Inquiry into the Heredity-Environment Controversy. New Brunswick: Rutgers University Press.

BRINGING THE FIRMS BACK IN: STRATIFICATION, SEGMENTATION, AND THE ORGANIZATION OF WORK*

JAMES N. BARON AND WILLIAM T. BIELBY
University of California—Santa Barbara

American Sociological Review 1980, Vol. 45 (October):737–765

This essay examines the shift toward "structural" explanations in recent studies of inequality. After reviewing this body of research and some of its shortcomings, we examine its theoretical underpinnings, comparing "structuralist" perspectives on work organization derived from institutional economics and neo-Marxism to more orthodox accounts based on neoclassical and "industrialism" theories. This discussion suggests areas where the different perspectives overlap and diverge. We conclude that work arrangements within the firm and their trend are the focus of most "structural" perspectives on positional stratification; thus, empirical studies grounded at the organizational level are more likely to inform current debates about the "structure of work" than is the growing body of research about structural effects on individual attainment or covariation among industrial/occupational characteristics. Toward that end, an agenda for future research is outlined, focusing on three aspects of work organization: (a) the units which comprise the structure of work and the dimensions underlying economic segmentation; (b) the effects of sectoral differentiation on technical and administrative arrangements within firms; and (c) temporal changes in how enterprises organize production. We provide some illustrations of the kinds of empirical data and research hypotheses required to link research on segmentation and stratification more closely to studies of organizations.

INTRODUCTION

Since Blau and Duncan's exemplary study of *The American Occupational Structure* (1967), stratification research has focused almost exclusively on the determinants of individual socioeconomic achievement. While researchers might acknowledge that attainment occurs within specific structures (e.g., Duncan, 1968:680–2), critics have charged that this body of work ignores the dependence of attainment processes on the structure of positional inequality—that is, the "empty places" in the economy and the relations among them (e.g., Sørenson, 1975; Burawoy, 1977). Indeed, many investigators have maintained that structure is both more important than and logically prior to individual attainment (e.g., Wright and Perrone, 1977; Beck et al., 1978; Snyder et al., 1978; Kalleberg and Griffin, 1980). In recent years, empirical research from explicitly structuralist perspectives has sought to redress the imbalance. Much of this research purports to show that individual achievement is a function of structural factors such as class (Wright and Perrone, 1977; Wright, 1978; Kalleberg and Griffin, 1978; 1980), authority (Kluegel, 1978; Robinson and Kelley, 1979; Wolf and Fligstein, 1979), organizational size (Stolzenberg, 1978), or labor market sector (Stolzenberg, 1975; Bibb and Form, 1977; Beck et al., 1978; Hodson, 1978). We refer to this emerging body of literature as the "new structuralism" in stratification research.

Unfortunately, the "new structuralists" have yet to offer a coherent and consistent representation of the structurally contingent nature of attainment processes (Pfeffer and Ross, 1979; Zucker and Rosenstein, 1980; Granovetter, forthcoming). In our view, this reflects two

---

* Direct all correspondence to: James N. Baron; Department of Sociology; University of California; Santa Barbara, CA 93106.

This essay is based largely on papers we presented to the National Science Foundation Conference on "Labor Market Structure and Socioeconomic Stratification," University of Georgia, March, 1980. The authors were supported in part by a grant from the National Science Foundation (DAR 79-24905). J. Edmund Phillips of the California Occupational Analysis Field Center has generously and patiently helped us acquire and interpret the data on work organization within firms. Skillful research assistance was provided by Karen V. Hansen. We gratefully acknowledge the comments of Robert P. Althauser, Neil D. Fligstein, and Roger O. Friedland on earlier drafts of this paper.

basic limitations which characterize this research. First, an explicit conceptualization of the structure which underlies and organizes attainment is rarely offered. Instead, we find numerous descriptive interpretations loosely consistent with countless variations upon notions of dual economy, internal labor markets, segmented labor markets, and Marxian class theory, incorporating insights from historiography, institutional economics, organizational theories, and more orthodox approaches to social inequality. Second, most analysts would acknowledge that the relationship between structure and attainment is intimately tied to the *organization of work*—that is, the interplay between technical and administrative imperatives on the one hand, and relations among people, positions, and objects within the workplace on the other. Yet competing perspectives on work organization have not been examined directly in their empirical studies.

We will examine some of these limitations in detail and propose a heuristic scheme for representing how various levels and units of work organization figure in the new structuralism. This discussion inspires a research agenda focusing on *firms* as a missing link in contemporary structural stratification research. We suggest that work is structured at five levels of social organization: societal, institutional, organizational, role, and individual. Correspondingly, social inequality may be examined at five hierarchically connected units of analysis: economy, industry or sector, firm, job, and individual. Within this context, we propose that firms link the "macro" and "micro" dimensions of work organization and inequality. Within firms, labor is priced and allocated, techniques of production are arranged and implemented, and power is organized and executed. Social and economic mobility are achieved within and between economic organizations. Societal and institutional forces impact jobs and workers through the organizational constraints faced by firms; conversely, actions taken by work establishments affect their macro environments. Indeed, the central role of firms in bridging structure and process at various levels of social and economic organization has been emphasized by theorists aligned with such disparate schools of thought as structural functionalism and Marxism (cf. Parsons and Smelser, 1956; Edwards, 1975). Yet the organization of work within (and between) firms has been almost universally ignored in current stratification research. Studies aimed at specifying and testing various perspectives on how work roles are organized (and change over time) have been few and far between, relative to efforts aimed at enhancing our ability to predict individual socioeconomic success. Accordingly, we begin by critically reviewing the substance and method of some representative structural stratification studies.

ADDUCING "STRUCTURE" FROM ITS EFFECTS ON ATTAINMENT: YOU CAN'T GET THERE FROM HERE

The new structuralists have increasingly recognized the need to understand the "structure of work" and how it changes over time *before* studying its effect on individual achievement (e.g., Wright and Perrone, 1977; Beck et al., 1978; Kalleberg and Griffin, 1980). However, the tendency of analysts to confound two questions, "How is work organized?" and "Who gets ahead?" may help explain certain anomalies and ambiguities which plague most recent empirical efforts.

1. Are occupations and industries relevant units of analysis? Occupations and industries appear almost universally in structural stratification research, either as the loci of worker mobility, or in the guise of structural variables influencing earnings, job satisfaction, and the like. Yet the significance of occupational and industrial designations for work organization and individual achievement depends, in turn, on how one thinks work is structured in the first place.[1]

---

[1] This criticism is thus equally applicable to studies which factor analyze industrial and/or occupational characteristics in order to derive sectoral typologies. We suggest below that such operationalizations and measurements of "structure" are less informative than studies which reflect more meaningful distinctions in work organization at the *firm* and *job* levels.

According to most labor market theorists (e.g., Piore, 1975), occupations are relevant for labor market processes only in certain types of craft and administrative employment in which movement occurs predominantly *between* firms and where work performance is regulated by professional norms disseminated through occupational associations, unions, ideologies, and related institutions (Caplow, 1954). Even if occupations *are* relevant units, schemes for classifying them do not necessarily reflect the distinctions among work roles which those who apply such typologies intend to capture. Braverman (1974) and Conk (1978), for example, have convincingly demonstrated that early classification schemes lacked an adequate conception of the technical division of labor among occupations, coming to depend heavily on the very same sociocultural divisions which occupations were presumed to index. Furthermore, while some analysts have defended occupations on the grounds that detailed categories are homogeneous with respect to most salient dimensions of work (e.g., Temme, 1975; Snyder et al., 1978), some evidence suggests that occupations may display far more internal heterogeneity along crucial dimensions than many investigators have acknowledged (e.g., Blaug, 1970; Bridges and Berk, 1974: 213; also see Spenner 1977:70). Others, such as Gordon (1972), claim that the degree of heterogeneity within occupations is itself a part of the historical puzzle to be unravelled by empirical research, rather than a convenient assumption to be imposed by fiat (see Baron, forthcoming). Finally, most theories which buttress the new structuralist research emphasize *jobs* (rather than occupations). Taken together, these arguments militate against analyses of changes in occupational distribution and aggregate occupational characteristics as a means of characterizing changes in the structure of work.

Similarly, industries may be differentially heterogeneous with respect to certain aspects of work. As in the case of occupations, industry boundaries capture meaningful distinctions in work organization only in certain instances. For example, government analysts studying boilermaking in Wisconsin and California for the United States Employment Service in the early 1960s could not even agree whether boilermaking constituted an industry (much less what its defining criteria were) due to the constellation of technological changes, diverse union activities, and administrative arrangements among enterprises engaged in manufacturing and installing boilers (internal memoranda, California Occupational Analysis Field Center, United States Employment Service). Indeed, even when industrial distinctions *are* clear, government experts often find it difficult to determine whether a specific establishment belongs to a particular industry (U. S. Bureau of the Census, 1975: 1.11).[2] Nevertheless, many analysts have assumed industries to be relatively homogeneous with regard to technological and administrative arrangements (e.g., Bluestone, 1970; Spilerman, 1977; Kalleberg et al., 1980; Kaufman et al., 1980; Tolbert et al., 1980). However, numerous organizational studies indicate substantial heterogeneity on these same dimensions within firms and even within departmental units of a single establishment (e.g., Child and Mansfield, 1972; Grimes and Klein, 1973; Billings et al., 1977; Comstock and Scott, 1977), suggesting that the assumption of industrial homogeneity—while no doubt convenient for those analyzing industry-level data—is differentially sensible for certain segments of the industrial structure.

Blaug and his colleagues (1967:18–9) neatly summarized several of these points over a decade ago, criticizing researchers for overlooking

> the crudity of definition of [occupational categories] and the variety of interpretations placed on them by different firms in different industries, and the fact that technology itself permits a variety of educational inputs to the same occupation. The unexamined assumption in most of the literature on manpower

---

[2] Even at the most disaggregated level, industries differ markedly in the extent to which their principal product is produced within the industry and in the degree of product specialization which characterizes firms in the industry (see, for example, the "coverage" and "specialization" ratios provided for four-digit S.I.C. categories in United States Bureau of the Census, 1975: Table 3).

> planning is the stability of occupational classifications, if not across, then certainly within, the various sectors of the economy. . . .
>
> It may be doubted, however, whether the census concept of occupations is sufficiently well-defined to be useable as a research variable. At any rate, it should not be assumed, to say the least, that an analysis of the job content of a particular occupation in one industry would constitute an adequate basis for generalization about all similarly titled occupations in other industries. Indeed, the same argument may well be applied to individual firms within a given industry . . . [since] we know that firms within an industry vary widely in the technical processes they have adopted to produce an identical output.

2. *How do independent variables represent structures and processes?* Not only may the relevant units of work organization depend on one's conception of "structure"; the specification and measurement of how attainments are determined may also depend on structural context. To be sure, some investigators have been mindful of the circularities that arise when characteristics of the work force are included in deriving sectoral typologies which are used, in turn, to explain individual attainment (cf. Hodson and Kaufman, forthcoming; Horan et al., forthcoming). Much less attention, however, has been devoted to specifying precisely how the parameters of the attainment process are dependent on structural influences. Researchers typically disaggregate models of socioeconomic achievement along some structural dimensions (e.g., Wright and Perrone, 1977; Stolzenberg, 1978; Beck et al., 1978), ignoring the possibility that very different factors (not just different regression slopes) may shape allocation and attainment in various structural contexts, or that the interpretation of some parameter may depend on the structure itself. As Granovetter (forthcoming) notes, such processes as income determination reflect at least three types of influences: (a) workers' attributes and choices; (b) employers' decisions and characteristics of jobs; and (c) the process which matches (a) and (b). Structural variables introduced in analyses of individual achievement typically fall under the rubric of (b) above. Yet, as Granovetter argues, the relative *importance* of individual vis-à-vis role attributes, in determining rewards, *itself* depends on how jobs and workers are matched [i.e. (c)], a process which presumably also taps the influence of "structure." For example, Granovetter and others (e.g., Stinchcombe, 1959) argue that features of *roles* are expected to determine the allocation of prestige, income, and other rewards in bureaucratic (core?) enterprises; contrariwise, in the small, decentralized firms characteristic of the so-called periphery, the mechanisms postulated by human capital theorists which link outcomes to *individual* choices and talents are more plausible.

Even if one could legitimately specify the same regression equation for each structural segment, hypothesizing that slopes differ as a function of some contextual factor, the same variable may still represent distinct social and economic processes within each context. Researchers include education, job tenure, and other personal characteristics, for example, in models disaggregated by some structural typology, typically referring to these as "human capital" variables (e.g., Bibb and Form, 1977; Beck et al., 1978; Pfeffer and Ross, 1979; Aldrich and Weiss, 1980).[3] This illustrates precisely the ambiguous notion of "structure" motivating such empirical efforts and may help explain certain anomalous results which have plagued the new structuralism. Many dualists (including some radical segmentation theorists) acknowledge that the secondary labor market and competitive sector approximate the concept of "the market" in neoclassical models of how investments in human capital shape individual career patterns (Doeringer and Piore, 1971; Gordon, 1972; Edwards, 1979). Meanwhile, managerial discretion, bureaucratic imperatives, and objective role requirements embodied in detailed job descriptions purportedly flourish in center firms, suggesting that there education should serve as a credentialling device or means of cultural control—in light of the ongoing political struggles among competing coalitions within such enterprises' (Collins, 1979). Thus, the effect of education may *mean* something very different in each sector.

---

[3] Mark Granovetter reminded us of the theoretical implications of this syntactical convention.

Some researchers have attempted to examine such differences, for example by distinguishing between the effects of years of schooling and the attainment of credentials (Beck et al., 1978; cf. Jencks et al., 1979: chap. 6). However, such efforts still assume that the same specification applies within each sector. More importantly, they do not entirely account for the fact that sectoral differences in the distribution and effects of schooling are neither as substantial nor as systematic as predicted. Data reported by Hodson (1978:452), for example, reveal few striking differences between the monopoly and competitive sectors in the distribution of educational attainment. Bibb and Form (1977:988) found that blue-collar workers in the core had less education than those in periphery industries. Furthermore, Pfeffer and Ross (1979) found little evidence of sectoral differences in the *effect* of education on income and status attainment, regardless of whether segments were delineated in terms of industrial concentration or predominant organizational control strategy ("market" versus "normative" regulation). Some data analyzed by Beck et al. (1978:713) also do not seem to support dualists' claims concerning the influence of schooling and credentials on achievement across sectors.[4] Finally, Kalleberg et al. (1980) found drastically different patterns of association between education and each of four dimensions of industrial segmentation. They also found virtually no evidence that aspects of industrial structure condition the effects of education on income.

In short, we have highlighted the ambiguous definitions and representations of structure in models of individual attainment and have pointed to some anomalous empirical results which reflect those shortcomings.

3. Could these anomalies reflect the new structuralism's inattentiveness to organizational arrangements? The inconsistent findings regarding schooling and segmentation may reflect the cleavage between "good" and "bad" jobs *within* core firms, a split that only modestly differentiates workers in terms of their educational attainment. If primary jobs are largely protected from market competition, and secondary jobs are for the most part homogeneous, then the relationship between education and income within such firms will reflect simply the difference in average education and income between these two classes of labor. In contrast, incremental returns to schooling or vocational training may be attainable through apprenticeship, licensing, credentialling, or career lattices within certain bureaucratic, professionalized, and small craft firms in periphery industries. Consequently, the surprisingly strong relationship, there, between education and income may reflect a more finely graded association between job requisites and rewards.[5]

These speculations illustrate the fact that empirical associations between structure and attainment can be explained only by attending to organizational arrangements within firms. Yet industrial sectors have been defined in a manner which has severed their links to "organizations and organizational properties . . . [even though] it is in organizations that wage and occupational decisions are made, for the most part, and in which mobility and stratification occur" (Pfeffer and Ross, 1979:18). These organizational bases of stratification are largely obscured at the industrial level. Pfeffer's (1977) informative study of wage attainment among students from business schools indicated that the relationship between social origins and destinations is stronger (1)

---

[4] For example, Hauser's (1980) reanalysis of their data indicates that sectoral interactions with respect to the education coefficients are by no means systematic (see his Table 2); indeed, if nonearners are excluded from the analysis, the sectoral interactions vanish altogether (but cf. Beck et al., 1980).

[5] Researchers have also often overlooked the possibility that reliance on formal credentials, internal labor markets, and the like may constitute *alternative* mechanisms of organizational control available to center firms, rather than companion arrangements. Gordon and Thal-Larsen (1969:274), for example, present some interesting data regarding the tradeoff between educational credentials and internal job ladders as means for recruiting (and controlling?) managers within firms.

in staff than in line positions, (2) in small than in large firms, and (3) in nonmanufacturing than in manufacturing enterprises. Indeed, Pfeffer found these three dimensions of organizations which mediate attainment outcomes to be only weakly intercorrelated. According to these results, to the extent that center firms are administratively intense, they should rely on ascriptive background characteristics—rather than education or meritocratic criteria—in promoting within-staff positions. However, ascription is hypothesized to figure less prominently in large firms and manufacturing establishments, presumably in which bureaucratic and standardized operating procedures enhance the utility of educational credentials in allocating and rewarding employees. The fact that income returns to education in the core do not dwarf those observed in the periphery may thus reflect the manner in which distinct dimensions of organizations have been overlooked in empirical examinations of industrial segmentation.[6]

Once we appreciate the fact that the meaning of a variable (such as education) may depend on its "structural context," and that its effects may be masked in analyses which are inattentive to *organizational* processes, we can perhaps make sense of other anomalous findings in the structural stratification research literature. For example, while dualist and segmentation theorists have underscored sectoral differences in the distribution of job tenure and its consequences for income determination, the empirical results have generally failed to support these hypotheses. Pfeffer and Ross (1979) discovered no sectoral differences in the effect of men's tenure with current employers on their income or status attainment. Indeed—while certain "core" industries (e.g., metal, petroleum, and railroad transportation) are characterized by high median levels of employee tenure with the firm—workers in agriculture, leather products, and certain public utilities, as well as females in the paper-products industry, are among those with the longest tenure with their employers (United States Department of Labor, 1975:Table E), despite the fact that such forms of employment are frequently banished to the "periphery" sector (see Zucker and Rosenstein, 1980:Appendix A). As in the case of education, the significance of job tenure for attainment outcomes has been confounded in empirical studies which fail to link job tenure to its specific occupational and organizational bases. Kalleberg et al. (1980) demonstrate the importance of attending to such links. For example, they find that tenure with a firm *is* strongly related to establishment size and industrial concentration but *not* to the economic scale (profitability) of an industry, arguing that the former variables capture the prevalence of internal labor market arrangements within *firms*. Tenure was also negatively related to the proportion of an industry's goods purchased by the state. The authors explain this result by observing that industries selling goods to the state tend to employ higher proportions of professionals and craftspersons, who are more likely to view the occupation, rather than the employer, as a source of career advancement. Thus, they are also likely to change firms more frequently.

Organizationally informed analyses may help clarify yet another anomaly in the empirical literature relating segmentation to individual careers—namely, why unemployment would be higher in core vs. periphery industries (e.g., Beck et al., 1978). Focusing on the basis of control exercised over various classes of workers, and its relations to *types* of unemployment, Schervish (1980) demonstrates that in the primary sectors *jobs* are unstable, while, in the secondary sector, *workers* exhibit employment instability. Schervish's Marxian orientation, emphasizing the basis of control exercised over various class segments, implicitly reflects an organizational focus, underscoring, again, the importance of firm-level processes in mediating societal and industrial forces.

---

[6] Like Stolzenberg (1978) and others, Gordon and Thal-Larsen (1969) found an overall relationship between size of establishment and reliance on education and other formal requirements in recruiting and promoting employees. However, they suggest that the relationship is actually somewhat spurious, obscuring the specific organizational bases of the observed association. They argue that large firms also tend to be more unionized and to be branches or corporate headquarters (rather than single unit establishments), both of which serve to enhance their reliance on formal credentials.

In sum, by ignoring the organizational bases of stratification processes, "structural attainment research" has perhaps overstated or obfuscated sectoral differences in the distribution and consequences of such factors as education, job tenure or internal labor markets, and unemployment (also see Gordon and Thal-Larsen, 1969:325–31; Cassell et al., 1975; Director and Doctors, 1976; Steinberg, 1975; Leigh, 1976a; 1976b; 1978; Spilerman, 1977:583–4; Edwards, 1979:169). In our view, more frequent or elaborate attempts to disaggregate models of socioeconomic achievement, or to factor analyze industry-level data, are unlikely to improve our understanding of how work roles are structured and shape individual biographies. Rather, we suggest that debates about the structure of work can best be advanced by clarifying the areas of conflict and complementarity among competing perspectives on work organization, specifically regarding the role of firms in mediating the links between social structure and processes at macro and micro levels. Below we examine how various accounts of work and inequality differ in emphasizing specific levels of social organization, units of analysis, and relations among them. Having placed these perspectives within such a heuristic framework, we assess where they overlap and diverge, motivating a research agenda which provides more content and coherence to the emerging body of structural theory and research on work organization and inequality.

STRATIFICATION AND WORK: LEVELS OF SOCIAL ORGANIZATION AND UNITS OF ANALYSIS

Research on work and inequality spans levels of organization from individual behavior (e.g., mobility) to the dynamics of entire social systems (e.g., class relations and technological change). As a heuristic, Table 1 arranges five levels of social organization from macro to micro: societal, institutional, organizational, role, and individual. It also presents corresponding units of analysis in studies of work: economy, sector, firm, job, and worker. The table is *not* a conceptualization of work structure, but the centrality of firms within it does suggest that the micro and macro forces affecting work are played out within the organizational arena of the firm. Further, it provides a means for imposing some semblance of order on the myriad empirical findings and conceptual ambiguities one encounters in the new structuralist research.

Table 1. Levels of Social Organization and Units of Analysis in Accounts of Work Organization

Level of Social Organization	Unit of Analysis
Societal	Economy
Institutional	Sector (market, industry)
Organizational	Firm
Role	Job
Individual	Worker

Orthodox attainment studies, according to our scheme, focus on individual workers as units of empirical analysis, but their theoretical concerns embrace both individual and societal levels of organization. At the macro level, the modernizing influence of technical differentiation is seen as undermining ascriptive sources of inequality that persist from an earlier era (e.g., Blau and Duncan, 1967:429). Consequently, an important concern of research at the individual level is the degree to which nonmeritocratic sources of intergenerational status persistence are attenuated by the technical demands of a "postindustrial" society (e.g., Featherman and Hauser, 1978:308–11).

As we noted in the previous section, empirical research by the new structuralists generally examines whether supraindividual units of analysis influence (or dominate) the process of individual achievement. The empirical design in such studies is straightforward (essentially an analysis of covariance), and the relations between units of analysis quite obvious: processes defined over individual units are contingent upon classification according to some more macro unit. But the correspondence between units of analysis and *levels of social organization* is by no means self-evident in this research. For example, Marxian class categories denote ownership (and control) at the societal level. But explanations of class differences in attainment processes typically refer to firm-level (organizational) decisions implemented by capitalists and managers regarding the distribution of power and opportunity, in turn affecting job (role) conditions and the relations among jobs and workers. In short, Marxian variants of the new structuralism (e.g., Wright and Perrone, 1977; Wright, 1978; Kalleberg and Griffin, 1978; 1980; Robinson and Kelley, 1979; Aldrich and Weiss, 1980) pose class mechanisms at the societal level which influence individual attainment (worker units) through imperatives acting upon institutions (industries and sectors), organizations (firms), and roles (jobs).[7] But the organization of work within firms and the attributes and relationships among jobs are often bypassed in empirical applications of Marxian concepts to stratification concerns.[8]

Robinson and Kelley (1979:38), for example, sought to assess the veracity of Marx's and Dahrendorf's accounts of the "more conflict oriented aspects of stratification stemming from the hierarchical organization of work" in the United States and Great Britain. It is curious that the debate among these two perspectives—which centers around the relationship between technical and organizational imperatives in the workplace—should be reduced to regression models with dummy variables indicating how many subordinates one has and whether or not one has a supervisor or employer. According to Dahrendorf (1959), an emergent hierarchy of *technical skill* within the working class ostensibly makes obsolete Marx's rendition—which hinges on the gradual substitution of capital for labor and the concomitant deskilling, homogenization, and unification of the proletariat. In short, the two theorists disagree about both the overall trend in work organization *and* the relationship between technical, administrative, and political imperatives in the workplace. Had Robinson and Kelley focused on how technology and social relations of production within the firm combine to shape worker attitudes and attainments—and how the role of each has changed over time—perhaps they might have achieved a closer correspondence between their empirical results on the one hand and the levels of organization and units of theoretical concern on the other.

The correspondence between units of analysis and levels of social organization is even more ambiguous in empirical research motivated by non-Marxian "dualist" and "segmentation" perspectives. Institutional economists have variously differentiated jobs, firms, labor markets, and industries into segments or sectors. Averitt's (1968) widely cited work, for example, distinguishes between center and periphery *firms* based on their organizational resources and their resulting ability to manipulate institutional environments. While many center firms are located in certain key *industries* (p. 66), "it is economic size, not industrial location, that defines firms in the center economy." Bluestone (1970), Piore (1975), Edwards (1979), and others differentiate *labor markets* into primary and secondary segments. According to these schemes,

---

[7] Thus, like adherents of the "industrialism thesis," many Marxists derive and analyze micro level processes from transformations presumed to operate at macro levels. Of course, a complete Marxian account would encompass the dialectical relations between individual and societal level relations. However, the feedback effects of individual workers on the seemingly deterministic control imperative have only recently been considered in neo-Marxian accounts of work organization (e.g., Aronowitz, 1978; Burawoy, 1980).

[8] To be sure, Marxists recognize that the "logics" of capital accumulation and class struggle (at the macro level) permeate institutions, organizations, and roles in affecting the life chances of individual workers. Industrial structure is seen as increasingly bifurcated between monopolistic and competitive sectors, reflecting the concentration and centralization of capital, the political power exercised by industrial elites through interlocking corporate directorates, and State intervention on their behalf (Baran and Sweezy, 1966; O'Connor, 1973). Struggles within and among unions at the industrial and firm level are seen as fragmenting the working class (Aronowitz, 1973), as are efforts by owners and managers to deskill and homogenize work at the enterprise and job level (e.g., Braverman, 1974; Marglin, 1974; Stone, 1974). But empirical efforts to link Marxian theory to stratification outcomes rarely model the effects of such intervening forces.

the pricing and allocation of labor and the linkages among jobs both within and between firms differ markedly across labor market segments. Empirically, jobs in primary markets are most often situated in center firms and key industries, while jobs in secondary markets occur disproportionately in periphery firms and competitive (O'Connor, 1973) or unconcentrated (Bluestone, 1970) industries.

In short, theories of dualism and segmentation suggest quite a number of conceptual and empirical links between sectors, firms, jobs, and the labor markets through which individual workers obtain employment, but schemes for differentiating jobs, firms, and industries are by no means interchangeable. Unfortunately, the new structuralists have, for the most part, been inattentive to *differences* among perspectives in the levels and/or units emphasized, and the mechanisms posited to link them. Instead, as we have suggested, the dualism and segmentation literatures have served to rationalize various ad hoc schemes for assigning workers to sectors, most often simply on the basis of crude industrial classifications. Far from revealing how work arrangements are structured at various levels of social organization—a principal concern of these institutional approaches—such research efforts at best provide a "reduced form" representation of the structure of work and its effects on individual attainment. Below, we examine more closely the causal imagery of the dominant perspectives on work organization, relying on our heuristic scheme to determine the areas of consensus and disagreement among them.

CLARIFYING THE ISSUES: CONFLICT AND COMPLEMENTARITY AMONG PERSPECTIVES

Though often overlooked, a coherent statement of how work is arranged at different levels of organization can be found (at least implicitly) in conventional studies of achievement. The thesis of "industrialism," which underlies most status attainment research, does not ignore "structure," but differs radically from the stance of its structuralist critics. Consider how Blau and Duncan (1967:429) conclude:

> The basic assumption underlying these [empirical] conjectures is that a fundamental trend toward expanding universalism characterizes industrial society. Objective criteria of evaluation that are universally accepted increasingly pervade all spheres of life and displace particularistic standards of diverse ingroups, intuitive judgments, and humanistic values not susceptible to empirical verification. The growing emphasis on rationality and efficiency inherent in this spread of universalism finds expression in rapid technological progress and increasing division of labor and differentiation generally, as standards of efficiency are applied to the performance of tasks and the allocation of manpower for them. The strong interdependence among men and groups engendered by the extensive division of labor becomes the source of their organic solidarity, to use Durkheim's term, inasmuch as social differentiation weakens the particularistic ingroup values that unite men in common bonds of mechanical solidarity.

It is clear from this passage that the industrialism thesis infers micro level phenomena from macro level processes. As we suggested above, at the *societal* level social differentiation generates change, including transformations of work. Indeed, the application of technical rationality eventually makes work—specifically, occupational roles—the most salient characteristic defining individuals' positions in the social structure (Blau and Duncan, 1967:6; Treiman, 1977:chap. 1). Technical proficiency and individual performances increasingly become the bases for allocating persons to those roles.

According to this thesis, *organizations* are simply microcosms of society (Blau and Scott, 1962). As societal differentiation progresses inexorably, organizations approach the Weberian ideal-typical bureaucracy in which calculable rationality is applied to the organization of work (Parsons and Smelser, 1956:113; Treiman, 1977:9–11). Blau et al. (1976:39–40), for example, succinctly suggest how industrialism operates at the level of the firm:

> As education expands further and growing proportions of the labor force acquire technical and professional competence, the adaptation of computer technologies to pro-

> duction and office work will undoubtedly take these higher skills into account. While it is possible to automate many operations without raising the skills workers use, a highly trained labor force makes this increase unnecessary. Hence, one can expect the growing automation of factories to raise the level of skill and responsibility of workers.

Correspondingly, this perspective views the *institutional* structure of industrial sectors as a reflection of how the technical division of labor has advanced historically from agriculture to manufacturing to information-based services as the center of economic and social organization (Clark, 1951; Kerr et al., 1964; Lenski, 1966; Bell, 1973).

The dominant perspective on work organization among economists—neoclassical micro theory—has its own version of the technological imperative. Neoclassical theories of the firm also assign primacy to rationalization and technological progress, but are more clearly grounded at the organizational level of analysis. The firm's production function expresses a technical relationship: the amount of output produced by different combinations of factor inputs. Given the prices of output and factor inputs, profit-maximizing enterprises employ each factor until the marginal contribution to firm revenue no longer exceeds the marginal cost of using the factor. The mix of factors, including different types of capital and labor, is solely a function of their relative productivities and prices. The supply of job relevant individual abilities, preferences, and human capital endowments combines with the derived demand for labor generated by firms' productive efforts, thereby linking individuals, roles, and organizations (Addison and Siebert, 1979). At the institutional level, the market structure of an industry is derived either from technical relationships at the organizational level (e.g., declining long-run average costs) or from exogenously imposed conditions such as governmental franchises (Hirschliefer, 1976:275). Similarly, the societal level impinges on the theory in the guise of aggregate "general equilibrium," constrained by external arrangements effected by government, geography, international trade, and the like.[9]

In attempting to redress the individualistic flavor of industrialist and neoclassical approaches to inequality and work organization, the new structuralist research has often drawn on insights from Marxian, institutional, and/or organizational frameworks. However, too little attention has been focused on how (or whether) the latter cohere into a unified alternative conceptualization of the structural bases of stratification. Accordingly, we think it prudent to compare how these perspectives embrace each level of social organization.

We have already noted how Averitt (1968) defines the dual economy by distinguishing between center and periphery firms. The linkage with institutional and societal levels is established by what might be called "dualist historiography" (Berle and Means, 1932; Chandler, 1962; 1977; Shepherd, 1975; Caves, 1980). It is claimed that firms implemented mass and process technologies and initiated new strategies of coordination and control as an adaptive response to changing markets. But not all firms experienced the combination of market, technical, and administrative preconditions which made it possible to adopt the new form of corporate structure—that is, decentralized, vertically integrated, multidivisional, diversified, etc. (Chandler, 1962). The requisite conditions occurred most often in certain key industries (Averitt, 1968), resulting in a new institutional order of industrial *sectors*. Galbraith (1967; 1973) and others have linked these developments to the *societal* level by emphasizing the emergence of the planning function within large corporations and the state, which, in turn, shapes the national (and international) economy and polity.

---

[9] While the application of technical rationality is central to *both* neoclassical and industrialism approaches, the former perspective is less "structural," predicated upon the rational maximization behavior of individual economic agents. Institutional and societal phenomena are simply aggregations of that behavior. In contrast, social differentiation "drives" the industrialism thesis at the societal level, as the irreversible progress of the division of labor is played out at more micro levels of organization.

Neo-Marxian and dualist accounts stress some of the same processes and mechanisms, but the perspectives vary in assigning priority to different levels of social organization (Gordon, 1972:85–96). As we have indicated, neo-Marxists typically start with class relations and capital accumulation at the societal level and derive implications for micro levels of social organization. However, dualists begin at intermediate levels—role, organization, sector—from which societal and individual level consequences can be derived. Theories of *internal labor markets* link the organizational, role, and individual levels in dualists' accounts of work. Job ladders define promotion, skill, and supervision hierarchies within the firm, and the need for minimal labor turnover determines which individual attributes become the criteria for selection into entry level jobs (Thurow, 1975). Small periphery establishments without internal labor markets supposedly have job structures which differ markedly from those found in center firms—few well-defined promotion lattices, fewer levels of supervision, and relatively low skill requirements. Individual attributes such as "trainability" and long-term commitment to the firm should be less important criteria for selection into jobs in the secondary labor market (Freedman, 1969; 1976; Doeringer and Piore, 1971; Gordon, 1972; Harrison, 1972).

While both neo-Marxian and dualist explanations are often posed in opposition to those of neoclassical economics or attainment research (Gordon, 1972), it is not always clear how (or if) the different views directly conflict. For example, the processes represented by "deskilling" and the "control imperative" might be viewed simply as elaborations of the calculus of marginal substitution whereby capital replaces labor (or vice versa). Such a neoclassically inspired perspective would explicitly recognize the costs of administrative uncertainty introduced whenever labor is employed as a factor in production. Similarly, neoclassical economists have long recognized the consequences of skill specificity for training, labor turnover, and advancement, which are not inconsistent with the notion of internal labor market structures (Oi, 1962; Becker, 1964; Rosen, 1974:438; Wachter, 1974).

Perhaps the most conspicuous and significant disagreements among these diverse perspectives concern the causes of the historical development of work. While both the industrialism thesis and neoclassical theory regard contemporary economic organization as an outcome of increasingly rationalized and efficient productive arrangements, neo-Marxists argue that the logic of capitalism embodies a social control imperative within that technical division of labor. Task differentiation and the substitution of capital for labor are not simply matters of engineering and economics, confined to the realm of production functions and the calculus of marginal costs and revenues. Rather, division of labor and automation of production are implemented in a manner which also maximizes the control of capitalists and managers over the production process. The control imperative brings about the separation of work conceptualization from execution, a homogenization of labor which artificially differentiates work in a manner only loosely related to skill differentials (Braverman, 1974), the implementation of internal labor markets to placate and regulate workers in monopoly industries (Stone, 1974), and creation of a reserve army of the unemployed to help "discipline those inside the factory gates" by posing a threat to their job security (Gordon, 1977:94).[10]

Neo-Marxian accounts thus typically regard work arrangements as outcomes of the logic of capital in which social control is inextricably linked to accumulation and profit extraction. While couched within a broader historical view, Marxian theory also suggests specific expectations regarding *contemporary* work organization. Note, for instance, that the logic of job fragmentation and hierarchy fetishism implies the existence of more highly differentiated and authoritarian work structures within certain firms than could reasonably be ascribed to considerations of technical efficiency. Thus, disagreements among perspectives largely reflect different views of the decision-making process *within firms*, especially with regard to the adoption and management of innovations in the workplace. For example, some argue that internal labor markets are established for reasons of technical efficiency (Doeringer and Piore, 1971), while others emphasize their functions as mechanisms of social control (Stone, 1974). Approaching employment relations in terms of the features and costs of "transactions" within the organization, Williamson (1975:72–81) derives an explanation of the same structural arrangement which entails *both* its efficiency and control aspects. Hence, even if the structuralists were to subject the internal workings of organizations to detailed empirical scrutiny, it is not clear that their findings would indicate which of several structural perspectives is most viable. As David Gordon (1972:129–30) notes, it is difficult to discover employer motivations regarding the design of work; one must rely on indirect inferences. In addition to historical case studies he suggests that

---

[10] There seems to be an inconsistency between radical and internal labor market perspectives on this matter. Doeringer and Piore (1971) and others maintain that firm-specific skills are a crucial feature of jobs in internal labor markets, and that nonentry level jobs are insulated from wage competition (Thurow, 1975). In that situation the "reserve army" poses little threat, since its members are not substitutes for the specifically trained workers protected by internal job ladders. Thus, workers as well as employers in center firms may develop an interest in maintaining organizational arrangements which in effect export the threat of the reserve army to the periphery.

> one might try to infer from comparative studies some evidence about the direction of employer choices. Given firms which employ the same technologies in different social settings, one might try to adduce something about the consequences for job design of alternative social objectives.

Further, Gordon underscores the irony that most analyses which have sought to contrast Marxist, dualist, and more orthodox accounts have relied primarily on macro level data, even though these perspectives can most readily and meaningfully be contrasted by analyzing firms and their internal workings. Much of the new structuralism has been inattentive to the different models of administrative decision making which underlie competing perspectives. Investigators have sought to address debates about the distribution and consequences of authority, power, or social control within firms without considering the ways in which hierarchies are arranged or implemented at the organizational and suborganizational level (Wright and Perrone, 1977; Kluegel, 1978; Stolzenberg, 1978; Robinson and Kelley, 1979; Wolf and Fligstein, 1979). For example, without knowing explicitly how managers define, price, and evaluate different types of labor (including management itself) and the respective productivity of each type, it is impossible to discriminate satisfactorily between accounts of deskilling which stress utility maximization of a dominant managerial coalition on the one hand (e.g., Galbraith, 1967; Chandler, 1977) and those which emphasize capital's inherent logic of exploitation and hegemonic control (e.g., Braverman, 1974; Edwards, 1979). For this reason, it is specious to infer support for a particular account of work organization simply from evidence which is not inconsistent with competing viewpoints (cf: for example, Snyder et al., 1978; Baron, forthcoming).

These disputes are not without consequences for ongoing debates among students of structural aspects of inequality. Specifically, analyses of the structure of work and how it changes over time embody some assumptions (most often implicit ones) about *who* decides how work will be organized and what *criteria* are employed in making those choices. Thus, differences among structuralist perspectives often reduce to disagreements about what motivates those who decide on behalf of the firm.

BRINGING THE FIRMS BACK IN: ORGANIZATIONS AND THE STRUCTURE OF WORK

If firms are indeed "where the action is," then social scientists interested in the structure of social inequality should find the vast literature on complex organizations illuminating. Surprisingly, with just a few exceptions, the literature on organizational structure, technology, and social control has been largely ignored in both new structuralist empirical research and dualist theory. To be sure, organizational sociology provides an implicit or explicit rationale for the topics examined in a number of structural analyses. However, as we noted, many such studies are linked to organizational theory merely by virtue of including authority variables within individual attainment models, even though the organizational context within which authority is exercised is ignored. Indeed, almost all structural stratification research addressing "organizational" concerns focuses on the consequences of a person's location within the firm for his or her attainment (e.g., Talbert and Bose, 1977; Halaby, 1979; Rosenbaum, 1979), but not on the relationship between an individual's tasks and position on the one hand, and those of supervisors, subordinates, and co-workers on the other. In short, there is little attention to how work is actually arranged within enterprises.[11] Interfirm differences in work organization either are expressed in terms of some single dimension, such as size, or are dismissed altogether (as in studies of "industry effects").

Yet even a superficial overview of the organizational literature sensitizes one to the difficulties encountered in accurately characterizing organizational structures and processes. Concepts and measures of technology have been intensely debated (Bright, 1958; Amber and Amber, 1962; Woodward, 1965; Perrow, 1967; Hickson et al., 1969; Lynch, 1974), to say nothing of its relationship to the internal structure of the organization (Aldrich, 1972; Child, 1972; Van de Ven and Delbecq, 1974; Blau et al., 1976). Even such a seemingly straightforward concept as organizational size—which figures prominently both in theories of dualism (Averitt, 1968) and empirical applications (Bibb and Form, 1977; Stolzenberg, 1978; Aldrich and Weiss, 1980; Kalleberg et al., 1980; Kaufman et al., 1980)—has generated about as much debate (still unresolved) in the organizations literature as has "class" in the stratification literature (see Kimberly, 1976, for an overview). Furthermore, as noted earlier, the claim of structural and technological homogeneity within organizations or industries—assumed in most sociological and economic accounts of work—is inconsistent with recent research.

Despite problems of conceptualization and measurement, there are also several provocative organizational analyses of the relationship of technology and size to alternative strategies of power and dominance within the firm (Burns and Stalker, 1961; Indik, 1964; Blauner, 1964; Pondy, 1969; Child, 1973; Freeman, 1973; Ouchi, 1977), often focusing on issues strikingly similar to those raised in studies of segmentation and labor stratification. Most importantly, "profit maximization" as an administrative imperative has been criticized from a variety of theoretical standpoints. Typically, institutional economists formulate the challenge in terms of a dichotomy between models of "profit maximization" and "utility maximization" or "managerial discretion" (Williamson, 1963; 1975; Galbraith, 1967; Pondy, 1969). Radical economists (e.g., Gordon, 1972; 1976; Marglin, 1974; Edwards, 1975; 1979) juxtapose "qualitative" and "quantitative" efficiency; like dual labor market theorists, they (Gordon, 1972:129–30) suggest

> that there is a frontier of job design possibilities, given a specific technology, which promise[s] equally efficient production in output per worker terms. Employers then choose a point of production organization along the frontier which satisfies some other objectives. One can easily hypothesize, especially from the radical perspective, that employers choose along the job design frontier in order to maximize the stratification of labor and to minimize class consciousness within the potentially most dissatisfied strata.

However, based on organizational theory, one can also easily hypothesize that employers choose to pursue some other objectives in addition to (or instead of) profits. As the institutional (and some neoclassical) economists argue, managerial discretion may be applied to administrative decision making in order to maximize a utility function which includes managers' (or owners') preferences for growth, maximum sales, perquisites, stability, or even for the proliferation of management itself (see Nicholson, 1978:269–77 for an example of how neoclassical orthodoxy treats alternatives to the hypothesis of profit maximization). Alternatively, managers may "satisfice" (Simon, 1957)—that is, adopt some strategy (optimal or otherwise) which fulfills their aspirations, based on limited information. Or employer choices may perhaps affect outcomes only through a complex process of intraorganizational negotiation among potentially competing coalitions (Cyert and March, 1963; Leibenstein, 1979).

---

[11] A noteworthy exception is the research by Melvin Kohn, Carmi Schooler, and their associates (e.g., Kohn and Schooler, 1973; 1978; Miller et al., 1979) which examines the relationships between "structural imperatives of the job" and workers' psychological functioning. However, these structural imperatives (e.g., "ownership," "organizational locus," and "occupational self-direction") span several levels of social organization. Furthermore, this research has focused primarily on the causes and consequences of individual psychological "attainments" (e.g., intellectual flexibility), treating the imperatives of work as exogenous.

In short, since the distribution and dispensation of *power* within firms are central foci of competing theories of work organization and stratification, researchers interested in the structures which frame socioeconomic achievement can ill afford to continue treating firms as "black boxes" in their empirical analyses (Pfeffer, 1977; Stolzenberg, 1978; Pfeffer and Ross, 1979). Extant theory and data pertaining to the organization of work certainly preclude any definitive analyses of the relative merits of reigning structuralist perspectives. Nonetheless, a great deal could be gleaned by examining the relative veracity of competing accounts through empirical studies addressing: (1) the dimensions which differentiate work settings; (2) the relationship between organizational characteristics and linkages among jobs; and (3) how (and *why*) the structure of work roles within a firm changes over time. For example, do technical and administrative arrangements within firms reflect a cleavage between monopolistic and competitive industrial sectors? Do relations among jobs within large center firms reflect more detailed and clearly defined layers of promotion, skill, and dominance than in periphery enterprises? Do changes in work organization over time indicate the operation of an incipient control imperative—manifested in deskilling and/or homogenization of labor—which is not reducible to the marginal calculus of neoclassical micro theory?

These concerns conspicuously highlight the importance of *organizations* as the crucial entities within which work is structured and underscore the relevance of the literature on complex organizations and administration for debates about the causes, trends, and consequences of positional inequality. It may be that characteristics of individuals are easier to conceptualize and measure than are attributes of the "empty places" within the stratification system (cf. Burawoy, 1977), perhaps explaining both the emerging consensus among researchers about the need for analyses of "structure" and their lack of agreement on how to undertake them. In the following section we outline a course for future structural studies; to demonstrate the feasibility of our suggestions, we restrict our discussion of hypotheses and models to those capable of being examined with *extant data*, using examples from our own ongoing research as illustrations (Bielby and Baron, 1979; 1980).

A RESEARCH AGENDA FOR THE "NEW STRUCTURALISM"

The Contours of Dualism: Units Comprising the Structure of Work

This essay has suggested that researchers employing dichotomies (or polychotomies) of sectors, markets, occupations, industries, and the like have often devoted too little attention to *what* is being dichotomized—that is, at which levels of organization and from which units work is structured. Since the fondness of stratification researchers for achievement studies employing sectoral typologies seems to be increasing inexorably, we suggest that detailed empirical studies of how (or whether) dualism or segmentation is constituted are an essential precondition for making sense of the new structuralism. As argued above, occupations and industries are quite heterogeneous, and are of limited relevance to many of the structuralists' concerns.

Therefore, we suspect that job- and firm-level analyses can more meaningfully portray the structure of work than can the growing literature analyzing occupational or industrial data (e.g., Rosenberg, 1975; Freedman, 1976; Lowell, 1978; Oster, 1979; Kaufman et al., 1980; Tolbert et al., 1980; Wallace and Kalleberg, forthcoming).[12]

An illustration: The dualist and segmentation literatures (e.g., Averitt, 1968; Bluestone, 1970; Gordon, 1972; Edwards, 1975) suggest that the attributes of firms listed in Table 2 are among those which should differentiate economic sectors.[13] Preliminary support for the dualism hypothesis would be provided if such characteristics successfully discriminate among industries and if the latter cluster into clearly bounded sectoral groupings. Since firm-based dualism is conceptually distinct from industrial differentiation, and, given the remarkably weak consensus among several industry-based schemes for classifying sectors, we would not expect to find overwhelming evidence of dualism at the *institutional* level of analysis. Furthermore, while many of these bivariate hypotheses are plausible, we have argued in this essay that sectoral differences in work organization have been overstated.[14] For example, while dualist and segmentation approaches regard the center/periphery distinction as approximately dividing the labor force in terms of skill (see Table 2), we expect such overall differences in skill *level* to be small. Research by Blauner (1964), Bright (1958), and others indicates that the transition to highly capitalized and/or automated technologies—such as those which purportedly characterize the core—do not necessarily engender any monotonic rise in skill levels, although they do appear to increase the *variation* in workers' skills.[15]

Thus, our aim in formulating and testing these hypotheses would be neither to "prove" nor to "refute" structuralist theories of work and inequality, but rather to provide a more detailed specification of where the organization of work does and does not correspond to their predictions. Moreover, while such theories typically base their sectoral distinctions on different patterns of work arrangements, most industrial data used to derive sectoral typologies (e.g., mean assets, business receipts, state purchases) are proba-

---

[12] To study the structure of "empty places" or work roles, we need to develop methods for acquiring and analyzing data on positions rather than on individuals. Our own work in progress utilizes data describing jobs and work establishments in California collected by its Occupational Analysis Field Center for the United States Employment Service. Since the late 1950s, the Employment Service has conducted field visits of establishments in over 100 industries, obtaining complete enumerations of all jobs within firms; the administrative and technical structure of each establishment; the sex composition, educational and skill requirements, and psychological concomitants of jobs, as well as their placement within networks of promotion, supervision, transfer, and vocational preparation. "Narrative reports" furnish a wealth of qualitative information about establishments' formal and informal personnel practices; collective bargaining arrangements; corporate age, history, and structure; production processes and departmental organization; firms' relations to automation, product markets, parent companies; and related matters. A number of enterprises were also revisited some five to ten years later, providing follow-up data with which to analyze temporal changes in work organization. The empirical illustrations which punctuate this research agenda are based largely on our continuing analyses of these data. (For a full description of this data source and how key variables are operationalized, see United States Department of Labor, 1972; Bielby and Baron, 1979).

[13] With the possible exception of "production continuity," the attributes listed in Table 2 should be familiar to students of stratification and segmentation. See Blauner (1964), Meissner (1969), and Caves (1980) for representative overviews of this concept and its consequences for work organization.

[14] It should be apparent that we expect a number of these attributes to vary substantially *within* firms, even though they are frequently used as criteria for characterizing (or even defining) economic sectors. Accordingly, we expect empirical analyses of hypotheses such as those listed in Table 2 to demonstrate two things: (1) that differences among sectors in the *level* of certain attributes are modest; and (2) that industries, firms, departments, and jobs each display substantial heterogeneity not captured by current sectoral schemes.

[15] Kalleberg and his colleagues (1980) regressed occupational skill (SVP) on a series of individual demographic and work experience variables and four dimensions of economic segmentation. Net of the person-level factors, they found that while economic scale of an industry and relative state consumption of its product had positive effects on SVP, concentration had no effect, and establishment size exercised a substantial *negative* influence (also see Baron and Bielby, 1980).

Table 2. Hypothesized Distributions of Firm Attributes by Sector

Firm Attribute	Sector	
	Core	Periphery
1. Scale of operations	Large	Small
2. Vertical integration	High	Low (or absent)
3. Division of multiestablishment organization	Yes	No
4. Skill level	High	Low
5. Skill dispersion	Dispersed	Homogenous
6. Internal labor markets	Yes	No
7. Task complexity	Complex	Simple
8. Social control (mode of supervision)	Bureaucratic	Simple hierarchy
9. Collective bargaining contract	Yes	No
10. Production continuity	High	Low
11. Capital intensity	High	Low
12. Sex composition	Predominantly male	Predominantly female

bly less representative of how work is actually organized within firms than are variables such as those listed in Table 2. To test this assertion, one could compare a multidimensional configuration of industries based on such job and firm attributes against one based on the industry-level variables typically used to determine sectors.[16] If these configurations differ, then schemes in which distinctions among firms are subsumed completely at the industrial level are likely to obscure important sectoral divisions at the organizational level.

Having thus investigated "industrial dualism," segmentation among organizations could be further explored by examining the distribution of firms on the attributes listed above. For example, analyses of the covariation among firm attributes should differentiate firms in a manner consistent with the twelve bivariate hypotheses listed in Table 2. Organizationally based dualism would be evident if firms clustered into center and periphery sectors in the resultant multidimensional space. If, as Averitt argues, dualism is generated by differences among firms and the institutional structure is not strictly demarcated by industries, then a specific pattern should emerge from the analysis of industrial differentiation proposed above. First, firm attributes should discriminate moderately among industries; second, *firms* should exhibit stronger tendencies toward sectoral clustering than do industries. Furthermore, industries with ambiguous sectoral locations should be precisely those about which there is most disagreement in the various a priori schemes for assigning industries to sectors (see Zucker and Rosenstein, 1980).

Finally, a parallel analysis would discriminate firms on the basis of *job* attributes that are presumed to vary between center and periphery. For example, task specificity and complexity, promotion opportunities, bureaucratic control, and male composition have all been advanced as characteristics of "primary" jobs in center firms. Dualism at the organizational level would be supported if such job attributes differentiate firms into core and periphery locations.[17] The relative importance of each job attribute in discriminating firms would assist in evaluating various perspectives on work organization and economic dualism which, as we have seen, accord differential emphasis to specific aspects of jobs.

Institutional economists (Averitt, 1968; Doeringer and Piore, 1971; Williamson, 1975) have argued that "markets" and "hierarchies" are two different "rational" responses to the problem of organizing production; large firms have greater incentive to internalize the market through organizational rules and procedures, while similar arrangements are of

---

[16] Such configurations could be obtained using a number of descriptive multivariate statistical techniques, such as discriminant and cluster analysis; for some detailed illustrations, see Bielby and Baron (1979).

[17] Since the correspondence between core/periphery sectors and primary/secondary labor markets is far from perfect, one might find a "deviant" cluster of firms containing *both* primary and secondary production. jobs.

limited utility within small firms. If so, then this should be clearly reflected in the pattern of such results on the extent of dualism or segmentation, the crucial level(s) of analysis at which it prevails, and the dimensions of sectoral structure. Failure to detect cleavages at the industry and firm level of analysis would certainly discredit segmentation accounts. Of course, evidence of dualism or segmentation is not necessarily inconsistent with a neoclassical economic model incorporating imperfect competition and the costs of information and firm-specific training. But, unless sectoral differentiation corresponds to industry differences in capital concentration and composition, it is unclear how other patterns of "clustering" could be accounted for within a neoclassical framework.

The Impact of Dualism: Sectoral Location and the Firm's Internal Job Structure

In a recent review of directions in stratification studies, Udy (1980:157) notes how early researchers' focus on occupations and status "did not require [them] to penetrate organizations, observe the tasks and constraints of the immediate job, [or] the degree of authority and control it conveys. . . ." Despite their avowed concerns with these very phenomena, the new structuralists have done little to redress these glaring omissions. Naturally, we would not expect the analyses outlined in the previous section to reveal a crystal-clear portrait of the dual (or segmented) economy; indeed, a good deal of prior research cautions against such an expectation (Freedman, 1976; Kaufman et al., 1980; Wallace and Kalleberg, forthcoming). However, anticipating at least *some* evidence of economic segmentation, the most obvious question to be examined in turn is: what are the empirical consequences of sectoral differentiation for work arrangements within firms?

As noted above, a major dispute among rival theories of economic organization concerns "the complicated relationships among *technology, jobs, and people*" and changes in those relations over time (Gordon, 1972:89, his emphasis). In the remainder of this subsection, we suggest some illustrative analyses of those relationships and how they differ across sectors. Then we consider how historical changes in such relations can be studied.

An illustration: We focus on relationships between sectoral location and three critical dimensions of the internal job structure of firms—their internal *task differentiation* or division of labor, their *internal labor markets* or promotion ladders, and their *control mechanisms* or supervision hierarchies.

Table 3 presents the hypothesized net relationships between each of twelve firm attributes and these three dimensions of the firm's nonadministrative job structure.[18] Some of these hypotheses have been drawn directly from dualist and organizational perspectives, while others reflect our interpretations of how those accounts view technology and social control in relation to work arrangements. As our discussion throughout should indicate, we think certain of these hypotheses (e.g., the correspondence between sectoral location and the structure of internal labor markets) are either incorrect or drastically overstated. Nonetheless, we regard Table 3 as a "fair" characterization of what dualist, segmentation, and organizational perspectives imply about the determinants of task differentiation, promotion hierarchies, and control mechanisms. Notice that a majority of relationships are hypothesized to differ between center and periphery firms. A fairly stringent test of a generalized dualism hypothesis would be provided by evaluating these differences in firms with unambiguous sectoral locations. Space limitations obviously preclude reviewing and substantiating each of the 69 hypothesized relationships in Table 3, so an overview of the more important ones must suffice.

---

[18] We focus on nonadministrative (i.e., production) jobs for two reasons. Most importantly, organizational research has repeatedly demonstrated that technological and administrative forces exercise their influence most conspicuously at and around the workflow (e.g., Hickson et al., 1969; Grimes and Klein, 1973; Van de Ven and Delbecq, 1974). Additionally, these illustrations were selected from our own research, which utilizes data pertaining primarily to industry-specific (i.e., production-oriented) jobs (see Bielby and Baron, 1979).

Table 3. Hypothesized Relationships between Firm Attributes and the Structure of Nonadministrative Jobs within Firms

	Dimension of Internal Structure					
	Task Differentiation		Internal Labor Market		Levels of Supervision	
Firm Attribute	Center	Periphery	Center	Periphery	Center	Periphery
1. Task Complexity	++	++[a]	++	0	++	+
2. Skill Level	++	0	+	0	–	0
3. Skill Dispersion	++	0	+	0	+	+
4. Skill Specificity	++	+[a]	++	0	+	0
5. Scale	++	+	++	0	++	+
6. Sex Composition (% female)	–	––	––	––	–	––
7. Craft Union	++	+	++	+	+	0
8. Industrial Union	–	––	++	+	?	?
9. Production Continuity	++[b]	+[b]	?	0	inverted U	inverted U
10. Capital Intensity	++	+	+	0	+[c]	+[c]
11. Age of Establishment	0	0	+	0	0	0
12. Sector Main Effect	+	–	+	–	+	–

Net Relationships: Strong positive (negative) relationship: ++ (––)
Moderate positive (negative) relationship: + (–)
No relationship: 0
No hypothesis: ?

Notes:

[a] Much less variation is expected in the periphery; therefore the standardized effect should be much weaker in periphery firms.

[b] Perhaps inverted U.

[c] Increasing at a decreasing rate.

Also, the table does not present hypotheses relating outcome variables to interactions among firm attributes, even though they might be defensible on theoretical grounds.[19]

Task differentiation—the subdivision of work into a number of component jobs—may be a consequence of technical or control imperatives. The *technical* division of labor simply reflects the efficiency of task specialization, but differentiation as a strategy of domination should control the work force through fragmentation in bureaucratically organized firms, while "simple hierarchy" is more easily exercised over a homogeneous work force (Child, 1973; Edwards, 1975; 1979). The hypothesized positive relationships between differentiation and complexity, skill, scale, and technology variables among center firms reflect the operation of the technical imperative, which is reinforced by a tendency for technical and bureaucratic control strategies to accompany specialization. In the periphery, however, simple hierarchy (in which power is still personal rather than systematized within roles) should be the more prevalent mode of dominance, and the strategic effort to homogenize work should attenuate the effects of the technical variables (Edwards, 1979).

Efforts to deskill and homogenize work often produce (or take advantage of) an increasingly larger proportion of female workers, and such efforts should be most typical of periphery firms (Davies and Reich, 1972; Braverman, 1974). It is in the interest of craft unions to maintain skill hierarchies and detailed job descriptions, while industrial unions are more likely to mobilize an undifferentiated work force (Aronowitz, 1973). Therefore, the strategy of craft unions should be congruent with the prevailing technical and control imperatives in center firms, while the opposite should be true of industrial unions. On the other hand, core/periphery typologies often construct sectors by combining industries in which craft *and* industrial unions predominate. For example, construction and mining are both regarded as core industries, while printing and garment manufacture are often assigned to the periphery. Indeed, firms within certain industries (e.g., metal trades) are often organized by either or both types of unions. Thus, it is quite possible that these hypotheses pertaining to the impact of unions would not be supported in analyses utilizing industrial classifications of economic sectors.

---

[19] For example, among center firms the ability of unions to implement internal labor markets may be contingent upon such factors as skill specificity, capital intensity, and age of the establishment (Doeringer and Piore, 1971). Moreover, technical constraints such as scale and production continuity may attenuate or accelerate the efficacy of control strategies (and vice versa). While such hypotheses have not escaped our attention, elaborating them here would complicate matters unnecessarily.

Internal labor markets are ideally suited to the bureaucratic mode of social control: they institutionalize and reduce conflict between management and labor; instill long-term commitment to the firm and reduce labor turnover among employees; and reinforce workers' interests in existing administrative arrangements. Therefore, according to dualist theory, internal labor markets should be virtually absent in periphery establishments, and particularly prevalent in large-scale, unionized, center firms where tasks are complex and skills are sophisticated, diverse, and firm-specific.

The number of levels of supervision among nonadministrative jobs is an indirect measure of bureaucratic social control (Child, 1973). Firms organized according to simple hierarchy have fewer levels of supervision between top management and the workflow. In contrast, bureaucratically organized firms have decentralized decision-making apparatuses which are likely to be reflected in multilevel supervision hierarchies (Stinchcombe, 1959).[20] Technical considerations such as the scale and complexity of operations are also likely to effect a proliferation of supervisory levels (Perrow, 1967; Blau and Schoenherr, 1971), although in periphery firms simple hierarchy is likely to attenuate trends toward vertical differentiation. For both sectors, we hypothesize the well-documented curvilinear relationship between production continuity and the expansion of supervisory levels (e.g., Blau and Schoenherr, 1971; Blau et al., 1976).

A crucial aspect of firm structure is left unexamined by such interfirm comparisons—namely, how jobs are connected relationally within an establishment. For example, the contours of supervision and promotion hierarchies among the production jobs within a firm—and their relation to such attributes as skill level, closeness of supervision, demographic composition, and on-the-job training—might be examined by applying any of a variety of sociometric techniques (e.g., Hubert and Baker, 1978) to data describing positional networks within a firm (cf. Moch, 1979). Dualist and segmentation accounts would suggest that center firms, in which internal ladders and bureaucratic control supposedly predominate, are likely to recruit production-level supervisors from within the establishment and to tie hierarchies of promotion and supervision to gradients of skill and firm-specific experience. Periphery enterprises, on the other hand, purportedly lack internal ladders of authority and career advancement, and are more likely to utilize simple hierarchy as a mode of dominance. Thus, networks of power and promotion should less faithfully reflect gradients of skill and training in the periphery.

A note on differences in work organization within sectors: Such a focus on the sociometry of jobs would certainly inform debates about sectoral variation in work organization. Moreover, it would also reveal how analyses of industrial segmentation obscure important differences in job arrangements from firm to firm. To illustrate this claim, we have constructed matrices which show the promotion and supervision links among production jobs for two chemical establishments contained in our sample of California firms (Figures 1a and 1b). In addition, the figures list the general educational development (GED) and specific vocational preparation (SVP) required for each job, and indicate whether the job is characterized by situations in which adaptability to vari-

---

[20] Edwards (1979) distinguishes "technical control" as an intermediate evolutionary form in the transition from simple hierarchy to bureaucratic control. This suggests a continuum of organizational control (rather than discrete categories) ranging from centralized and personalized to decentralized and systematized (also see Blauner, 1964).

	Job Title	P1 1	P1 2	P1 3	P1 4	P1 5	P1 6	P2 1	P2 2	P2 3	P2 4	P2 5	P2 6	P2 7	P2 8	P2 9	P2 10	P2 11	M1 1	GED	SVP	VARCH	REPCON	DCP	STS	PHYSDEM
P1-1	Foreman	-	S	S	S	S	S													4	7	1	0	1	0	L
P1-2	Chief Operator	P	-				S													4	7	0	0	0	1	L
P1-3	Operator I		P	-																4	7	0	0	0	1	L
P1-4	Operator II			P	-		S													3	5	0	0	0	1	L
P1-5	Operator III				P	-														2	2	0	0	0	1	M
P1-6	Laborer					P	-													1	2	0	1	0	0	H
P2-1	Foreman							-	S	S	S	S	S	S	S	S	S	S		4	7	1	0	1	0	L
P2-2	Splitting Operator							P	-											4	7	0	0	0	1	L
P2-3	Distillation Operator								P	-										3	6	0	0	0	1	L
P2-4	Waste Water Operator									P	-									3	6	0	0	0	1	M
P2-5	Pumper										P	-								3	4	0	1	0	1	L
P2-6	Deionizer Operator											P	-							3	4	0	1	0	1	L
P2-7	Twitchell Operator												P	-						3	4	0	1	0	1	M
P2-8	Crystallization Operator													P	-					3	4	0	1	0	1	L
P2-9	Methylization Operator														P	-				3	4	0	1	0	1	M
P2-10	Fractionation Operator															P	-			3	4	0	1	0	1	L
P2-11	Laborer																P	-		1	2	0	1	0	0	H
*M-1	Plant Superintendent	S						S											-	5	8	1	0	1	0	L

Figure 1a. Promotion and Supervision Links among Production Jobs in Chemical Plant "A" (see text for explanation).

	Job Title	P1	P2	P3	P4	P5	P6	P7	P8	P9	P10	P11	P12	P13	P14	P15	M1	GED	SVP	VARCH	REPCON	DCP	STS	PHYSDEM
P1	Shift Supervisor	-	S	S	S	S	S	S	S	S	S	S	S	S	S	S		4	7	1	1	1	0	L
P2	Leadman	P	-		S											S		2	3	0	0	0	0	H
P3	Chemical Operator	P		-		S												3	5	0	0	0	1	L
P4	Solution Operator				-								S			S		3	5	0	0	0	1	M
P5	Chemical Operator Helper			P	P	-												2	2	0	1	0	0	M
P6	Flaker Operator					P	-											2	2	0	1	0	1	L
P7	Dust Plant Operator							-	S						S	S		3	5	0	0	0	1	L
P8	Dust Plant Charger							P	-									2	2	0	1	0	0	H
P9	Grinding Operator									-	S							2	2	0	1	0	1	M
P10	Grinding Operator Helper									P	-							1	2	0	1	0	0	M
P11	Bagger Operator						P					-						2	2	0	1	0	0	H
P12	Container Filler Operator												-					2	2	0	1	0	0	M
*P13	Lift Truck Operator						P							-				1	2	0	1	0	0	M
P14	Dust Packager													P	-			2	3	0	0	0	1	L
P15	Laborer		P						P		P	P	P		P	-		1	1	0	1	0	0	H
*M1	Plant Superintendent	S															-	5	8	1	0	1	0	L

Figure 1b. Promotion and Supervision Links among Production Jobs in Chemical Plant "B" (see text for explanation).

ety and change are important (VARCH), in which tasks are repetitive or continuous (REPCON), in which responsibility for the direction, control, or planning of an activity is required (DCP), and in which the precise attainment of set limits, tolerances, or standards is necessary (STS). The physical demands (low, medium, or high) of the job are also listed.[21] It should be underscored that these data pertain to typical "job-worker situations" which prevail within the establishment. They thus combine official policies of the enterprise with the analyst's observations of what actually has transpired vis-à-vis specific jobs (and their incumbents) in a firm.

---

[21] VARCH, REPCON, DCP, and STS are dummy variables with "1" indicating the presence of each temperament. For more information about data collection methods, definition of concepts, and measurement of these variables, see United States Department of Labor (1972). Production jobs which were not specific to this industry—and thus were not analyzed in detail by the Employment Service—are indicated in Figures 1a and 1b by an asterisk. Ratings for those jobs have been taken from published and unpublished data prepared for the third edition of the *Dictionary of Occupational Titles*.

Figure 1a pertains to firm "A," a chemical plant involved in processing and purifying fatty acids. The parent corporation, now multinational, was founded in 1840, but this plant was established after World War II. The firm has 161 employees (of which 105 are involved in production), is unionized, and operates four shifts at two production sites located five miles apart. Fatty acids produced in one department ("P2" in Figure 1a) are the chief constituent of organic chemicals produced by the other ("P1"). All workers enter as laborers, and advance by a union bidding arrangement based on seniority rights.

The letter "P" in the matrix indicates that the job listed in that row can *promote* to the job listed in the relevant column. An "S" indicates that the row job *supervises* the column job. Figure 1a corresponds rather nicely to simplified conceptions of dual economy which characterize the chemical industry as belonging to the core, manifesting internal ladders of promotion based on skill and seniority. Like the chemical plant studied by Blauner (1964), firm "A" is organized departmentally, with rigid promotion ladders within each of its two production units (i.e., P1–1 through P1–6 vs P2–1 through P2–11). With the exception of laborers in the first productive unit, each job within a department is supervised by the foreman, indicating one level of administration per department. Foremen, in turn, report to the production superintendent (M1). These hierarchies faithfully reflect differentials among jobs with respect to educational and technical requirements (GED and SVP). In each department, only the foreman job is characterized by "variety and change" or "direction, control, and planning"; jobs toward the bottom of the promotion and supervision ladders are "repetitive and continuous." Adherence to predetermined tolerances or standards is important for all jobs except those at the bottom and the top of each department's hierarchy. Ladders of promotion and authority also correspond rather closely to the physical demands of work, with those at the bottom performing the most strenuous tasks.

Figure 1b presents comparable data for chemical firm "B." This establishment is in exactly the same four digit S. I. C. classification as firm "A," and of almost identical size (173 employees, of which 83 are engaged in production). Its administrative and operating procedures also seem to be virtually identical to those of firm "A": establishment "B" operates four shifts, is also part of a larger corporation, and is divided administratively into five departments (versus six for firm "A").

However, the authority hierarchy and the structure of opportunity implied by the promotion network are conspicuously different from those shown in Figure 1a. (Since the promotion ladder in firm "B" is especially difficult to decipher from Figure 1b—that is, one cannot permute the rows to obtain a systematic pattern of promotion—we have also expressed it graphically in Figure 1c.) Note that laborers (P15), for example, can be promoted directly to leadmen; indeed, union agreements stipulate that *only* laborers can be promoted to leadmen. (Laborers are also promoted into *five* other positions. Prevailing union practices allow laborers to bid for any position; however, the detailed job analyses indicate that laborers actually *do* move up as indicated in Figure 1c.) Leadmen, in turn, can move into supervisory positions, while dust plant operator (which requires greater educational and vocational training) is a "dead-end" job. Leadmen also supervise solution operators, even though the latter job again requires more educational and vocational preparation. A lift truck operator can promote to flaker operator, even though the latter job requires less training time (SVP).

In short, promotion and supervision hierarchies are considerably less orderly in this establishment. Entry level incumbents can in principle bypass certain "dead-end" jobs between the bottom and top of the departmental hierarchy. Moreover, gradients of skill and experience bear less relation to the structure of opportunity and authority. (There is also much less correspondence between levels of promotion and supervision on the one hand, and temperaments and physical

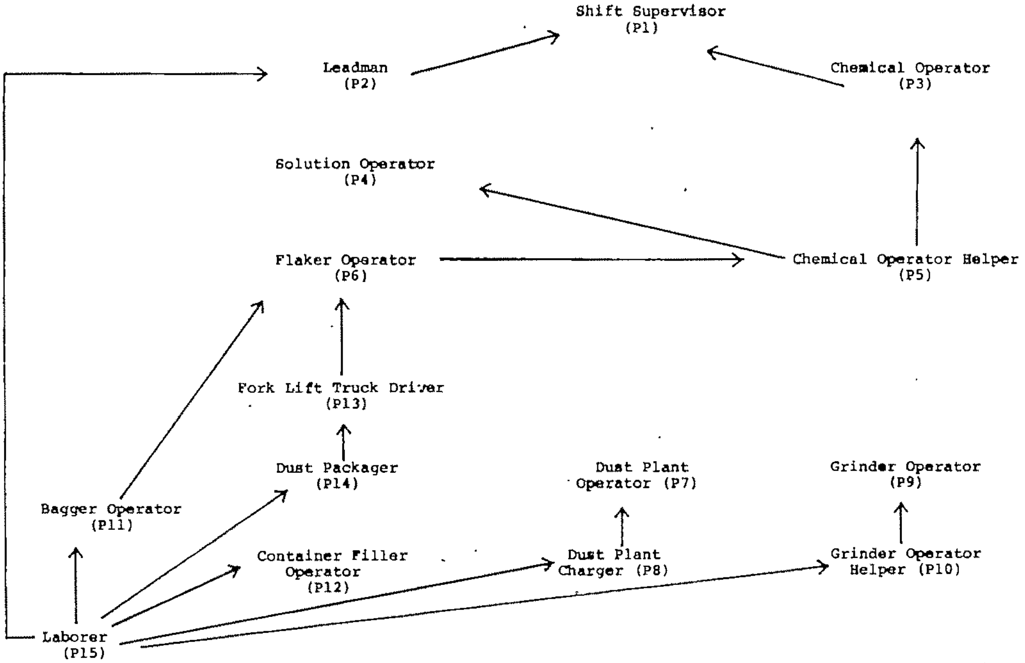


Figure 1c. Promotion Ladder within Chemical Firm "B" (adapted from Figure 1b; arrow[s] indicate job to which promoted).

demands of jobs on the other, at least compared to establishment "A.") Both chemical firms are represented by the same union, are located in the same state and detailed industry category, and, indeed, were analyzed by the same individual at the same point in time. Thus, any persuasive explanation of why these two establishments organize work roles so differently must move beyond simple, mechanistic notions of "dualism" or "segmentation," focusing on the concrete organizational and suborganizational determinants of promotion and supervision.[22] Of course, this is not to suggest that industrial distinctions are irrelevant, or that hypotheses inspired by dualist or segmentation approaches would not be helpful in distinguishing these establishments from those in some other industry.[23] However, as Burawoy (1979:245–6) notes,

> the distinction between competitive and monopoly sectors can explain only so much of the variation in the labor process. Differences within each sector are probably as great if not greater than differences between sectors.

---

[22] One conspicuous difference between the establishments involves the physical separation of departmental units in firm "A" but not in "B." This factor might explain, for example, the existence of two nonproduction jobs—shipping clerk and truck driver—within the organic chemicals department of firm "A." Presumably, these jobs involve transferring fatty acids from the other production site. However, another anomaly is apparently unrelated to this difference between establishments. While firm "A" employs its stock clerk in the maintenance department, the same nonproduction job is situated within the production unit of firm "B." The psychological and economic significance of work-unit location and structure has been documented in the chemical industry (Blauner, 1964; Gallie, 1978) and in economic organizations generally (e.g., Grimes and Klein, 1973; Van de Ven and Delbecq, 1974; Comstock and Scott, 1977). Thus, such differences in work organization among firms—which cannot be explained by simplistic theories of segmentation—are likely to have important effects on worker experiences.

[23] Indeed, network data of this type would be quite useful in examining such hypotheses. Normalized measures of "fit" between hierarchies of supervision, promotion, skill, training, and other job attributes can be computed for each firm (see, for example, Hubert, 1978). Such measures may then be employed as firm-level characteristics with which to test whether internal job structures do in fact differ across sectors.

The Trend in Dualism: Temporal Changes in Enterprise Work Organization

We suggested earlier in this essay that historical transformations in work organization—while central to disputes among students of inequality—have been largely ignored by researchers. The few empirical studies undertaken (e.g., Horowitz and Herrnstadt, 1966; Scoville, 1969; Dubnoff, 1978; Spenner, 1979) have done little to advance the debate, since differences among perspectives primarily concern how changes in work arrangements are instigated and implemented *within the firm*. Furthermore, most analyses have failed to consider rival explanations for the patterns of change documented.

An illustration: One could compare various structuralist perspectives against a neoclassically inspired alternative by studying temporal changes in numerous firm-level phenomena—for example: in the number of distinct jobs within the enterprise; the number of employees (by sex, race, etc.); degree of administrative intensity; or the overall level and degree of homogeneity of skill among jobs. For expository purposes, we select one of these—changes in the firm's "skill mix"—and outline analyses for manufacturing establishments.

According to neoclassical theory, changes in the quantity and quality of labor utilized should reflect the calculus of marginal substitution whereby factor inputs are employed until their marginal productivity and cost are equal. Assume, for the moment, that the firm is in perfectly competitive commodity and labor markets and that it experiences constant returns to scale; furthermore, we allow all factor inputs to be variable over the interval under study. Under these conditions, the mix of labor and capital (and of various types of labor) employed, far from reflecting any control imperative or the like, should be

> determined by purely technical properties of the production function relating the quantities of these inputs used and their respective marginal physical products. If we knew the exact form of the production function, we could predict the behavior of the factoral shares (Nicholson, 1978:419).

Thus, if perfect competition prevails, one could examine temporal changes in industrial capital expenditure and the prevailing wages paid to different classes of workers (grouped according to skill) on the one hand, and the value added by capital and labor on the other hand, in order to determine if this simplified base-line model "makes sense." In other words, one would follow neoclassical precedent by first assuming that firm decisions are derivatives of industrial conditions over which the perfectly competitive enterprise has no control.

Briefly stated, to the extent that this naive neoclassical model is accurate, changes in absolute and relative employment among various skill families should correspond to patterns of change in productivity and factor prices for the industry within which the firm produces.[24] Clearly, this model will fail to explain firm-specific changes in skill composition in many instances. This failure may only reflect the dubiety of assuming perfect competition or constant returns to scale. Thus, one would next conduct "deviant case" analyses to determine whether departures from this simplistic neoclassical scenario can be subsumed under more complex and realistic assumptions concurrent with neoclassical orthodoxy. For example, the types of data usually employed in studies of industrial segmentation index the structure of input and output markets, allowing an assessment of whether firm "deviance" is simply attributable to location within an industry which departs from perfect competition. Conditions of industrial instability—tapped by such variables as work stoppages—may also account for patterns of skill utilization not predicted by the base-line model, either because the specification of the production function is inappropriate for such industries, or because of the effect of uncertainty on administrative decision making. If, as radical theorists suggest, owners and managers typically respond to such uncertainty by fragmenting, deskilling, or homogenizing their work force, then firms should institute such changes in their skill mix even when "market forces" imply drastically different "rational" responses.

---

[24] Since the enterprise is assumed to be in perfect competition, published industry-level data (e.g., in the *Census of Manufactures*) documenting costs and productivities of classes of labor and other factor inputs should characterize the economic and technical constraints which guide the firm's decisions regarding productive arrangements.

In sum, we envision a dispersion of firms around predicted changes in skill distributions based on a simple version of the neoclassical model. Indeed, segmentation theorists often grant the decisiveness of market forces within the periphery, and, thus, these naive predictions should depict patterns of change most accurately for firms located there. If critics of neoclassical orthodoxy are correct, the most drastic outliers should consist of center firms so insulated from market mechanisms that even the most generous variants of neoclassical theory cannot account for their behavior.

SUMMARY AND CONCLUSIONS

This essay has examined the recent shift toward more structural explanations in studies of inequality. After providing a brief overview of this body of research and some of its shortcomings, we examined its theoretical underpinnings, comparing structuralist perspectives on work organization derived from institutional economics and neo-Marxism to more orthodox accounts based on neoclassical and industrialism theories. This discussion, coupled with an examination of the levels and units of analysis adopted by various approaches, suggested areas where the different perspectives overlap and diverge. We concluded that social scientists interested in the structure of social inequality would benefit immeasurably by "bringing the firms back in"—that is, by incorporating organizational structure and process into empirical analyses of economic segmentation and work. Work arrangements within firms (and their trend) are the focus of most structural perspectives on positional stratification; thus, empirical studies grounded at the organizational level are more likely to inform current debates about the structure of work than is the growing body of research about structural effects on individual attainment or covariation among occupational and industrial characteristics.

Structuralists typically dismiss status attainment research by arguing that models of the socioeconomic career are misspecified unless they embrace the structures which organize attainment (e.g., Sørenson, 1975; Bowles and Gintis, 1976; Burawoy, 1977; Wright and Perrone, 1977; Beck et al., 1978; Stolzenberg, 1978). However, "structures," too, are not immune from misspecification. Unlike research on the person-level determinants of achievement, the new structuralism is not leading toward a cohesive set of findings, perhaps because structures have been either assumed or inferred from their consequences for individuals, rather than subjected to detailed empirical scrutiny. In an effort to correct that deficiency, we outlined an agenda for future research, focusing on three aspects of work organization: (a) the units which comprise the structure of work and the dimensions underlying economic segmentation; (b) the effects of sectoral differentiation on technical and administrative arrangements within the firm; and (c) temporal changes in how enterprises organize production. While data sources at the job or firm level are admittedly scarce, they do exist; each of the analyses outlined here is part of our own ongoing program of empirical research utilizing data collected by the United States Employment Service (Bielby and Baron, 1979; 1980).

We fully expect that more complete data on jobs, firms, and industries will be assembled by social scientists as they become increasingly fond of structural perspectives on social stratification. However, we think careful consideration of this new orientation—its underpinnings, strengths, weaknesses, and implications—is timely, before the emerging consensus about the importance of "structure" assumes the status of dogma and engenders a conditioned response among researchers to construct categorical variables.

Underlying our discussion is the conviction that the first step among structural stratification researchers must be to specify a theory of the relevant structure(s), and provide empirical referents which allow the veracity of that theory to be tested. To the extent that the various perspectives employed in structural research are scientifically useful, they must possess empirical content; as a consequence, the choice among them requires analyses which subject predictions of competing accounts to the possibility of meaningful empirical falsification. In our view, this aim is best accomplished neither by simply imposing (or justifying) a particular structure by fiat, nor by inferring it post hoc from its effects on the micro elements over which it is assumed to be logically and temporally prior. Rather, one must examine its constituent elements and the ways in which they are organized. Yet much of the recent structuralist achievement research either adopts a specific perspective (e.g., Marxism) to rationalize a model of attainment, or else assigns priority to the attainment model and justifies the importation of one or more substantive frameworks (e.g., dual economy theory, organizational sociology). In our view, issues of individual socioeconomic attainment should not detract attention from more fundamental questions regarding the characteristics of those structures which (presumably) shape achievement. By relating studies of segmentation and stratification more directly to the organization of work within firms, we hope to strengthen the correspondence between theory and method among those studying either the "empty places" in our economy or the people who occupy them.

REFERENCES

Addison, John T., and W. Stanley Siebert
1979 The Market for Labor: An Analytical Treatment. Santa Monica: Goodyear.

Aldrich, Howard E.
1972 "Technology and organization structure: a reexamination of the findings of the Aston group." Administrative Science Quarterly 17:26–43.

Aldrich, Howard E., and Jane Weiss
1980 "Class, business size, and economic inequality: patterns of income determination among owners and managers." Paper presented at annual meeting of the American Sociological Association, New York.

Amber, George S., and Paul S. Amber
1962 Anatomy of Automation. Englewood Cliffs: Prentice-Hall.

Aronowitz, Stanley
1973 False Promises. New York: McGraw-Hill
1978 "Marx, Braverman, and the logic of capital." Insurgent Sociologist 8:126–46.

Averitt, Robert T.
1968 The Dual Economy. New York: Horton.

Baran, Paul, and Paul Sweezy
1966 Monopoly Capital. New York: Monthly Review Press.

Baron, James N.
Forthcoming "Sex, segments, and stratification: comment on Snyder, Hayward, and Hudis." American Journal of Sociology.

Baron, James N., and William T. Bielby
1980 "Workers and machines: a missing link in current stratification research." Paper presented at annual meeting of the American Sociological Association, New York.

Beck, E. M., Patrick M. Horan, and Charles M. Tolbert II
1978 "Stratification in a dual economy: a sectoral model of earnings determination." American Sociological Review 43:704–20.
1980 "Social stratification in industrial society: further evidence for a structural alternative (Reply to Hauser)." American Sociological Review 45:712–8.

Becker, Gary
1964 Human Capital. New York: National Bureau of Economic Research.

Bell, Daniel
1973 The Coming of Post-Industrial Society. New York: Basic.

Berle, Adolf, and Gardiner Means
1932 The Modern Corporation and Private Property. New York: Macmillan.

Bibb, Robert, and William H. Form
1977 "The effects of industrial, occupational, and sex stratification on wages in blue-collar markets." Social Forces 55:974–96.

Bielby, William T., and James N. Baron
1979 "Jobs, firms, and industries: economic 'dualism' and the organization of work." Unpublished manuscript, Social Process Research Institute, University of California, Santa Barbara.
1980 "Economic 'dualism' and work organization." Paper presented at annual meeting of the American Sociological Association, New York.

Billings, Robert S., Richard J. Klimoski, and James A. Breaugh
1977 "The impact of a change in technology on job characteristics: a quasi-experiment." Administrative Science Quarterly 22:318–39.

Blau, Peter M., and Otis D. Duncan
1967 The American Occupational Structure. New York: Wiley.

Blau, Peter M., Cecilia M. Faabe, William McKinley, and Phelps K. Tracy
1976 "Technology and organization in manufacturing." Administrative Science Quarterly 21:20–40.

Blau, Peter M., and Richard A. Schoenherr
1971 The Structure of Organizations. New York: Basic.

Blau, Peter M., and W. Richard Scott
1962 Formal Organizations. San Francisco: Chandler.
Blaug, Mark
1970 An Introduction to the Economics of Education. London: Penguin.
Blaug, Mark, Maurice Peston, and Adrian Ziderman
1967 The Utilization of Educated Manpower in Industry (Report No. 2, Unit for Economic and Statistical Studies in Higher Education, London School of Economics and Political Science). Edinburgh: Oliver and Boyd.
Blauner, Robert
1964 Alienation and Freedom. Chicago: University of Chicago Press.
Bluestone, Barry
1970 "The tripartite economy: labor markets and the working poor." Poverty and Human Resources Abstracts (supplement) 5:15–35.
Bowles, Samuel, and Herbert Gintis
1976 Schooling in Capitalist America. New York: Harper and Row.
Braverman, Harry
1974 Labor and Monopoly Capital. New York: Monthly Review Press.
Bridges, William P., and Richard A. Berk
1974 "Determinants of white collar income: an evaluation of equal pay for equal work." Social Science Research 3:211–33.
Bright, James R.
1958 Automation and Management. Boston: Graduate School of Business Administration, Harvard University.
Burawoy, Michael
1977 "Social structure, homogenization, and 'The process of status attainment in the United States and Great Britain.' " American Journal of Sociology 82:1031–42.
1979 "The anthropology of industrial work." Annual Review of Anthropology 8:231–66.
1980 "The politics of production and the production of politics: a comparative analysis of piecework machine shops in the United States and Hungary." Pp. 261–99 in Maurice Zeitlin (ed.), Political Power and Social Theory, Vol. I. Greenwich: JAI Press.
Burns, Tom, and G. M. Stalker
1961 The Management of Innovation. London: Tavistock Publications.
Caplow, Theodore
1954 The Sociology of Work. New York: McGraw-Hill.
Cassell, Frank H., Steven M. Director, and Samuel I. Doctors
1975 "Discrimination within internal labor markets." Industrial Relations 14:337–44.
Caves, Richard E.
1980 "Industrial organization, corporate strategy and structure." Journal of Economic Literature 18:64–92.
Chandler, Alfred D., Jr.
1962 Strategy and Structure. Cambridge, Mass.: M. I. T. Press.
1977 The Visible Hand. Cambridge and London: Belknap Press.
Child, John
1972 "Organization structure, the environment, and performance: the role of strategic choice." Sociology 6:1–22.
1973 "Strategies of control and organizational behavior." Administrative Science Quarterly 19:1–17.
Child, John, and Roger Mansfield
1972 "Technology, size and organizational structure." Sociology 6:369–80.
Clark, Colin
1951 The Conditions of Economic Progress. London: Macmillan.
Collins, Randall
1979 The Credential Society. New York: Academic.
Comstock, Donald E., and W. Richard Scott
1977 "Technology and the structure of subunits: distinguishing individual and workgroup effects." Administrative Science Quarterly 22:177–202.
Conk, Margo A.
1978 "Occupational classification in the United States Census: 1870–1940." Journal of Interdisciplinary History 9:111–30.
Cyert, Richard M., and James G. March
1963 A Behavioral Theory of the Firm. Englewood Cliffs: Prentice-Hall.
Dahrendorf, Ralf
1959 Class and Class Conflict in Industrial Society. Stanford: Stanford University Press.
Davies, Margery, and Michael Reich
1972 "On the relationship between sexism and capitalism." Pp. 348–56 in Richard C. Edwards, Michael Reich, and Thomas E. Weisskopf (eds.), The Capitalist System. Englewood Cliffs: Prentice-Hall.
Director, Steven M., and Samuel I. Doctors
1976 "Racial differences in blue-collar turnover rates." Industrial Relations 15:338–42.
Doeringer, Peter B., and Michael J. Piore
1971 Internal Labor Markets and Manpower Analysis. Lexington, Mass.: D. C. Heath.
Dubnoff, Steven
1978 "Inter-occupational shifts and changes in the quality of work in the American economy, 1900–1970." Paper presented at annual meeting of the Society for the Study of Social Problems, San Francisco.
Duncan, Otis D.
1968 "Social stratification and mobility: problems in the measurement of trend." Pp. 675–719 in Eleanor B. Sheldon and Wilbert E. Moore (eds.), Indicators of Social Change. New York: Russell Sage Foundation.
Edwards, Richard C.
1975 "The social relations of production in the firm and labor market structure." Pp. 3–26 in Richard C. Edwards, Michael Reich, and David M. Gordon (eds.), Labor Market Segmentation. Lexington, Mass.: D. C. Heath.
1979 Contested Terrain. New York: Basic.
Featherman, David L., and Robert M. Hauser
1978 Opportunity and Change. New York: Academic.

Freedman, Marcia K.
1969 The Process of Work Establishment. New York and London: Columbia University Press.
1976 Labor Markets: Segments and Shelters. Montclair, N.J.: Allanheld, Osmun, and Co.

Freeman, John H.
1973 "Environment, technology, and the administrative intensity of manufacturing organizations." American Sociological Review 38:750–63.

Galbraith, John K.
1967 The New Industrial State. New York: Houghton Mifflin.
1973 Economics and the Public Purpose. Boston: Houghton Mifflin.

Gallie, Duncan
1978 In Search of the New Working Class: Automation and Social Integration within the Capitalist Enterprise. London: Cambridge University Press.

Gordon, David M.
1972 Theories of Poverty and Underemployment. Lexington, Mass.: D. C. Heath.
1976 "Capitalist efficiency and socialist efficiency." Monthly Review 28:19–39.
1977 "Capitalism and the roots of urban crisis." Pp. 82–112 in Roger E. Alcaly and David Mermelstein (eds.), The Fiscal Crisis of American Cities. New York: Vintage.

Gordon, Margaret S., and Margaret Thal-Larsen
1969 Employer Policies in a Changing Labor Market. Berkeley: Institute of Industrial Relations, University of California.

Granovetter, Mark S.
Forthcoming "Toward a sociological theory of income differences." To be published in Ivar Berg (ed.), Sociological Perspectives on Labor Markets. New York: Academic.

Grimes, A. J., and S. M. Klein
1973 "The technological imperative: the relative impact of task unit, modal technology, and hierarchy on structure." Academy of Management Journal 16:583–97.

Halaby, Charles N.
1979 "Sexual inequality in the workplace: an employer specific analysis of pay differences." Social Science Research 8:79–104.

Harrison, Bennett
1972 Education, Training, and the Urban Ghetto. Baltimore: Johns Hopkins Press.

Hauser, Robert M.
1980 "On 'Stratification in a dual economy.' Comment on Beck et al." American Sociological Review 45:702–12.

Hickson, David J., D. S. Pugh, and Diana Pheysey
1969 "Operations technology and organization structure: an empirical reappraisal." Administrative Science Quarterly 14:378–97.

Hirschliefer, J.
1976 Price Theory and Applications. Englewood Cliffs: Prentice-Hall.

Hodson, Randy D.
1978 "Labor in the monopoly, competitive, and state sectors of production." Politics and Society 8:429–80.

Hodson, Randy D., and Robert L. Kaufman
Forthcoming "Circularity in the dual economy: comment on Tolbert, Horan, and Beck." American Journal of Sociology.

Horan, Patrick M., Charles M. Tolbert, II, and E. M. Beck
Forthcoming "The circle has not closed." American Journal of Sociology.

Horowitz, Morris A., and Irwin L. Herrnstadt
1966 "Changes in the skill requirements of occupations in selected industries." Pp. 227–87 in The Employment Impact of Technological Change (Appendix Vol. II to Report of the National Commission on Technology, Automation, and Economic Progress). Washington, D.C.: U.S. Government Printing Office.

Hubert, Lawrence J.
1978 "Generalized proximity function comparisons." British Journal of Mathematical and Statistical Psychology 31:179–92.

Hubert, Lawrence J., and Frank B. Baker
1978 "Evaluating the conformity of sociometric measurements." Psychometrika 43:31–41.

Indik, Bernard F.
1964 "The relationship between organizational size and supervision ratio." Administrative Science Quarterly 9:301–12.

Jencks, Christopher, Susan Bartlett, Mary Corcoran, James Crouse, David Eaglesfield, Gregory Jackson, Kent McClelland, Peter Mueser, Michael Olneck, Joseph Schwartz, Sherry Ward, and Jill Williams.
1979 Who Gets Ahead? New York: Basic.

Kalleberg, Arne L., and Larry J. Griffin
1978 "Positional sources of inequality in job satisfaction." Sociology of Work and Occupations 5:371–401.
1980 "Class, occupation, and inequality in job rewards." American Journal of Sociology 85:731–68.

Kalleberg, Arne L., Michael Wallace, and Robert P. Althauser
1980 "Economic segmentation, worker power, and income inequality." Paper presented at National Science Foundation Conference on Labor Market Structure and Socioeconomic Stratification, University of Georgia, Athens.

Kaufman, Robert, Randy D. Hodson, and Neil D. Fligstein
1980 "Defrocking dualism: a new approach to defining industrial sectors." Paper presented at National Science Foundation Conference on Labor Market Structure and Socioeconomic Stratification, University of Georgia, Athens.

Kerr, Clark, John Dunlop, Frederick Harbison, and Charles A. Myers
1964 Industrialism and Industrial Man. New York: Oxford University Press.

Kimberly, John R.
1976 "Organizational size and the structuralist

perspective: a review, critique, and proposal." Administrative Science Quarterly 21:571–97.

Kluegel, James R.
1978 "The causes and costs of racial exclusion from job authority." American Sociological Review 43:285–301.

Kohn, Melvin L., and Carmi Schooler
1973 "Occupational experience and psychological functioning: an assessment of reciprocal effects." American Sociological Review 38:97–118.
1978 "The reciprocal effects of the substantive complexity of work and intellectual flexibility: a longitudinal assessment." American Journal of Sociology 84:24–52.

Leibenstein, Harvey
1979 "A branch of economics is missing: micro-micro theory." Journal of Economic Literature 17:477–502.

Leigh, Duane E.
1976a "Job experience and earnings among middle-aged men." Industrial Relations 15:130–46.
1976b "The occupational mobility of young men, 1965–1970." Industrial and Labor Relations Review 30:68–78.
1978 An Analysis of the Determinants of Occupational Upgrading. New York: Academic.

Lenski, Gerhard E.
1966 Power and Privilege. New York: McGraw-Hill.

Lowell, Ruth F.
1978 "Testing a dual labor market classification of jobs." Journal of Regional Science 18:95–103.

Lynch, Beverly P.
1974 "An empirical assessment of Perrow's technology construct." Administrative Science Quarterly 19:338–56.

Marglin, Stephen A.
1974 "What do bosses do?: the origins and functions of hierarchy in capitalist production." Review of Radical Political Economics 6:60–112.

Meissner, Martin
1969 Technology and the Worker. San Francisco: Chandler.

Miller, Joanne, Carmi Schooler, Melvin L. Kohn, and Karen A. Miller
1979 "Women and work: the psychological effects of occupational conditions." American Journal of Sociology 85:66–94.

Moch, Michael K.
1979 "The relational structure of formal organizations: employee location in the work group and clique structure and satisfaction with interpersonal relationships." Paper presented at annual meeting of the American Sociological Association, Boston.

Nicholson, Walter
1978 Microeconomic Theory. Second edition. Hinsdale, Illinois: Dryden.

O'Connor, James
1973 The Fiscal Crisis of the State. New York: St. Martin's Press.

Oi, W. Y.
1962 "Labor as a quasi-fixed factor." Journal of Political Economy 70:538–55.

Oster, G.
1979 "A factor analytic test of the theory of the dual economy." Review of Economics and Statistics 61:33–9.

Ouchi, William G.
1977 "The relationship between organizational structure and organizational control." Administrative Science Quarterly 22:95–113.

Parsons, Talcott, and Neil J. Smelser
1956 Economy and Society. New York: Free Press.

Perrow, Charles
1967 "A framework for the comparative analysis of organizations." American Sociological Review 32:194–208.

Pfeffer, Jeffrey
1977 "Toward an examination of stratification in organizations." Administrative Science Quarterly 22:553–67.

Pfeffer, Jeffrey, and Jerry Ross
1979 "Do wage and status attainment differ by industrial sector?" Unpublished manuscript, Graduate School of Business, Stanford University.

Piore, Michael J.
1975 "Notes for a theory of labor market stratification." Pp. 125–50 in Richard C. Edwards, Michael Reich, and David M. Gordon (eds.), Labor Market Segmentation. Lexington, Mass.: D. C. Heath.

Pondy, Louis R.
1969 "Effects of size, complexity, and ownership on administrative intensity." Administrative Science Quarterly 14:47–60.

Robinson, Robert V., and Jonathan Kelley
1979 "Class as conceived by Marx and Dahrendorf: effects on income inequality, class consciousness, and class conflict in the United States and Great Britain." American Sociological Review 44:38–58.

Rosen, Sherwin
1974 Review of David M. Gordon's Theories of Poverty and Underemployment. Journal of Political Economy 82:437–9.

Rosenbaum, James A.
1979 "Organizational career mobility: promotion chances in a corporation during periods of growth and contraction." American Journal of Sociology 85:21–48.

Rosenberg, Samuel
1975 "The dual labor market: its existence and consequences." Unpublished Ph.D. dissertation, University of California, Berkeley.

Schervish, Paul G.
1980 "Class segments and the structure of unemployment." Paper presented at National Science Foundation Conference on Labor Market Structure and Socioeconomic Stratification, University of Georgia, Athens.

Scoville, J. G.
1969 The Job Content of the U. S. Economy, 1940–1970. New York: McGraw-Hill.

Shepherd, William G.
1975 The Treatment of Market Power: Antitrust, Regulation, and Public Enterprise. New York and London: Columbia University Press.
Simon, Herbert A.
1957 Models of Man. New York: Wiley.
Snyder, David, Mark D. Hayward, and Paula M. Hudis
1978 "The location of change in the sexual structure of occupations, 1950–1970: insights from labor market segmentation theory." American Journal of Sociology 84:706–17.
Sørenson, Aage B.
1975 "Models of social mobility." Social Science Research 4:65–92.
Spenner, Kenneth I.
1977 "From generation to generation: the transmission of occupation." Unpublished Ph.D. dissertation, University of Wisconsin, Madison.
1979 "Temporal changes in work content." American Sociological Review 44:968–75.
Spilerman, Seymour
1977 "Careers, labor market structure, and socioeconomic achievement." American Journal of Sociology 83:551–93.
Steinberg, Edward
1975 "Upward mobility in the internal labor market." Industrial Relations 14:259–65.
Stinchcombe, Arthur L.
1959 "Bureaucratic and craft administration of production: a comparative study." Administrative Science Quarterly 4:168–87.
Stolzenberg, Ross M.
1975 "Occupations, labor markets, and the process of wage attainment." American Sociological Review 40:645–65.
1978 "Bringing the boss back in: employer size, employee schooling, and socioeconomic achievement." American Sociological Review 43:813–28.
Stone, Katherine
1974 "The origins of job structures in the steel industry." Review of Radical Political Economics 6:113–73.
Talbert, Joan, and Christine E. Bose
1977 "Wage-attainment processes: the retail clerk case." American Journal of Sociology 83:403–24.
Temme, Lloyd V.
1975 Occupations: Meanings and Measures. Washington: Bureau of Social Science Research.
Thurow, Lester C.
1975 Generating Inequality. New York: Basic.
Tolbert, Charles M., II, Patrick M. Horan, and E. M. Beck
1980 "The structure of economic segmentation: a dual economy approach." American Journal of Sociology 85:1095–116.
Treiman, Donald J.
1977 Occupational Prestige in Comparative Perspective. New York: Academic.
Udy, Stanley H., Jr.
1980 "The configuration of occupational structure." Pp. 156–65 in Hubert M. Blalock, Jr. (ed.), Sociological Theory and Research: A Critical Approach. New York: Free Press.
U. S. Bureau of the Census
1975 Census of Manufactures, 1972. Subject Series: General Summary, MC72(1)-1. Washington, D.C.: U. S. Government Printing Office.
U. S. Department of Labor
1972 Handbook for Analyzing Jobs. Manpower Administration. Washington, D.C.: U. S. Government Printing Office.
1975 Job Tenure of Workers, January 1973. Bureau of Labor Statistics, Special Labor Force Report 172. Washington, D.C.: U. S. Government Printing Office.
Van de Ven, Andrew H., and Andre L. Delbecq
1974 "A task contingent model of work-unit structure." Administrative Science Quarterly 19:183–97.
Wachter, Michael L.
1974 "Primary and secondary labor markets: a critique of the dual approach." Brookings Papers on Economic Activity 3:637–93.
Wallace, Michael, and Arne L. Kalleberg
Forthcoming "Economic organization, occupations, and labor force consequences: toward a specification of dual economy theory." To be published in Ivar Berg (ed.), Sociological Perspectives on Labor Markets. New York: Academic.
Williamson, Oliver A.
1963 The Economics of Discretionary Behavior: Managerial Objectives in a Theory of the Firm. Englewood Cliffs: Prentice-Hall.
1975 Markets and Hierarchies: Analysis and Antitrust Implications. New York: Free Press.
Wolf, Wendy C., and Neil D. Fligstein
1979 "Sex and authority in the workplace: the causes of sexual inequality." American Sociological Review 44:232–52.
Woodward, Joan
1965 Industrial Organization: Theory and Practice. London: Oxford University Press.
Wright, Erik O.
1978 "Race, class, and income inequality." American Journal of Sociology 83:1368–97.
Wright, Erik O., and Luca Perrone
1977 "Marxist class categories and income inequality." American Sociological Review 42:32–55.
Zucker, Lynne G., and Carolyn Rosenstein
1980 "Institutional structure and individual characteristics as explanations of labor market processes and outcomes: dual economy reconsidered." Paper presented at annual meeting of the American Sociological Association, New York.

CRIME AND POVERTY: SOME EXPERIMENTAL EVIDENCE FROM EX-OFFENDERS

RICHARD A. BERK
University of California at Santa Barbara

KENNETH J. LENIHAN
John Jay College
City University of New York

PETER H. ROSSI
University of Massachusetts at Amherst

American Sociological Review 1980, Vol. 45 (October):766–786

Building on perspectives from sociology, criminology, and economics, this article reports findings from a randomized experiment undertaken with over 2,000 ex-offenders in the states of Texas and Georgia in which unemployment benefits were extended to individuals immediately upon release from prison. The analysis focuses on the endogenous relationships (over a one-year follow-up period) between these "transfer payments," unemployment, arrests for property and nonproperty crimes, and the resulting time spent in jail or prison. By and large, the hypotheses derived from a priori theory are supported by the data.

INTRODUCTION

Strong individual and aggregate level correlations between poverty and official measures of crime are perhaps among the most firmly established of social science empirical generalizations. However calculated, official crime rates are almost always higher among the poor, and poor people are more likely to be arrested and convicted for a wide variety of offenses. Yet, the precise mechanisms linking poverty to crime remain elusive. Writers as diverse in their perspectives as William A. Bonger (1916), Robert K. Merton (1957) and Gary S. Becker (1968) have provided plausible theoretical explanations, but, to date, the empirical tests have proved equivocal.

Part of the difficulty no doubt stems from conceptual ambiguities: should economic need, for example, be treated as an attribute of an individual (e.g., Ehrlich, 1973), or as an attribute of some social entity (e.g., Miller, 1958)? Problems in measuring the "real" rate of crime (Erikson and Empey, 1963; Sparks et al., 1977; Penick and Owens, 1976) and in making proper causal inferences from aggregate data (Blumstein et al., 1978; Brier and Fienberg, 1980) further complicate matters. However, perhaps the major obstacle has been the necessity of relying on naturally occurring variation in critical exogenous variables.[1]

First, there is no guarantee that the exogenous variables of particular interest

---

* Direct all correspondence to: Richard A. Berk; Department of Sociology; University of California; Santa Barbara, CA 93106.

The research reported in this paper required the active cooperation of a large number of people. We are enormously indebted to the research staff put together under the auspices of the prison systems of Texas and Georgia; to research assistants Anthony Shih, Nancy Jurik, Jeffrey Liker, Eleanor Weber-Burdin, and J. Patrick Henry; and, especially, to Dr. Howard Rosen, formerly of the Department of Labor's Employment and Training Administration. The research was supported by DOL Grant No. 21-25-77-05, "Analysis of Transitional Aid Research Project (TARP) Experimental Data," of which Rossi was Principal Investigator. A full exposition of the TARP experiments and the findings is to be found in Rossi et al., 1980. The authors are listed in alphabetical order to denote equal authorship.

[1] Possibly the strongest empirical case for disentangling the relationships between poverty and crime through the observation of naturally occurring variation can be built from time-series analyses in which economic indicators (with appropriate lags) are related to indices of crime over time (Brenner, 1976). Although such studies have shown that various crime indices are indeed sensitive to lagged unemployment rates and other economic indicators, the level of aggregation in such analyses has been countries as a whole or large jurisdictions within countries, a level far removed from individuals. Thus, one risks the ecological fallacy if inferences to individuals are made.

will possess sufficient variance to permit powerful tests of causal hypotheses. At the extreme, some variables may approximate constants. Second, when sufficient variance does exist, collinearity among the exogenous variables can effectively prohibit the estimation of stable multivariate models. At the extreme, the matrix of regressors may be singular. Finally, even with the aid of recent advances in structural equation modeling, the direction of causal effects can be extremely difficult to disentangle. At the extreme, a set of simultaneous equations may be hopelessly underidentified so that unique parameter estimates cannot be calculated.

Consider the following alternative: suppose it were possible to manipulate at least some of the important causal variables in a manner that would generate the desired variation, that would reduce and perhaps eliminate multicollinearity, and that would permit one to determine whether change in a prospective causal variable in fact preceded change in an outcome of interest. This model, of course, comes from experiments undertaken in laboratories and, of late, has been increasingly applied in field settings. Indeed, the randomized experiment, probably the definitive laboratory research design, has been used in well over 300 natural settings within evaluation research traditions alone (Boruch et al., 1978).

The prospect of experimental manipulation suggests a number of possibilities for the study of poverty and crime. In particular, while it would clearly be cruel and capricious to randomly impoverish persons or households in service of social science rigor, it is at least not cruel to randomly provide supplemental income to poor persons or households in order to determine if such changes in resources have important behavioral consequences.

In this article we will present evidence on the relationship between poverty and crime gathered from two randomized experiments, each conducted with samples of close to 2,000 persons who were released over a 6-month period in 1976 from the state prisons of Georgia and Texas. Under the name of the Transitional Aid Research Project (TARP), persons released from the prisons were assigned either to one of four treatment conditions or to one of two control groups. The treatments provided either eligibility for unemployment benefits at several levels or the alternative of job counseling. One of the two control groups was followed in detail while the other was followed only through administrative records.[2]

In the analysis reported here, we will pay particular attention to the impact of the TARP experimental treatments on arrests (proxies for participation in crime) over a 12-month period following release from prison. These treatments, in effect, substantially changed many ex-felons' income prospects, and the primary goal of TARP was to determine whether or not these efforts to reduce economic hardship in the period immediately after release from prison lowered participation in crime and, therefore, the likelihood of arrest.

Are ex-felons an appropriate group for the study of the relationship between poverty and crime? On the negative side, one might argue that they constitute so self-selected a portion of some broader population that any findings would be difficult to generalize. After all, they have presumably engaged to a greater or lesser degree in a "life of crime": that is, officially at least, why they were sent to prison. On the positive side, it can be argued that since these are persons who have already on previous occasions opted for illegal activities, a test of the effect of poverty on crime may be especially strong. If the provision of income to individuals with clear "criminal proclivities" can reduce participation in illegal activities, then the difference made in income would be especially compelling. We believe that ex-felons are a fully appropriate population to study and, in the pages to follow, will provide theory and data to show support of this view.

BACKGROUND OF THE TARP EXPERIMENT

A host of obstacles face ex-felons upon their release from prison (Lenihan, 1977; Irwin, 1970). These obstacles take a number of forms, and among the most formidable is extreme financial hardship. "Gate money" provided by the state is rarely sufficient for more than a few days of subsistence, accumulated earnings from prison work are usually little more than loose change, and eligibility for welfare or other transfer payments is almost nonexistent for the unmarried young man in his twenties who is the typical released prisoner.

---

[2] Greater detail on the treatment and control groups will be provided later in this article.

Employment prospects are equally grim. Few prisoners have attractive work histories, having spent very large proportions of their adult lives in prison. Correspondingly, they have few marketable skills, "human capital" or contacts that would facilitate the job search process. Moreover, the stigma of a criminal record hampers efforts to find legitimate employment. Indeed, given their lengthy day-to-day involvement with other criminals in prison and their record of previous criminal activity, a rational course of action might well lead directly to renewed participation in illegal activities (Becker, 1968; Ehrlich, 1973; Block and Heineke, 1975).

With these realities in mind, the beginnings of the TARP experiments can be found in the Manpower Development and Training Act of 1962 (and subsequent amendments) which gave to the Department of Labor a mandate to devise and provide programs responding to the employment needs of ex-prisoners. The Department initially launched manpower training projects within prisons and after release. Unfortunately, evaluations of these efforts found little evidence of success. The Department then turned to testing short-term, income-support strategies for the period immediately after release from prison. The first experimental program consisted of two kinds of interventions: special job placement and counseling for ex-prisoners, and eligibility for short-term transfer payments similar to unemployment insurance benefits[3] (Lenihan, 1977; Mallar and Thornton, 1978; Rossi et al., 1980). In the first instance, special placement and counseling services were thought to speed an ex-offender's reentry into the labor force and, consequently, reduce participation in criminal activities. In the second instance, modest transfer payments of about $60 a week for periods of up to 13 weeks (for those who could not find jobs) were thought to subsidize the job-search process and, in addition, to reduce the need to resort to crime as an alternative source of income.

After some initial pilot testing, the two interventions were field-tested in a randomized experiment conducted in Baltimore. Known as the LIFE experiment, approximately 400 men about to be released from state prisons were randomly assigned to either one treatment, both treatments, or neither treatment (thus, three experimental groups and one control group in a fully crossed design). Exprisoners were then followed for up to two years in order to gauge immediate and longer-term effects. The placement and counseling intervention apparently had no impact on re-arrests, but the payments-eligibility intervention reduced re-arrests for property crimes about 8% (the difference between the two groups eligible for payments and the two groups that were not eligible). While the reduction was statistically significant and encouraging, it was hardly definitive (Lenihan, 1977; Mallar and Thornton, 1978).

In order to better document the apparent success of the LIFE experiment and explore the mechanisms by which the reduction in re-arrests occurred, the TARP experiment was launched. Approximately 2,000 men and women in each of the states of Texas and Georgia who were about to be released from state prison were randomly assigned to four treatment groups and two control groups based loosely on the treatments delivered in the LIFE experiment.[4]

Table 1 shows that the three payment groups varied along two important dimen-

---

[3] Given the usual eligibility rules based on employment for the preceding five quarters (or so), most ex-prisoners are automatically excluded from unemployment payments even if they had been working immediately prior to their incarceration.

[4] As the second control group was not interviewed during the postrelease year but only followed through administrative records, no information is available on employment for this group. Hence, it is not considered in this analysis.

Table 1. Target and Actual Number of Persons in TARP Groups Achieved at End of Recruitment

TARP Group	Target No. in Each State	Actual No. in Georgia	Actual No. in Texas
Group 1: 26 weeks of payments, 100% tax	175	176	175
Group 2: 13 weeks of payments, 100% tax	200	199	200
Group 3: 13 weeks of payments, 25% tax	200	199	200
Group 4: job placement only	200	201	200
Group 5: interviewed controls	200	201	200
Total	975	976	975
Group 6: noninterviewed controls[a]			

[a] Randomly selected controls, whose postrelease behavior was monitored only through administrative records—e.g., state criminal justice computerized arrest records and unemployment insurance wage files. See fn. 4.

sions: the maximum number of weeks for which unemployment benefits would be provided and the tax on earnings from legitimate employment. Thus, ex-prisoners could obtain up to either 13 or 26 weeks of support, and their payments would be reduced either dollar for dollar (100% tax) or 25¢ on the dollar (25% tax). Georgia participants were eligible for payments of $70 a week and, for the 100% tax groups, forgiveness amounts of $13.75 (i.e., the first $13.75 of earnings had no impact on unemployment payments). Texas participants were eligible for $63 a week and, for the 100% tax groups, forgiveness amounts of $8.00. The job-placement treatment consisted of special counseling efforts and grants of up to $100 for the purchase of tools, special work clothes, and other work-related expenses. Finally, Table 1 indicates that, with a few trivial exceptions, the recruitment process in each state produced the desired number of participants in each of the groups. There was also no evidence that the assignment process was other than random.

To anticipate some important issues that will surface later, the TARP treatments differed in at least three important ways from the earlier Baltimore LIFE treatments. First, planned variation in weeks of eligibility was introduced into the TARP experiments in order to better determine the relative cost-effectiveness of different levels of support. If the experiments were successful in reducing rearrest rates, the question of cost would certainly rise to the top of the political agenda. Thus, some effort was made to see how sensitive the effects would be to variations on the number of weeks for which support was provided. Second, similar arguments were used to justify taxing earnings—and taxing them at two different rates. This is in marked contrast to the LIFE experiment in which there was, in effect, little or no tax on earnings. Finally, the LIFE experiment was administered on a day-to-day basis by a research staff under the direction of Kenneth J. Lenihan, while the TARP experiment was administered on a day-to-day basis by the staff of state employment offices, an alteration in administrative procedures that followed from a desire to "field-test" the treatment under conditions that likely would obtain should a TARP-like program eventually be adopted. Thus, in the LIFE experiment, ex-prisoners were in contact almost day-to-day with a research staff whose energies were devoted solely to the experiment, while ex-prisoners in the TARP experiments were in contact on a less-frequent basis with regular public servants who had a range of duties.

Data on the randomly assigned TARP subjects came from several different sources. Prison records were extracted for biographical material predating participation in the TARP experiment, criminal justice records were used to obtain an "official" reading on postrelease law violations, and unemployment insurance files were used to obtain data on earnings subject to unemployment insurance taxes. In addition, TARP subjects in groups 1 through 5 were interviewed immediately prior to release and, then, 3 months, 6 months, and 12 months after release. These interviews covered a host of postincarceration experiences including employment, marital status, living arrangements, major illnesses and accidents, consumer purchases, and the like. Eventually, all four data sources were merged into a single record for each TARP participant addressing the first 12 months after release from prison.

Despite the fact that each of the states organized its own data-collection activities, recruiting and training an ad hoc interviewing staff, high completion rates were experienced; the lowest completion rate for any wave of interviewing was 84%. While measurement left something to be desired in each state, internal evidence indicated that the main policy variables—employment, arrests, and experimental payments—were fairly well measured (see Rossi et al., 1980).

THEORETICAL ISSUES AND MODEL SPECIFICATION

The empirical world rarely comes organized into neat cubbyholes of sociological phenomena, economic phenomena, psychological phenomena and the like. Moreover, it would be hard to argue that any particular social science discipline has had outstanding success in explaining criminal behavior, particularly with respect to the relationship between poverty and crime. Consequently, the theoretical underpinnings for our model specifications will be eclectic, borrowing most heavily from criminology, sociology, and economics.

It is, however, important to understand from the outset that the backgrounds of TARP participants rule out a priori the application of many popular perspectives on crime. In effect, a number of exogenous factors cited by earlier researchers are for all practical purposees held constant. First, all participants in the TARP experiment had just finished at least one year in state prison and most had rather extensive criminal records. In other words, all had shown a considerable talent for getting into trouble with the law. Thus, it is virtually impossible (with this sample) to consider in some fundamental sense why some individuals are arrested while others are not; we are limited to the question of whether past criminal proclivities can be altered. Second, most TARP participants returned to precisely the same environments that presumably had spawned their earlier problems. By and large, they were again exposed to the same mix of family experiences, friendship patterns, economic opportunities, and police practices that had earlier led to their incarceration in a state prison for terms of a year or more. Finally, TARP participants were drawn from the lowest strata of society, having poor educational backgrounds, scant job experience, and meager earnings histories. Few sons and daughters of the middle class were represented.

In short, TARP participants were rather homogeneous with respect to social class, criminogenic background, and exposure to other felons. Moreover, what the TARP experiment is best designed to consider is whether relatively modest changes in an ex-prisoner's economic circumstances affect postprison behavior. These constraints imply the need to focus on the *short-run* interplay between transfer payments, employment, and arrests. Well-known theories resting on such concepts as subcultures, differential association, and blocked legitimate opportunities (see Glaser, 1979, for a recent review) are essentially irrelevant in the sense that variables measuring these concepts are, for TARP participants, effectively constants.

The social science theoretical traditions to which we must turn in order to construct models of the interplay between TARP payments, employment, and crime are to be found primarily in economics and sociology. From the economists we obtain a utilitarian perspective on how choices are made between such things as the alternative sources of income, particularly those of employment and crime. Sociological theory provides us with an understanding of employment variables without monetary equivalents (Heineke, 1978:11–4), including such features as social status, the interpersonal context of employment, and the like.

Utilitarian perspectives on crime rest primarily on the efforts of economists working within neoclassical traditions (e.g., Becker, 1976; Sjoquist, 1973; Ehrlich, 1973; Block and Heineke, 1975; Heineke, 1978) although some sociologists have made contributions (e.g., Letkemann, 1973; Richards et al., 1979). While utilitarian approaches to criminal behavior are quite diverse, the following (Heineke, 1978:1–2) overview captures much of what such perspectives share.

> Economic models of criminal behavior take as given those influences in the personal and social backgrounds of individuals that determine "respect for law," proclivities to violence, preferences for risk and other behavioral characteristics held to be determinants of criminality. These models are based on characteristics of individuals which are alleged to be common not only to large classes of offenders, but to large classes of economic agents in general. In a sentence, the models of economic choice theory, of which the criminal choice is a special case, hypothesize that all individuals, criminals and non-criminal alike, respond to incentives; and if the costs and benefits associated with an action change, the agent's choices are likely to change. More specifically, these models postulate that the decision to commit an illegal act is reached via an egocentric cost-benefit analysis. As is implicit in this statement, the expected benefits and costs associated with an illegal act may contain both monetary and psychic elements. But by treating the individual's "taste for crime" as a datum, one may build a theory of criminal behavior based on the opportunities confronting the potential offender.

Within this broad framework, Heineke (1978) distinguishes two general classes of models: portfolio models and time allocation models. The former address what portion of a decision maker's *wealth* is risked in criminal activity and, therefore, requires that all returns from illegal activity be expressed in monetary equivalents. The latter notes the importance of psychic consequences and the fact that illegal activities are time consuming and, therefore, views participation in illegal activities as, formally, "a labor supply problem with uncertain consequences" (Heineke, 1978:3). For reasons that will soon become apparent, time-allocation approaches are more appropriate for the concerns of this article.

Within the time-allocation perspective, individuals are assumed to face an ongoing set of choices between illegal and legal activities, and individuals will participate in illegal activities as the anticipated returns from illegal actions exceed the anticipated returns from legal actions. Incentives include such things as money, possessions, and excitement, while disincentives include such things as arrests, imprisonment, and guilt. In the usual formal approaches (e.g., Ehrlich, 1973), it is assumed that the returns from illegal activities are uncertain. The uncertainty results from an inability to know in advance whether illegitimate actions will lead to sanctions from the criminal justice system, implying that the costs associated with crime are, in effect, weighted by the probability of experiencing sanctions. For example, if the likelihood of arrest is small, even the most Draconian punishments will have little impact on the anticipated costs of crime.

A critical point often overlooked in more vulgar translations of formal utilitarian approaches is that individual decision making proceeds on a marginalist basis. This means that the *incremental* returns of the next action are compared to the *incremental* costs and that, therefore, it is essential to know the decision maker's circumstances at the point when a decision needs to be made. For example, it is common to assume that each increment in income is worth less to the decision maker than the last (that is, the assumption of diminishing marginal utility); an *additional* $60 a week will mean a lot more to a person earning $100 a week than to a person earning $500 a week.[5]

Given that legal and illegal activities are not completely mutually exclusive, individuals must continually decide what mix of activities is appropriate. Within formal utilitarian approaches, individuals are assumed to select some mix of activities yielding the maximum anticipated returns conditional on such practical constraints as the amount of time and other resources available (that is, budget constraints). For some decision makers this may mean that crime becomes virtually a full-time job; for others this may mean that criminal activities are at most an occasional source of supplemental income. And all manner of combinations in between are fully possible. What matters, then, is the balance of incentives coupled with practical constraints.

---

[5] This also implies that an individual is "risk averse" (Barnett, 1973:105–9). The expected returns from a "gamble" must exceed (somewhat) the value of resources invested—or the gamble will not be taken. We will have more to say about this later.

Given the constraints, the mix of legal and illegal activities producing maximum returns implies that no other mix can yield greater returns, and, when this mix is achieved, individuals are said to be "at the margin." It cannot be overemphasized that the empirical predictions from the formal utilitarian models apply *only at the margin*. Thus, the implications of a change in the probability of sanction, for instance, can be formally assessed only for the mix of activities from which maximum returns are obtained. The predictions do not apply to individuals who, for one reason or another, are not at the margin.

While the formal utilitarian perspectives typically emphasize the role of monetary incentives or incentives with monetary equivalents, there is, nonetheless, some concern for factors that are perhaps of more interest to sociologists. Block and Heineke (1975), for example, "build in" the effects of the psychic consequences of criminal activities. Human-capital theories provide for the possibility that the skills required for both legal and illegal activities can be enhanced through a variety of formal and informal educational processes. (See Rosen, 1977, for a recent review of the human-capital literature.) Becker (1971) and Arrow (1972; 1973), among others, have provided models that can be used to explain, in a not entirely satisfactory fashion, the kinds of discrimination faced by ex-prisoners in the job market. Finally, the job search literature (for a recent review see Lippman and McCall, 1976a; 1976b; 1979) can be applied not only to efforts to find legitimate employment but also to finding illegitimate "employment."

The fact that utilitarian models allow for variation in tastes only as a residual causal variable is, from a sociological point of view, a major flaw. However, a more serious deficiency for our purposes lies in the treatment of legitimate employment almost exclusively as a source of income. The sociological perspective on employment provides a number of additional dimensions. First, being employed at any job is a valued status, as the occupational-prestige studies have shown (e.g., Nock and Rossi, 1978). Studies of the unemployed (Hyman, 1980) have documented the deleterious effects of unemployment on self-esteem and morale. Indeed, an analysis of the social-psychological impact of unemployment among TARP members (Liker, 1980) shows that ex-felons are not immune to the same processes. Second, employment on the usual legitimate job implies being placed in contact with fellow workers, providing an interpersonal environment that may lead to new social circles, exposure to different values concerning the legitimacy of illegal activities, and so on. Finally, the returns from employment often increase with seniority; continued employment increases the stake involved in loss of employment (which economists also claim).

Thus, these briefly outlined theoretical considerations provide the background for the construction of a testable model representing the competitive interplay among work, payments, and crime—all, the endogenous outcomes of primary interest. At the outset, however, it is necessary to deal with a serious measurement issue. While we have been stressing time allocations as a "labor supply" problem, we have no direct measure of time allocated to crime. We are forced, therefore, to rely on the *number* of arrests recorded by the criminal justice system over a 12-month follow-up. We are fully aware of the loose fit between arrests and participation in illegal activity, but—at least from a policy point of view—arrests are a popular measure of recidivism. Moreover, since we are concerned with *changes* in criminal activity in response to the TARP experiment, we are able to tolerate either random measurement error and/or consistent underestimates (or overestimates) when arrests are endogenous.

Two kinds of arrests will be used: arrests for property crimes and arrests for nonproperty crimes.[6] This distinction follows from the desire to distinguish those crimes that most likely have an earnings motive from those crimes not likely to produce income. As a measure of time invested in legal income-producing activities, we will rely on the number of weeks worked during the postrelease year, as reported in follow-up interviews with TARP participants.

---

[6] "Nonproperty" crimes are a heterogeneous class ranging from murder and serious assault to

An additional endogenous variable, jail and prison time, must be added to the original three as it often follows from arrests and, in turn, affects both payments and employment. Clearly, while sitting in jail (usually awaiting arraignment or trial) or in prison, after sentencing, an individual can neither work, engage in the usual criminal activities, nor receive payments.

Finally, the TARP treatment for which randomization was undertaken was *eligibility* for unemployment compensation. And eligibility, by itself, probably has little meaning unless translated into measures of how much money was actually given to TARP participants; it is, presumably, the money, not the eligibility per se, that is important. Consequently, our fifth, and last, endogenous variable is the number of dollars given to TARP participants over the 12-month follow-up.[7]

It should be apparent that a number of causal links are likely to exist among the five endogenous variables. *By design*, payments were reduced by employment and eliminated if the ex-prisoner was incarcerated. Prisoners behind bars could not seek employment and were not subject to additional arrests. Arrests for both property and nonproperty crimes should increase the number of weeks spent in jail or prison. These relationships are shown in Figure 1 along with causal effects that require somewhat more explanation.

First, if one assumes a diminishing marginal utility from income, TARP payments should reduce the time allocated to both employment and income-producing crime. Thus, other things being equal, TARP participants who receive greater amounts of money through TARP should evidence fewer arrests for property crime and fewer weeks worked.

The assumption of a diminishing marginal utility from income is common within the broader framework of neoclassical traditions (e.g., Nicholson, 1978:60–76) and is equivalent in this instance to saying that individuals are risk-averse (Barnett, 1973:105–9). By assuming that ex-prisoners are risk-averse, we are claiming that there is nothing basically unusual about the way they make decisions under uncertainty. However, we must also acknowledge that some economists who study crime resort to alternative assumptions and, in particular, that at least some individuals with criminal proclivities are risk-prone. (For the implications of different assumptions about risk, see Heineke, 1978.) We find such alternative conceptualizations vulnerable to the charge of ad hoc theory construction and prefer to continue with the more general perspective that ex-prisoners allocate their time through mechanisms common in the general population.

Second, TARP payments increase the opportunity-costs of arrest because recipients have more to lose (that is, the TARP payments). Consequently, individuals who receive support through TARP should experience fewer arrests for both property *and* nonproperty crimes.

In "real life," of course, it was not arrests per se but, rather, incarceration that eliminated TARP payments. Moreover, some ex-prisoners no doubt received TARP payments despite brief periods of incarceration, perhaps prior to trial. However, the critical point is that TARP participants were required to appear at the state unemployment office to certify their eligibility, and it is probably reasonable to assume that they believed that payments would not be made to incarcerated individuals. At the very least, the program was introduced to participants in such a manner that this contingency was highlighted.

---

disorderly conduct. Most of the charges included under this rubric consist of quite trivial charges.

[7] It should already be apparent that some important substantive and statistical decisions have been made. In particular, despite the fact that data were available at least by quarter (that is, three-month intervals), we are aggregating up to a year. In fact, we estimated models by quarter for the full year in part to consider lagged effects. However, these efforts were plagued with serious statistical problems, and we will not consider the results here (see Rossi et al., 1980).

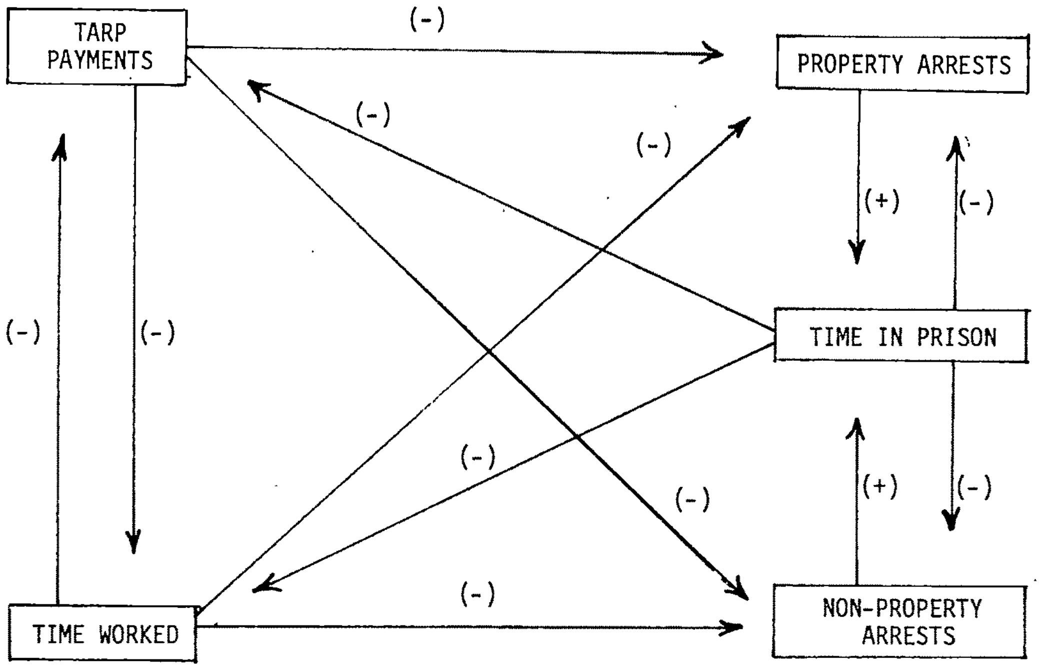


Figure 1. Hypothesized Relationships among Endogenous Variables in TARP Counterbalancing Model.

Third, employment should reduce the number of property and nonproperty arrests because (a) time is withdrawn from illegal activities, (b) the opportunity costs of arrest are increased as a job may now be lost, (c) human capital for legitimate pursuits is enhanced, and (d) preferences for legitimate activities are shaped. In addition, employment should reduce the number of property arrests (only) by providing another source of income (thereby reducing the marginal value of earnings from illegal activities).

The causal relationships just described can be elaborated in a number of interesting ways. For example, it was intended that TARP payments subsidize job search, enabling recipients to obtain positions with better wage rates and/or better working conditions. However, the theoretical background outlined earlier also suggests that TARP payments may subsidize a better search for illegal activities, with TARP recipients engaging in more lucrative and less risky illegal activities. While our data do not allow an analysis of the latter outcome, we can show that ex-felons who received payments did get better jobs (Rossi et al., 1980).

Several important implications follow from the proposed causal relationships among our five endogenous variables. Perhaps most important, TARP payments should have both a positive and a negative impact on property and nonproperty arrests. TARP payments should *directly* reduce the number of property and nonproperty arrests as discussed above. But, in addition, as TARP payments should reduce employment and as reduced employment should lead to a greater number of arrests, TARP payments should *indirectly* increase the number of property and nonproperty arrests. This counterbalancing model implies that a failure to formally consider both the direct and indirect effects may produce misleading results. In particular, the direct positive effects of the TARP payments may be *cancelled out* by the indirect negative effects of the TARP payments.

It should be stressed that many of the arrows drawn in Figure 1 are not speculative in character, but are built into the design of the experiment. Thus, it is the 100% tax on payments that defines the links between employment and TARP payments.[8] Similarly, rules about payment eligibility excluded payments to persons who were in jail or prison or who were too ill to work.

The reader may recall that the earlier Baltimore LIFE experiment operated under different rules and under a different manner of administration. Hence, a modelling of the Baltimore LIFE experiment would remove some of the links shown in Figure 1. In particular, because the experimenters were meticulous in administering a very generous tax rate, the link between employment and payments would have to be modified to indicate the employment did not lead to a loss in payments.

For those who prefer mathematical notation to path diagrams, we now show an abbreviated version of the five-equation nonrecursive model whose parameters we will estimate.

Number Property Arrests = f(TARP payments, employment, incarceration, exogenous variables) (1)
Number Nonproperty Arrests = f(TARP payments, employment, incarceration, exogenous variables) (2)
Number Weeks Employed = f(TARP payments, incarceration, exogenous variables) (3)
Number Weeks Incarcerated = f(property arrests, nonproperty arrests, interaction terms) (4)
TARP Payments = f(interaction terms, exogenous variables) (5)

There remains, of course, the question of what exogenous variables to include. Limited space prevents any extensive discussion of exogenous variables, equation by equation, but there are no surprises. For the first three equations (property arrests, nonproperty arrests, and employment), the exogenous variables fall within four broad groupings: indicators of economic need (for example, gate money and savings), indicators of human capital (for example, education, physical handicaps), indicators of past commitment to the given activity (for example, number of prior convictions), and indicators with unclear theoretical status, but with a history of empirical usefulness (for example, age). It is perhaps worth stressing that the exogenous variables were linked to particular endogenous variables a priori. For example, human capital variables speaking to productivity in legitimate employment were taken to be direct causes of the number of weeks employed. They were *not* taken as direct causes of (reduced) participation in illegitimate activities (that is, the number of property and nonproperty arrests), but could nevertheless affect participation in illegitimate activities indirectly *through* employment. Similarly, measures of past involvement in (and, perhaps, "tastes" for) illegitimate activities were seen as direct causes of arrests for property and nonproperty offenses but were *not* taken to be direct causes of (un)employment. Nevertheless, these measures could affect employment indirectly *through* incarceration resulting from arrests. In any case, specification issues are addressed in somewhat more detail in the next section as findings are considered, and a more thorough discussion can be found in Rossi et al. (1980).

For the last two equations (weeks incarcerated and TARP payments), the exogenous variables reflect either the experimental design (in the case of the last equation) and/or interaction terms. For example, while an ex-prisoner's criminal record may affect the sentence given for a crime, the impact is *conditional* on an arrest. Consequently, the impact of prior record must be represented as a product variable (arrest × prior record). Similarly, the impact of employment on TARP payments is *conditional* on being in one of the experimental groups eligible for support. Again, a product variable is required. We will have more to say about such issues later.

STATISTICAL PROCEDURES

The power of the TARP research design and the richness of the data provide both an unusual analytic opportunity and some difficult analytic problems. Besides the fact that the experimental intervention introduces an important exogenous factor into the system, the other major asset is, perhaps, that the experiment was conducted independently in two states. This means that if one state is taken as the initial site, the other state can be taken as *replication.*

---

[8] There is good evidence that Group 3, which operated under a nominal tax rate of 25%, did not know about the tax rate and, hence, behaved as if they were under the 100% rate.

The replication is critical in part because, even with the aid of theory from economics and sociology, it proved impossible to actually estimate models that were fully consistent with the theory. The major difficulty was that we often had several highly correlated indicators of the same conceptual variable and to have included all of the indicators would have introduced serious multicollinearity. Thus, painful choices had to be made,[9] and these risked capitalizing on type I and type II errors. Consequently, we "fine tuned" the model on the Texas data and then moved it over to the Georgia data virtually intact. With one exception to be discussed later, the only difference between the model specifications in the two states was the availability of comparable variables. For example, IQ scores were readily available for the Texas sample but not for the Georgia sample. The basic point is that if the results in Georgia replicated those in Texas, the impact of the Texas "fine tuning" was probably not serious. (This neglects, of course, the impact of similar specification errors in both states.)

The nonrecursive model required that ordinary least squares be abandoned, and we proceeded with two-stage and three-stage least squares. The former is more robust to specification errors, but is less efficient. The latter is less robust to specification errors, but is more efficient. By and large, the results from the two procedures were much the same, which we take to mean that there are perhaps not serious specification errors. We will, however, report the three-stage least-squares results because its greater efficiency on the average yields (as expected) smaller standard errors.[10]

There remains one problem, however, for which no fully acceptable solution could be devised. All of the endogenous variables are bounded by zero; no negative observations are possible. This implies that the residuals around the regression hyperplane cannot be distributed normally, or even symmetrically. The result is that, in principle, all of the regression coefficients are biased and inconsistent (Tobin, 1958). For single-equation models, relatively simple procedures exist that yield consistent estimates (Amemiya, 1973; Heckman, 1979; Ray et al., 1980). For simultaneous-equation models, however, existing alternatives are only practical for very simple models (e.g., Sickles and Schmidt, 1978). Hence, two fall-back strategies were applied. First, we transformed our most serious truncated endogenous variables into dummy variables (risking heteroskedasticity and, therefore, both inefficiency and biased standard errors) and reestimated the five-equation model. The overall story was much the same, although, as expected, the (biased) standard errors were typically inflated. Second, we undertook a number of transformations of the endogenous variables in an effort to capture the functional form implied by the truncation.[11] Again,

---

[9] The option of confirmatory factor analysis (Jöreskog and Sörbom, 1979) was ruled out as impractical given the large number of equations and variables.

[10] Product variables for which one of the components is an endogenous variable are also vulnerable to simultaneous equation bias. With the aid of reduced form equations with the exogenous component of the product variable removed, instruments for the product variables were constructed and inserted where required. More generally, all of the equations met the order and rank conditions necessary to obtain unique parameter estimates. Finally, it is important to emphasize once again that *any* structural equation model is only as good as its underlying theory and the parameter restrictions implied. In other words, all of our parameter restrictions (that is, to zero) were theoretically based and, should one disagree with our theoretical model, there is no guarantee that our results would hold up under alternative formulations. However, it may be some comfort to know that we did do extensive work testing the sensitivity of our key findings to modest alterations in our specifications (particularly with respect to the exogenous variables) and the major findings were much the same.

[11] Our efforts in dealing with the truncation problems were enormously aided by conversations with Thomas Cooley, Ann Witte, James Heckman, Peter Schmidt and Charles Mallar.

Table 2. Analysis of the TARP Benefits Received (in Dollars)

	Texas		Georgia	
Variable	Coefficient	t-value	Coefficient	t-value
Intercept	0.08	0.66	2.32	0.15
Group 1 (26 weeks/100%)	1,681.12	10.82	1,691.11	7.60
Group 2 (13 weeks/100%)	990.90	4.37	1,021.99	5.27
Group 3 (13 weeks/25%)[a]	738.88	4.67	621.80	3.80
Group 1 × Employment	−17.08	−2.86	−11.59	−1.03
Group 2 × Employment	−7.93	−1.08	−10.65	−1.21
Group 3 × Employment	−0.35	−0.07	3.90	0.58
Group 1 × Jail	−15.23	−1.64	−26.93	−2.02
Group 2 × Jail	−18.29	−1.81	−14.59	−1.07
Group 3 × Jail	−0.00	−0.00	28.21	1.60

[a] Deleted groups are groups 4 and 5.

the story held firm. Thus, we are inclined to believe that the biases created by the truncation are probably not serious although we want to stress that fully appropriate adjustments were *not* undertaken.

FINDINGS AND DISCUSSION

In the interest of space, we have not included tables showing the reduced-form results for Texas and Georgia. With two important exceptions, the substantive findings from the reduced forms are uninteresting, especially in the context of the structural-form results to be reported shortly. The first major finding is that membership in any of the three experimental groups eligible for payments had *no* statistically significant impact on either property arrests or nonproperty arrests. Because in the reduced forms the properties of the randomized design still hold, the null findings cannot be dismissed as a function of specification error or multicollinearity. In short, when the data are analyzed as a randomized experiment with the *orthogonal* treatments defined as membership in one of the experimental groups or the control group (that is, *eligibility* for payments), the number of property and nonproperty arrests is *not* reduced.

Second, the reduced forms reveal a statistically significant (at the .05 level) and nontrivial impact of TARP eligibility on employment. As predicted, there is evidence for a substantial work disincentive—averaging about 5 weeks' less work over the course of 12 months. Had we stopped here, the findings would have been disappointing indeed; the TARP treatment produced no reduction in arrests but did reduce the effort invested in legal employment.

Results from the Payment Equation

Table 2 shows the structural equation results for the equations for Texas and Georgia in which is examined the amount of TARP payments received. It is immediately apparent that in both states the experiments were implemented largely as designed. In both states the intercept is virtually zero with group 1 members receiving about $1,700, group 2 members receiving about $1,000, and group 3 members receiving about $700. With two exceptions, all of the interaction effects (reflecting the TARP design) are in the predicted direction, but only four are statistically significant at the .05 level for a one-tail test. Fortunately, the lack of statistical significance should be no grounds for serious concern because the product variables are, not surprisingly, highly correlated with one another.[12] In summary, the results from our first pair of equations are sensible and consistent with the TARP research design. Were this not the case, it would be almost pointless to proceed.

---

[12] It should also be noted that a fully proper specification would have used interaction terms with *earnings*, not weeks employed. However, the wage data were somewhat suspect and in any case were plagued with large numbers of missing observations.

Table 3. Analysis of the Number of Weeks of Employed

	Texas		Georgia	
Variable	Coefficient	t-value	Coefficient	t-value
Intercept	23.75*	6.66	18.79*	4.86
Weeks incarcerated	−1.69*	−9.89	−1.32*	−4.24
TARP payments (hundred $)	−0.64*	−6.49	−0.68*	−7.79
Male (dummy)	10.99*	5.74	8.56*	3.38
$ at release (hundreds)	−0.21*	−1.68	−0.35*	−1.89
$ savings (hundreds)	0.04	0.59	−0.20	−2.57
$ debts (hundreds)	−0.01	−0.12	0.04	0.76
Live w/spouse (dummy)	2.37	1.44	0.50	0.37
Physical handicap (dummy)	0.00+	0.00+	−1.44	−1.37
Number of dependents (integer)	−0.32	−1.01	−0.80	−2.34
Weeks sick or in hosp.	−0.87*	−8.67	−0.51*	−6.05
Black (dummy)	−0.76	−0.90	−0.72	−0.78
Chicano (dummy)	−1.07	−0.97	a	
Unemployment rate (%)	−0.65	−2.05	0.00+	0.21
Previous military service (dummy)	−1.48	−1.52	−0.63	−0.59
Achievement test score	0.00+	0.21	b	
IQ test score	0.05	1.60	b	
Years of education	−0.02	−0.14	0.20	1.00
Age 21–40 (dummy)	1.30	1.46	−0.05	−0.05
Employed at pre-TARP arrest (dummy)	1.88	2.58	1.28	1.34
Job arranged at release (4 levels)	0.96*	2.34	4.63*	4.94
Driver's license (dummy)	1.34	1.84	1.16	0.98
Prison vocational training (dummy)	0.26	0.31	1.77	1.61
Released on parole (dummy)	3.19*	3.70	3.56*	3.60
Union member	b		0.12	0.12

* Both coefficients significant at $p < .05$ for one-tail test.

a: No variance. b: Variable not available.

Results for the Number of Weeks Employed

Table 3 shows the two structural equations in which the number of weeks employed is examined. Given the large number of coefficients estimated and the fine tuning applied to the Texas equation, we will proceed conservatively. We will focus on variables that attain statistical significance at the .05 level for one-tail tests (as the hypotheses were one-tailed) in *both* equations. While the probability of observing two coefficients at the .05 level is not .0025 (.05 × .05), in part because of the many tests applied, the chances of type I errors are, nevertheless, dramatically reduced.

To begin with, for each week in jail or prison, the number of weeks worked decreases by about 1.5 weeks. While this is not surprising, the fact that the absolute values of the regression coefficients are greater than 1.0 implies that, often, loss of a job is involved. As suggested earlier, apprehension for illegal activities may well lead to substantial opportunity costs. It is instructive to compare the impact of incarceration with that of the number of weeks sick or in the hospital. Each week of illness and/or hospitalization reduces the number of weeks worked by about three-quarters of a week; the absolute values of the regression coefficients are less than 1.0. Perhaps illness is less likely to mean loss of a job or perhaps ex-prisoners are able to work some of the time when they are "under the weather."[13]

There seems to be little doubt that TARP participants respond to work disincentives. Every $100 of TARP payments reduces the number of weeks worked by about two-thirds of a week. This implies that TARP participants who received maximum benefits of over $1,500 worked an average of 10 less weeks over the 12-

---

[13] It is important to keep in mind that such point estimates have confidence intervals. Thus, the fact that the coefficient is not exactly 1.0 should be no cause for concern.

month interval. In addition, for each $100 dollars that ex-prisoners had on hand at release (for example, gate money, prison earnings, etc.), work effort was reduced by about one-quarter of a week. However, TARP participants rarely had more than a few hundred dollars at the time of release so that the overall impact was typically quite small. Finally, neither the size of savings accounts nor the amount of debt seemed to make any difference in work effort.[14] In summary, at least in the context of employment, there is some support for the assumption of a diminishing marginal utility from income and, as predicted, a nontrivial work disincentive produced by TARP payments.[15]

Three other variables are statistically significant in both equations. Men work an average of about 10 weeks more over the course of a year, and individuals who are more likely to have a job arranged at time of release work several weeks more. Finally, ex-prisoners released on parole work over 3 weeks more during the 12-month interval. Given current skepticism about the efficacy of parole, even the rather modest impact revealed here should give some critics pause. We reasoned a priori that since parole officers often make a concerted effort to have their parolees actively seeking work, an impact on employment would be found. Moreover, our specifications assume that, if being on parole reduces the number of arrests, the impact is mediated by employment. That is, there is no *direct* relationship between parole status and arrests.[16]

Turning briefly to some null findings, it appears that our human-capital variables have little impact. While this would be stunning for a cross-section of the American work force, ex-prisoners are hardly such a cross-section. There is not a great deal of variance in human capital and, for the kinds of jobs ex-prisoners typically seek, the usual human-capital variables may be of limited relevance.[17] Education, reading ability, and prior experience are probably of little importance to employers screening people to wash cars, pick lettuce, sweep floors, and carry heavy boxes. And, as all have criminal records, the one important variable on which employers *might* screen prospective employees is actually constant.

There is also no evidence that members of minority groups fare any worse in the job market. Again, for a cross-section of the American work force, this would be surprising. But one must keep in mind that the kinds of jobs sought by ex-prisoners are at the very bottom of the status heap where employers are unable to routinely pick and choose. In short, for the least desirable jobs society provides, there is apparently equal opportunity.

In this context, however, the fact that men work more than women requires further consideration. The number of women in the sample (126) is far too small to permit a compelling analysis of male-female differences in employment patterns, but there is some suggestion that while being married increases the labor force participation of men, it decreases the labor force participation of women (Jurik, 1979). In other words, although some form of discrimination may well be involved, one cannot rule out a host of other factors resting on such things as the traditional divisions of household labor (Berk and Berk, 1979).

Perhaps the major disappointment in Table 3 is the failure of the overall unemployment rate for the county to which the TARP participant was released to consistently affect employment. The predicted impact surfaces in Texas but not in Georgia. Of course, the overall unemployment rate is at best a very indirect measure of the demand for the labor of ex-prisoners, but we hoped for better.

---

[14] These variables were drawn from the interviews and besides showing little variance were subject to serious measurement error. While ex-prisoners are apparently able to report pretty accurately the amount of money in their possession as they left the prison, their ability to remember the size of their savings accounts (for the few that had them) or their debts was suspect.

[15] If one focused *only* on the employment equation and conceptualized the TARP payments as a lump-sum transfer (that is, an increase in wealth) the decreased work effort might well reflect an increased demand for leisure. However, this appears to be inconsistent with the results of the estimated equation for nonproperty crimes (presumably a "leisure" activity).

[16] Tests of this assumption proved our a priori conception of parole to be correct.

[17] It is probably worth noting in passing that TARP participants had average IQ's.

Results for the Number of Weeks in Jail or Prison

Building causal models for the number of weeks in jail or prison was fraught with problems. To begin with, once we included the obvious predictors of the number of property and nonproperty arrests, all other regressors had to be added as interaction effects with the initial pair. As mentioned before, such variables as prior record should affect incarceration, conditional upon arrest. However, the requisite product variables were necessarily highly correlated with the number of property and nonproperty arrests which in practice meant that only two interaction terms could be inserted. We settled on interactions between arrests and whether the individual was a first offender, after trying several related possibilities in Texas (we did not experiment with the Georgia equation).[18]

At first blush, this might imply that important causal variables were excluded. However, the reduced forms suggested otherwise. The R^2 for Texas was .12 and the R^2 for Georgia was .07, and the reduced forms contained a number of predictors that on theoretical grounds should have been important: number of prior convictions, being on parole, race, and the like. In short, for both Texas and Georgia, but especially for Georgia, we had very little with which to work.

Table 4 shows the structural form results and not a single causal variable is statistically significant in both equations (at the .05 level for a one-tail test).[19] Nevertheless, there is some evidence that a greater number of property arrests leads to a greater number of weeks behind bars. In Texas, the regression coefficient implies that for every property arrest about 17 weeks of incarceration follow (t = 7.89) and, in Georgia, the regression coefficient implies that for every property arrest about 10 weeks of incarceration follow (t = 1.36). In addition, all of the other regression coefficients—with one exception—have the predicted sign. In short, while the effects are small, there are at least no serious anomalies.[20]

To summarize, our data effectively stacked the deck against finding a large number of statistically significant effects. We suspect that greater success would have been attained if the follow-up had lasted 24 months and, in addition, if we had had access to variables more relevant to preconviction jail terms (for example, whether or not bail was raised).

Results for the Number of Property Arrests

With the first three structural equations discussed, we are now ready to deliver the punch line: what is the relationship between economic need and recidivism? And, in particular, did the TARP payments reduce arrests as predicted?

Table 5 shows the results for the number of property arrests. Five regression coefficients attained statistical significance at the .05 level (one-tail test) in both equations and continuing with our conservative strategy we will focus primarily on these.

---

[18] We also tried some interaction terms with endogenous regressors, but the multicollinearity problems were even worse.

[19] We tried specifying and estimating separate equations for the time in prison versus the time in jail, but that proved impractical for a number of reasons. For example, the R^2 from the reduced-form equation for time in jail was so low that the resulting instrument had virtually no variance.

[20] We are somewhat at a loss to explain the disappointing results in Table 4, although we have several speculations. First, the vast majority of incarcerations were for relatively minor offenses and the time in jail often reflected time awaiting an initial hearing before the magistrate, the time before bail was raised, or the time before trial. That is, the endogenous variables are heavily laden with preconviction and, therefore, presentencing experiences. It is probably fair to say that few of our exogenous variables are likely to be especially relevant to these sorts of jail terms. Second, much of the variance in time behind bars for those individuals who were arrested can be attributed to relatively few individuals who committed more serious crimes and, in addition, who committed them early enough in the 12-month interval to be fully processed by the criminal justice system. In other words, exogenous variables that *may* have had important effects at the sentencing stage were relevant for only a rather small number of offenders.

Table 4. Analysis for Number of Weeks in Jail or Prison

	Texas		Georgia	
Variable	Coefficient	t-value	Coefficient	t-value
Intercept	0.67	0.92	0.24	0.32
Number of property arrests	17.09	7.89	10.30	1.36
Number of nonproperty arrests	0.91	0.53	0.48	0.09
First offender × property arrests	−5.63	−1.64	7.04	0.69
First offender × nonproperty arrests	−2.05	−0.49	−3.04	−0.44

To begin, it seems apparent that, within the structural equation framework we have proposed, TARP payments *do* reduce the number of arrests. For Texas, each $100 reduces the number of property arrests by .02 (t = 3.19). This implies that for TARP participants receiving payments near the upper bound permitted by the program, the number of arrests was decreased by .30. For Georgia, every $100 reduces the number of property arrests by .01 (t = 2.21). Again, near the upper program bound of $1,500, the reduction in the number of arrests is about .15. In short, whatever the causal mechanisms (for example, increased opportunity costs, the diminishing marginal utility of income), TARP payments appear to work as predicted.

The number of weeks employed also has the predicted effects. In Texas, each week of employment reduces the number of arrests by .03 (t = 5.15). Thus, individuals who are employed over most of the 12-month interval can be expected to show about 1.5 fewer arrests for property crimes. In Georgia, each week of employment apparently reduces the number of property arrests by about .02 (t = 6.17). Over the course of a 12-month interval, individuals who are employed virtually full time can be expected to experience about 1 less arrest for property crime. Thus, once again, our predictions are supported, although—at this point—almost any set of several competing causal explanations may be involved: increased opportunity costs, diminishing marginal utility of income, increased human capital from legitimate activities, alteration in preferences, and the like. We will have more to say about these issues shortly when nonproperty crimes are considered.

The remaining three findings are at least superficially straightforward and need little elaboration. Men experience on the average about .30 more arrests for property crimes. This mirrors much of the criminology literature although the causal mechanisms are far from clear. The same point holds for the effect of age. Here, each year decreases the number of prop-

Table 5. Analysis of the Number of Property Arrests

	Texas		Georgia	
Variable	Coefficient	t-value	Coefficient	t-value
Intercept	1.19*	5.40	0.79*	5.75
Weeks employed	−0.03*	−5.15	−0.02*	−6.17
Weeks incarcerated	−0.01	−0.86	a	
TARP payments (hundred $)	−0.02*	−3.19	−0.01*	−2.21
Male (dummy)	0.28*	2.33	.32*	3.22
Age (years)	−0.01*	−1.98	−0.01*	−1.86
$ at release (hundreds)	−0.00+	−1.21	−0.00+	−0.62
$ savings (hundreds)	0.00+	0.45	−0.00+	−1.47
$ debts (hundreds)	0.00+	0.18	0.00+	0.43
Live w/spouse (dummy)	0.09	1.03	−0.01	−0.36
Physical handicap (dummy)	0.02	0.30	−0.02	−0.74
Number of dependents	−0.02	−1.01	−0.02	−2.19
No. of prior property convictions	0.02	1.49	0.04	1.54
Weeks sick or in hosp.	−0.03*	−3.96	−0.02*	−3.51

* Both coefficients significant at $p < .05$ for one-tail test.

a: Dropped because of multicollinearity.

erty arrests by about .01; for many, crime is a bit like a bad case of acne. Finally, each week of illness or time in the hospital decreases the number of property arrests by .03 in Texas and .02 in Georgia.

It may also be worth noting that two other variables perhaps have "real" effect but, nevertheless, fail to meet our conservative criterion for interpretation. There is a hint that a greater number of previous convictions for property crimes increases the number of property arrests and that those TARP participants with a greater number of dependents experience fewer property arrests.

Finally, a brief word about the exclusion of weeks incarcerated from the Georgia equation. As a consequence of the very low R^2 in its reduced-form equation—and the fact that the very few variables that had any predictive value were also strong predictors in the reduced forms for the other endogenous regressors in the equation (payments and employment)—the instrument for weeks in jail or prison produced multicollinearity problems in the property-arrest equation. When included, time in jail or prison yielded a statistically significant, *positive* regression coefficient of nearly .06. This implied that every week in jail or prison *increased* substantially the number of arrests and individuals who were arrested early enough in the 12-month follow-up to spend 40 weeks in jail, for example, experienced 2.5 *more* arrests. Moreover, the cross-equation correlations between the residuals increased enormously, another indicator of implausible and unstable results.[21]

To summarize, two important indicators of financial need appear to reduce the number of property arrests experienced by TARP participants: TARP payments and employment. This pair of results has implications for our proposed counterbalancing model to which we will soon return.[22]

Results for the Number of Nonproperty Arrests

Table 6 shows the findings for the number of nonproperty arrests, and perhaps a few words are needed about specification issues. First, we have approached nonproperty arrests as crimes for which there are very unlikely to be any monetary returns. Consequently, measures of financial need are not included.

Second, given the average nonseriousness of nonproperty arrests, we have assumed that they are more likely to reflect the discretionary practice of local law enforcement agencies. We had no direct measures of these practices but have included dummy variables for major jurisdictions in an effort to capture these effects.[23]

Third, consistent with the view that nonproperty arrests may have little to do with the severity of offenses actually

---

[21] Had we continued to include weeks in jail or prison in the equation, the impact of weeks employed would still have been statistically significant but the effects of TARP payments would *not* have been (in both the two-stage and three-stage least-squares results).

We also experimented with an entirely different specification to take the impact of incarceration into account. In brief, we divided all of the time-dependent variables (for example, weeks employed, property arrests) by the number of weeks "at large" (that is, 52 minus the number of weeks in prison or jail). Hence, all time-dependent variables were standardized for incarceration, and there then was no need for a separate equation predicting weeks in jail or prison. The results for the number of property arrests in Texas were virtually the same as for the five-question model we have been considering. However, the standardization process created some of the same problems for the Georgia property equation that we had been trying to circumvent. The correlation between the residuals for the Georgia property and nonproperty equations soared to .85, making the three-stage results especially suspect. Nevertheless, while t-value for the impact of TARP payments dropped to −.41, the negative impact of employment remained (for both two-stage and three-stage results). Note also that the standardization through the use of ratio variables by itself creates no artifacts, although the impact of measurement error is apparently exacerbated (Long, 1980).

[22] Late in the analysis we tried various kinds of interaction effects. We explored the possibility that the payments, for example, worked "better" for "good risks" (for example, older ex-felons, ex-felons with better work histories, etc.). However, nothing of interest surfaced and very high multicollinearity often resulted. Recall that instruments must be constructed for product variables in which one (or both) of the components is endogenous.

[23] The excluded dummy variable in both states is all rural counties.

Table 6. Analysis of the Number of Nonproperty Arrests

	Texas		Georgia	
Variable	Coefficient	t-value	Coefficient	t-value
Intercept	1.41*	3.29	1.19*	6.43
Weeks employed	−0.03*	−3.68	−0.03*	−5.35
Weeks incarcerated	−0.06	−2.64	a	
TARP payments (hundred $)	−0.02*	−2.21	−0.01*	−2.14
Male (dummy)	0.45*	3.01	0.25*	1.90
Age (years)	−0.00+	−0.30	−0.00+	−1.87
No. of residences over the year	0.02	1.44	0.05	2.67
Weeks sick or in hosp.	−0.03*	−2.59	−0.02*	−3.12
Age at first arrest	0.01	−2.18	−0.01	−1.32
Times arrested	−0.00+	−0.09	−0.00+	−0.90
Black (dummy)	−0.07	−1.17	−0.02	−0.54
Chicano (dummy)	0.11	1.38	b	
Prison adjustment score	−0.00+	−0.31	b	
Bexar (San Antonio) County (dummy)	0.39	3.12	b	
Dallas County (dummy)	−0.05	−0.81	b	
Harris (Houston) County (dummy)	−0.15	−2.33	b	
Tarrant (Fort Worth) County (dummy)	−0.21	−2.23	b	
Savannah County (dummy)	b		−0.16	−1.83
Atlanta County (dummy)	b		0.05	1.13
Bibb County (dummy)	b		0.10	1.11
Banks (dummy)	b		−0.15	−1.64

* Both coefficients statistically significant at $p < .05$ for one-tail test.
a: Dropped due to high multicollinearity.
b: Not available or does not apply.

committed, we have included several variables tapping characteristics of perpetrators that might predict either a "taste" for nonproperty offenses and/or attributes that might affect police practices: race, prior record, unstable living arrangements, and the like. Finally, we again had to drop weeks in jail or prison from the Georgia equation because of instabilities produced by multicollinearity.[24]

Table 6 indicates that, by and large, the findings for nonproperty arrests are much like those for property arrests. Again employing our conservative strategy of interpreting only regression coefficients that are statistically significant in both states, it seems that each $100 of TARP payments reduces the number of nonproperty arrests in Texas by .02 and in Georgia by .01. While the effects are a bit smaller than for property arrests, the overall impact is nontrivial. For example, $1,500 worth of payments in Texas should reduce the number of nonproperty arrests by .30.

The effects of employment also surface once again. In Texas, each week of employment reduces the number of nonproperty arrests by .03 and in Georgia each week of employment reduces the number of nonproperty arrests by .03. Thus, an individual who found a job shortly after release and worked steadily during the year of follow-up can be expected to experience about 1.5 fewer nonproperty arrests.

Of the remaining variables, men experience about .30 more nonproperty arrests while each week of illness or in the hospital reduces the number of nonproperty arrests by about .02. There is also some evidence that where an ex-prisoner lives makes a difference. Residents of Bexar County experience .39 more nonproperty arrests while residents of Harris County, Tarrant County and Savannah experience about .15 fewer nonproperty arrests. On the other hand, there is no indication that minorities are more subject to arrest, other things being equal.

In short, while there are some interesting differences found for the two kinds of arrests, the TARP payments and employment have almost identical effects for both. However, since nonproperty arrests are very unlikely to involve crimes for which monetary returns are involved, the impact of having more disposable income, per se, is probably not relevant. Rather, the TARP payments may operate through the greater opportunity costs of apprehension. The employment effects may operate by (a) withdrawing time from competing activities, (b) raising the opportunity cost of apprehension, and (c) shaping the preferences of TARP participants toward more law-abiding behavior.

---

[24] As in the case of property arrests, we tried to reduce the problems caused by our incarceration variable through standardization; we standardized each time-dependent variable for the number of weeks "at large." And, again, there was evidence of serious statistical problems. Nevertheless, employment continued to have its effect, and, while TARP payments were no longer statistically significant, t-values hovered around −1.50.

CONCLUSIONS

The analysis of the TARP experiment suggests both methodological and substantive conclusions. From the methodological perspective, the importance of *creating* variance in critical exogenous variables is highlighted. A host of technical difficulties inherent in data resting on naturally occurring variation may be surmounted. In addition, the potential merits of structural equation modeling to supplement the usual analyses of randomized experiments (that is, analysis of variance) is apparent. Had we not attempted to model the system of relationships affected by the TARP payments, a very different and incomplete story would have emerged. Finally, given all of the complexities and uncertainties of structural equation modeling, replications are clearly desirable. Without the Georgia replication, many of our most important findings could be rather easily dismissed.

From a substantive perspective, our counterbalancing model has several important implications for the relationship between poverty and crime among ex-offenders. First, it appears that experimentally induced unemployment does increase arrests for both property and nonproperty crimes. Moreover, the effect is direct and nontrivial; ex-prisoners who are unemployed have more difficulties with the criminal justice system. Second, modest transfer payments appear to reduce arrests for property and nonproperty crimes. Poverty is apparently causally related to crime at the individual level. Third, modest income transfer payments, especially when made conditional upon unemployment, produce substantial work disincentives.[25] In this context, it is important to bear in mind that the rules of the experiment as administered meant that often a 100% tax was placed on earnings (that is, TARP payments were reduced, dollar-for-dollar, for earnings received). Furthermore, work disincentive effects could have been expected to be large because the employment opportunities available to TARP participants were poor competition for the payments. Wages of between $100 and $150 per week before taxes earned at hard and/or unpleasant tasks can hardly appear to be extraordinarily appealing, compared to $60 or $73 per week obtained without working.

More broadly viewed, perhaps the most important substantive conclusion from the TARP experiment concerns the ability of a blend of theory from economics and sociology to explain at least some of the sources of variation in criminal behavior. For ex-offenders at least, unemployment and poverty do cause crime on the microlevel. Modest amounts of financial aid can reduce recidivism among ex-felons. Experimentally induced unemployment can increase recidivism. The mechanisms producing these two effects can be anticipated on the basis of a priori theoretical ideas from microeconomics and sociology.

Finally, there are also policy implications that can be drawn from the findings. A policy that would extend unemployment insurance benefits to released prisoners on a modest level will not increase recidivism overall. Hence, on purely humanitarian grounds, it is possible to argue for such a policy. When we also consider that the burdens of postprison support fall largely on families subsisting at very marginal levels, a good case can be made for implementing the policy as tested in Georgia and Texas. Modest extensions of the unemployment benefit system to released prisoners would transfer some portion of those burdens to the public sector.

---

[25] While it is difficult—and perhaps hazardous—to compare the work-disincentive effects of the negative income tax payments offered in the income maintenance experiments with those experienced in the TARP experiments, it does appear that the TARP payment disincentive effects were several magnitudes larger. Of course, tax rates were different (and more generous in the income maintenance experiments), the populations were markedly different (intact families as opposed, mainly, to single males in the TARP experiments), etc.

Although it may appear difficult to devise a payment plan that would be stripped of work disincentive effects, the experiences of the earlier LIFE experiment at least suggest that it may be possible to do so. A generous tax rate that is faithfully administered may provide positive incentives for working, as the LIFE experience seems to indicate. It is also possible to build into payments stronger incentives than a generous tax rate, for example, actually increasing payments for a time if the ex-felon becomes employed.[26] Indeed, this direction appears so promising that an additional program of experimental research appears indicated. Such a program would investigate alternative ways of reducing work disincentives of payments through testing of generous tax rates and positive incentives for employment.

REFERENCES

Amemiya, T.
1973 "Multivariate regression and simultaneous equation models when the dependent variables are truncated normal." Econometrica 42:997–1017.

Arrow, Kenneth
1972 "Models of job discrimination." Pp. 101–24, in A. Pascal (ed.), Racial Discrimination in Economic Life. Lexington: Lexington Books.
1973 "The theory of discrimination." Pp. 3–33, in O. Ashenfelter and A. Reed (eds.), Discrimination in Labor Markets. Princeton: Princeton University Press.

Barnett, Vic
1973 Comparative Statistical Inference. New York: Wiley.

Becker, Gary S.
1968 "Crime and punishment: an economic approach." Journal of Political Economy 76:169–217.
1971 The Economics of Discrimination (2nd ed.). Chicago: University of Chicago Press.
1976 An Economic Approach to Human Behavior. Chicago: University of Chicago Press.

Berk, Richard A., and Sarah F. Berk
1979 Labor and Leisure at Home. Beverly Hills: Sage.

Bonger, William A.
1916 Criminality and Economic Conditions. Boston: Little, Brown.
1967 Reprinted, New York: Agathon Press.

Boruch, Robert F., A. John McSweeny, and E. Jon Sonderstrom
1978 "Randomized field experiments for program planning, development and evaluation: an illustrative bibliography." Evaluation Quarterly 2:655–94.

Block, M. K., and J. M. Heineke
1975 "A labor-theoretic analysis of criminal choice." American Economics Review 65:314–25.

Blumstein, A. J., J. Cohen, and D. Nagin
1978 Deterrence and Incapacitation: Estimating the Effects of Criminal Sanctions on Crime Rates. Washington, D.C.: National Academy of Sciences.

Brenner, Harvey
1976 Estimating the Social Costs of National Economic Policy (for the Joint Economic Committee). Washington, D.C.: U.S. Government Printing Office.

Brier, S. S., and S. E. Fienberg
1980 "Recent econometric modeling of crime and punishment: support for the deterrence hypothesis?" Evaluation Review 2:147–91.

Ehrlich, Isaac
1973 "Participation in illegitimate activities: a theoretical and empirical investigation." Journal of Political Economy 81:521–65.

Edison, Bettye K., and Maurice E. B. Owens, III
1976 Surveying Crime. Washington, D.C.: National Academy of Sciences.

Erikson, Maynard L., and Lamar T. Empey
1963 "Court records, undetected delinquency and decision making." The Journal of Criminal Law, Criminology and Police Science 54:456–69.

Glaser, Daniel
1979 "A review of crime-causation theory and its application." Pp. 203–38, in N. Morris and M. Tonry (eds.), Crime and Justice: An Annual Review of Research 1. Chicago: University of Chicago Press.

Heckman, James
1979 "Sample selection bias as a specification error." Econometrica 47:151–61.

Long, S. B.
1980 "The continuing debate over the use of

---

[26] Richard A. Berk and David Rauma are currently evaluating a program in California providing unemployment benefits to ex-offenders based on eligibility earned from *prison* jobs. While the analysis is still in very preliminary stages, there is evidence that some kinds of transfer payments do not induce unemployment. In particular, parole officers often give grants and loans to parolees (often involving several hundred dollars) *conditional* upon the parolee making a serious effort to obtain work. For example, parole officers will often provide money to have a parolee's car repaired if the parole officer is convinced that the parolee needs a car to get to work and that a serious job search is underway. These transfer payments do not appear to induce unemployment.

ratio variables: facts and fiction." Pp. 37–67, in K. F. Schuessler (ed.), Sociological Methodology. San Francisco: Jossey-Bass.

Mallar, Charles, and Craig V. D. Thornton
1978 "Traditional aid for released prisoners: evidence from the LIFE experiment." Human Resources XIII:208–36.

Merton, Robert K.
1957 Social Theory and Social Structure. New York: Free Press.

Miller, Walter B.
1958 "Lower class culture as a generating milieu of delinquency." Journal of Social Issues 14:5–19.

Nicholson, Walter
1978 Microeconomic Theory (2nd ed.). Hinsdale, Il.: Dryden Press.

Nock, Steven S., and P. H. Rossi
1978 "Ascription versus achievement in the attribution of social status." American Journal of Sociology 84:541–64.

Penick, B. K., and R. Owens
1976 Surveying Crime. Washington, D.C.: National Academy of Sciences.

Ray, S., Richard A. Berk, and William Bielby
1980 "Correcting for sample selection bias with a bivariate logistic distribution of disturbances." Paper read at Annual Meeting of the American Statistical Association, August.

Richards, Pamela, Richard A. Berk, and Brenda Forster
1979 Crime as Play: Delinquency in a Middle Class Suburb. Cambridge, Mass.: Ballinger.

Rosen, Sherwin
1977 "Human capital: a survey of empirical research." Pp. 3–40, in R. Ehrenberg (ed.), Research in Labor Economics: An Annual Compilation of Research, Vol. I. Greenwich, Conn.: JAI Press.

Heineke, J. M.
1978 "Economic models of criminal behavior: an overview." Pp. 1–34, in J. M. Heineke (ed.), Economic Models of Criminal Behavior. New York: North-Holland.

Hyman, Herbert
1980 "The effects of unemployment: a neglected problem in social research." Pp. 240–64, in R. K. Merton, J. S. Coleman, and P. H. Rossi (eds.), Qualitative and Quantitative Social Research. New York: Free Press.

Irwin, John
1970 The Felon. Englewood Cliffs: Prentice Hall.

Jöreskog, K. G., and D. Sörbom
1979 Advances in Factor Analysis and Structural Equation Models. Cambridge, Mass.: Abt Books.

Jurik, Nancy
1979 "Women ex-offenders: their work and rearrest patterns." Unpublished Ph.D. dissertation, Department of Sociology, University of California, Santa Barbara.

Lenihan, Kenneth J.
1977 Unlocking the Second Gate. Washington, D.C.: U.S. Government Printing Office.

Letkeman, P.
1973 Crime as Work. Englewood Cliffs: Prentice Hall.

Liker, Jeffrey K.
1980 "Well-being and employment." Unpublished Ph.D. dissertation, Department of Sociology, University of Massachusetts.

Lippman, S. A., and J. J. McCall
1976a "The economics of job search: a survey (Part I). Economic Inquiry XIV:155–89.
1976b "The economics of job search: a survey (Part II). Economic Inquiry XIV:347–68.
1979 Studies in the Economics of Search. New York: North Holland.

Rossi, Peter H., Richard A. Berk, and Kenneth J. Lenihan
1980 Money, Work and Crime: Some Experimental Results. New York: Academic Press.

Sickles, R. C., and P. Schmidt
1978 "Simultaneous equation models with truncated dependent variables." Journal of Economics and Business 31:11–21.

Sjoquist, D. L.
1973 "Property crime and behavior: some empirical results." American Economics Review 63:439–46.

Sparks, Richard F., Hazel G. Glenn, and David J. Dodd
1977 Surveying Victims: A Study of the Measurement of Criminal Victimization. New York: Wiley.

Tobin, J.
1958 "Estimation of relationships for limited dependent variables." Econometrica 26:1–12.

SOCIAL NETWORKS AND SOCIAL MOVEMENTS: A MICROSTRUCTURAL APPROACH TO DIFFERENTIAL RECRUITMENT*

DAVID A. SNOW
University of Texas at Austin

LOUIS A. ZURCHER, JR.
University of Texas at Austin

SHELDON EKLAND-OLSON
University of Texas at Austin

American Sociological Review 1980, Vol. 45 (October):787–801

Past examinations of differential recruitment to and the differential growth of social movements have typically sought explanation at a social psychological/motivational level of analysis. That focus has recently been called into question by scholars concerned with the process through which movement organizations expand their ranks and mobilize support for their causes. Yet, as Useem (1975) and Zald and McCarthy (1979) have noted, there has been little systematic research conducted on the details of the influence process. Drawing on data derived from a synthesis of existing research and two primary sources, this paper attempts to shed greater empirical and theoretical light on the movement recruitment process. The findings indicate that differential recruitment is not merely a function of dispositional susceptibility, but is strongly influenced by structural proximity, availability, and affective interaction with movement members. The findings also indicate that a movement organization's network attributes function as an important determinant of its recruitment strategies and growth.

Among the various issues and questions relating to the study of social movements, few have generated as much discussion and research as those pertaining to differential recruitment. Why are some people rather than others recruited into a particular social movement organization?[1] Given the number of competing and functionally equivalent movement organizations frequently on the market at the same time, how is it that people come to participate in one rather than another? Why do some movement organizations attract a larger following and grow at a more rapid rate than others? Whether one scans the literature on religious cults and movements, political protest, the ghetto revolts of the 1960s, or student activism, the problem of differential recruitment surfaces as a dominant focal concern, both theoretically and empirically. Indeed, the study of movement recruitment has been "one of the most prominent characteristics of collective behavior research in recent years" (Marx and Wood, 1975:388).

Nonetheless, understanding of the process of differential recruitment, its underlying determinants, and implications for the spread and growth of social movements is still quite limited (Marx and Wood, 1975:393). As Zald and McCarthy (1979:240) concluded in their recent review of the resource mobilization approach, very little is known about the movement recruitment process and the conditions under which various recruitment techniques are more or less successful.

The purpose of this article is to advance understanding of the movement recruit-

---

* Address all communications to: David A. Snow; Department of Sociology; University of Texas; Austin, TX 78712.

We wish to thank the anonymous reviewers and the editors of *ASR* for their insightful and instructive suggestions. Preparation of the article was partially funded by a grant from the University Research Institute, University of Texas.

[1] Here we are following McCarthy and Zald's (1977:1217–9) distinction between a social movement and a movement organization. They define a social movement as "a set of opinions and beliefs in a population which represents preferences" in support of or opposition to social change. A social movement organization is defined as "a complex, or formal, organization which identifies its goals with the preferences of a social movement or a countermovement and attempts to implement those goals." We think this is a conceptually useful distinction since individuals are recruited into and devote time and energy to movement organizations.

ment process by assembling quantitative and qualitative data which bear on the relationship between movement recruitment and such microstructural factors as social networks, and by developing a set of corresponding propositions. Four focal questions structure the inquiry: (1) What are the microstructural avenues of movement recruitment? (2) What accounts for the differential availability of people for movement participation? (3) What are the structural characteristics of social movements which predict different recruitment strategies and patterns? (4) What implications do different recruitment strategies and patterns have for the spread and growth of a movement?

DATA AND PROCEDURES

The article draws on three sets of data. The first is derived from an examination of social movement case studies through which we synthesize quantitative data pertaining to the recruitment process. References to the salience of such microstructural factors as social networks are not uncommon, but our review of the literature revealed that most of the references tend to be impressionistic. There is a dearth of hard data specifying the function of social networks in relation to differential recruitment to and the differential spread and growth of movements. However, we did find ten studies with quantitative data bearing directly on the recruitment process. The sample size of the ten studies varies from 31 to 310 participants, with the combined N totalling more than 1,200.

The second data set comes from an examination of recruitment to the Nichiren Shoshu Buddhist movement in America.[2] These data were derived from the senior author's observations and experiences as a participant-observer for a year and a half, his informal interviews with recruits and members, and his examination of a random sample of members' testimonies appearing in the movement's newspaper, *The World Tribune.*[3] Since most editions of the newspaper contain several members' testimonies, 504 were randomly selected from 1966 to 1974 (six per month), excluding a two-year period in which the newspaper was not readily available. Three hundred and thirty of the 504 testimonies provided information specifying mode of recruitment into the movement. These 330 cases, coupled with 15 other members from whom recruitment information was informally elicited, yielded a sample of 345. Informal interviews with 25 Hare Krishna devotees supplement the data pertaining to Nichiren Shoshu. Twenty of the interviews were with members of the Krishna commune in Los Angeles, and five were with members of the Dallas commune. The interviews were conducted during the course of six visits to the Los Angeles commune and two visits to the Dallas commune.[4]

The third data set is derived from a nonrandom sample of University of Texas students. A questionnaire designed to gather information pertaining to movement recruitment and participation was administered to 550 students enrolled in ten different university courses in the Spring semester of 1979. The first part of the questionnaire contained a list of twenty-five social movement organizations within the Austin, Texas, area. The respondents were initially asked to indicate for each movement whether they were (a) unfamiliar with it; (b) familiar but not sympathetic or supportive; (c) sympathetic but not a participant; or (d) associated with it as a participant at one time or another. Three hundred of the 550 respondents indicated that they either were movement participants or were sympathetic with the objectives of one or more of the movements.[5] Since we are concerned with differential recruitment, only the responses of the 135 participants and the 165 sympathizers are considered. The sympathetic nonparticipants constitute a kind of control group in that their responses shed light on the question of why it is that people who are in sympathy with and supportive of a movement's value orientation do not always participate in movement activities. A consideration of the responses of both participants and sympathizers should yield a better understanding of differential recruitment.

---

[2] Nichiren Shoshu of America (NSA) is a Japanese-based, culturally transplanted, proselytizing Buddhist movement that seeks to change the world by changing individuals. It was formally introduced into America in 1960 and claims to have since attracted more than 200,000 members, the vast majority of whom are Occidental. For a detailed examination of the movement's ideology, goals, and operation in America, see Snow (1976; 1979).

[3] *The World Tribune* is published five times weekly in Santa Monica, California. It was first issued in August 1964 and now has over 55,000 monthly subscriptions. It constitutes a useful record of the growth and history of the Nichiren Shoshu movement in America.

[4] For a discussion of the origins, goals and ideology, and operation of Hare Krishna, see Daner (1976) and Judah (1974).

FINDINGS AND DISCUSSION

The Microstructural Avenues of Recruitment

Movement recruitment has generally been approached from a social-psychological/motivational level of analysis, with various states-of-mind or psychological attributes posited as the major causal variables (see Cantril, 1941; Feuer, 1969; Glock, 1964; Hoffer, 1951; Klapp, 1969; Toch, 1965). However reasonable the underlying assumption that some people are more susceptible than others to movement participation, that view deflects attention from the fact that recruitment cannot occur without prior contact with a recruitment agent. The potential participant has to be informed about and introduced into a particular movement. Thus, even if one accepts the popular contention that some individuals are predisposed social-psychologically to movement participation, the following question still remains: What determines which potential participants are most likely to come into contact with and be recruited into one movement rather than another, if into any movement at all?

It is a basic sociological tenet that social phenomena are not distributed randomly, but are structured according to aggregate or group membership, role incumbency, and the like. It thus seems reasonable to assume that movement recruitment, rather than being random or merely the function of social-psychological predispositions, will also be structured by certain sociospatial factors. Accordingly, we can begin to answer the above question by considering, first, the various sociospatial settings in which movements and potential participants can come into contact, and, second, the variety of generally available modes of communication through which information can be imparted.

Regarding the first concern, most spatial settings or domains of social life can be conceptualized in terms of a continuum ranging from public to private places.[6] Examples of the former include shopping malls, community sidewalks, airports, bus stations, and city parks. Country clubs, an office, a sorority or fraternity house, and an apartment or home are illustrative of the latter.

The means of information dissemination

---

[5] Classification of Respondents by Relationship to and Type of Movement

Movement Type	Participants[a]	Sympathizers[b]	Totals
Religious or Psychiatric[c]	54	50	104
Political[d]	81	115	196
Totals	135	165	300

[a] "Participants" refers to those individuals who devote time, energy, and other resources to the movement organization with which they are affiliated. They are what McCarthy and Zald (1977:1221) refer to as the "constituents" of a social movement organization.

[b] "Synpathizers" refers to those individuals who believe in or agree with the goals of a movement or movement organization, but do not devote any personal resources to it. They are what McCarthy and Zald (1977:1221) refer to as "adherents."

[c] Movements classified as "Religious or Psychiatric" include Transcendental Meditation, Church of Scientology, Baha'i, Jehovah's Witnesses, Campus Crusade for Christ, etc.

[d] Movements classified as "Political" include: National Organization for Reform of Marijuana Laws, NOW, Right to Life, Young Socialist Alliance, Lesbian-Gay Alliance, John Birch Society, Common Cause, University Mobilization for Survival (Anti-Nuke), etc.

[6] Following Goffman (1963) and Lyman and Scott (1967), "public places" refer to those regions or areas in a community or society that are freely and officially accessible to most members of that community or society. "Private places," on the other hand, refer to those spatial domains that are off-limits to all but acknowledged members and guests, and in which outsiders are considered as actual or potential intruders.

	PUBLIC CHANNELS	PRIVATE CHANNELS
FACE-TO-FACE	Face-to-face leafleting, petitioning, and proselytizing on sidewalks; participation in public events, such as parades; staging events for public consumption, such as sit-ins, protests, movement-sponsored conventions and festivals.	Door-to-door leafleting, petitioning, and proselytizing; information dissemination and recruitment among familiar others along the lines of promoter's extra-movement interpersonal networks.
MEDIATED	Promotion and recruitment via radio, television, and newspapers.	Promotion and recruitment via mail and telephone.

Figure 1. Classification of General Outreach and Engagement Possibilities for Movement Information Dissemination, Promotion, and Recruitment

can be conceptualized generally in terms of whether they are face-to-face or mediated. By face-to-face communication, we refer to all information, whether it be verbal or nonverbal, that is imparted when two or more individuals or groups are physically present. In contrast, mediated communication refers to information dissemination through institutionalized mass communication mechanisms, such as radio and television, or through institutionalized, but individualized and privatized, communication mechanisms such as the mail and telephone.

The cross-classification of these two dimensions suggests four general and fairly distinct microstructural avenues for movement information dissemination and recruitment. Figure 1 schematically summarizes these alternative avenues, each of which is distinguished by the spatial domain of social life in which contact can be established and the means through which information can be imparted.

Given the alternative microstructural avenues, the question arises as to the relative yield of each in terms of actual recruits. In other words, is recruitment among strangers in public places more productive than recruitment along the other avenues, or are movement recruits typically drawn from existing members' extramovement friends, acquaintances, and kin?

Examination of the movement literature strongly suggests that the network channel is the richest source of movement recruits. Numerous studies allude to the importance of social networks as a conduit for the spread of social movements. While some of these works are theoretical or critical in orientation (Oberschall, 1973; Tilly, 1978; Useem, 1975; Wilson, 1973; Wilson and Orum, 1976), most are empirical studies of religious movements (Bibby and Brinkerhoff, 1974; Dator, 1969; Gerlach and Hine, 1970; Harrison, 1974; Heirich, 1977; Judah, 1974; Lee, 1967; Lofland, 1966; Murata, 1969; White, 1970) or political protest or reform-oriented movements (Freeman, 1973; Jackson et al., 1960; Leahy, 1975; Marx, 1969; Orum, 1972; Petras and Zeitlin, 1967; Pinard, 1971; Tygart and Holt, 1972; Von Eschen et al., 1971; Woelfel et al., 1974; Zurcher and Kirkpatrick, 1976). Although suggestive, few of the investigations provide hard data specifying the relation between social networks and differential recruitment. However, ten studies were found with quantitative data bearing on the recruitment process. Table 1 summarizes the findings of all but one of the studies,[7]

---

[7] Heirich's (1977) findings regarding conversion to Catholic Pentecostalism do not neatly fit into Table 1. However, the findings are consistent with those reported in Table 1. Among the 118 respondents "introduced to the movement by trusted associates," 75% were converts. In contrast, among the 187 respondents who were not introduced to the movement by trusted associates, only 31% were converts. Based on similar findings for other social influence variables, Heirich (1977:667) concluded that "it is clear that members of the movement, when recruiting, turn to previous friends and to persons they meet daily."

Table 1. Studies with Data Specifying Mode of Recruitment to Various Movements (Percentage)

			Mode of Recruitment			
			Recruited through Social Networks			% Recruited Outside Networks[b]
Investigator	Movement	N	% By Relatives	% By Friends, Acquaintances[a]	Tot. % Rec. thr. Ntwks.	
Sills (1957)	March of Dimes	234	[c]	90	90	10
Murata (1969)	Sokagakkai	100	16	76	92	9
Dator (1969)	Sokagakkai	120	65	35	100	[d]
White (1970)	Sokagakkai	[e]	24	86	96	4
Gerlach and Hine (1970)	Pentecostal	77[f]	47	32	79	21
Harrison (1974)	Catholic Pentecostal	169	[g]	59	59	[g]
Bibby & Brinkerhoff (1974)	Evangelistic Protestantism	132	45	29	74	26
Leahy (1975)	Anti-Abortion	31	26	65	91	9
Judah (1974)	Hare Krishna	63	0	3	3	97

[a] Includes all individuals recruited through networks other than familial or kinship (e.g., occupational, neighborhood).

[b] Includes all individuals recruited by strangers in the street or at mass meetings, or who sought out the movement after learning about it through the public media, or who joined on their own initiative.

[c] Percent recruited by relatives, if any, not reported. Of the 90% recruited through networks, 52% were recruited by friends, 20% through community network, and 18% through organizational and occupational networks.

[d] Data derived from 200 testimonials of American members in a Sokagakkai newspaper. Of the 60% (120) indicating mode of recruitment, none was recruited by strangers or through the media. The reason for the large percentage recruited by relatives is that the majority of the individuals in the sample were American GI's stationed in Japan who were married to Japanese members.

[e] Sample size not given. Percent figures based on the average of three to five different surveys. Percent figure for friends and acquaintances based on the average of three surveys; percent figure for relatives and the last two columns based on the average of five surveys.

[f] Figures based on the compilation of Gerlach and Hine's (1970:79–80) reported findings regarding two separate Pentecostal churches.

[g] Percent recruited by relatives or strangers, if any, not reported. Therefore the actual percentage recruited through networks may have been greater than the 59% reported.

clearly demonstrating the salience of social networks as a recruitment avenue. For each of the movements studied, with the exception of Hare Krishna, extramovement social networks constitute the primary source of recruits.

Our findings regarding recruitment to Nichiren Shoshu are consistent with those just presented. As indicated in Table 2, Nichiren Shoshu members are typically recruited into the movement by one or more members with whom they have a preexisting, extramovement, interpersonal tie. Although Nichiren Shoshu members devote a considerable amount of time and energy to proselytizing in public places, these information dissemination and recruiting forays are not very productive. The senior author accompanied members on over forty such expeditions, and only twice were recruits attracted. In contrast, all but one of the twenty-five Krishna devotees we interviewed were recruited "off the street" by other members who were strangers at the time of contact. (Factors which account for the different recruitment patterns of Hare Krishna and the other movements will be discussed later.)

Table 2. Recruitment Patterns for Nichiren Shoshu and Hare Krishna (Percentage)

Recruitment Avenues	Nichiren Shoshu (N:345) Percent	Hare Krishna (N:25) Percent
Public Places	17	96
Social Networks	82	4
Mass Media	1	0
Mail/Telephone	0	0

The important bridging function of social networks in relation to movement recruitment is further demonstrated by our findings regarding the sample of university students participating in various social movements. Additionally, these findings suggest that network linkages are not only important in accounting for differential recruitment into religious movements, but also for political movements. As indicated in Table 3, 63% of the students participating in political movements were drawn into their respective movement's orbit of influence by being linked to one or more members through a preexisting, extramovement interpersonal tie.

The findings associated with the three data sets not only corroborate each other, but they also clearly demonstrate the importance of preexisting social networks in structuring movement recruitment. It is thus reasonable to suggest the following summary proposition:

> *Proposition 1:* Those outsiders who are linked to one or more movement members through preexisting extramovement networks will have a greater probability of being contacted and recruited into that particular movement than will those individuals who are outside of members' extramovement networks.

The Importance of Social Networks in Accounting for Differential Availability

Proposition 1 and the data in which it is grounded suggest that recruitment among social networks is likely to be more productive than recruitment via the other microstructural avenues. However, the fact remains that not all relatives, friends or acquaintances of movement members participate in movement activities when invited. The findings reported in the preceding section indicate that, for all the movements studied, at least some members were recruited "off the street" or through the public media. The findings pertaining to Hare Krishna in particular suggest that some movements recruit most of their members through channels other than social networks. The question thus arises as to who out of the pool of individuals contacted either through existing ties or through other recruitment avenues is most likely to become a movement participant?

Table 3. Recruitment Avenues of a Sample of University Students Participating in Various Movements (Percentage)

Recruitment Avenues	Movements: Political (N:81) Percent	Movements: Religious (N:54) Percent	Totals (N:135) Percent
Public Places	7	0	4
Social Networks	63	80	70
Mass Media	30	20	26
Mail/Telephone	0	0	0

One plausible answer is suggested by the many works that posit a psychofunctional linkage between social-psychological attributes (conceptualized as susceptibilities) and the goals and ideologies of movements (construed as appeals). According to this view, participation in movement activities is largely a function of certain fertile dispositions, such as alienation (Bolton, 1972; Oppenheimer, 1968; Ransford, 1968), relative deprivation (Aberle, 1966; Dayies, 1971; Glock, 1964; Gurr, 1970); and authoritarianism (Hoffer, 1951; Lipset, 1960; Lipset and Raab, 1973). Although of longstanding popularity, the social-psychological dispositional approach has in recent years been called into question on both theoretical and empirical grounds.[8] Moreover, our research suggests that the reason why some rather than other individuals join a movement once they have been introduced to it can be explained in large part by their structural availability. Specifically, the findings pertaining to Nichiren Shoshu and the sample of movement sympathizers suggest that the reason for participating or not participating in movement activities once invited is largely contingent on the extent to which extramovement networks function as countervailing influences. Since sets of social relationships can be more or less demanding in regard to time, energy, and emotional attachment (Etzioni, 1975; Kanter, 1972), it follows that they can also vary in the extent to which they constitute countervailing influences or extraneous commitments (Becker, 1960) with respect to alternative networks and lines of action. Hence, some individuals will be more available for movement exploration and participation because of the possession of unscheduled or discretionary time and because of minimal countervailing risks or sanctions.

---

[8] Much of the literature touching on the hypothetical association between preexisting social psychological strains and participation in movements suggests a relatively indeterminate relationship (see Aberle, 1965; Bolton, 1972; Marx and Wood, 1975; Petras and Zeitlin, 1967; Portes, 1971; Useem, 1975). Also the magnitude of statistical association between measures of social psychological variables and participation in movements and protest activities has generally been quite small and unconvincing (see Lewis and Kraut, 1972; McPhail, 1971; Moinat et al., 1972; Portes, 1971; Snyder and Tilly, 1972). For varied criticisms of the social psychological-dispositional approach to movement joining, see McCarthy and Zald (1977:1214–5), Snow and Phillips (1980), Turner and Killian (1972:365), Useem (1975:11–18), and Zygmunt (1972).

In the case of Nichiren Shoshu, these observations seem to hold for both those members recruited from the street and those recruited through social networks. Most were under 30, single, in a transitional role (such as that of student), employed in a line rather than in a managerial position, or in a state of occupational marginality. As a consequence, they tended to possess a greater amount of unscheduled time and generally lacked the kinds of extraneous commitments that are likely to inhibit movement participation. Aside from the absence of a social tie to one or more members prior to initial contact, those recruited from the street differed from those recruited through networks only in that they were structurally more available for participation. This observation is illustrated by the following account of how and why one street-recruit, a twenty-five-year-old male, came to join Nichiren Shoshu:

> I found myself here in L.A. with nothing but the clothes I was wearing. I didn't know anybody. I didn't have a lot of money. It was a really strange situation.
>
> I had just flown into L.A. airport, and all my baggage came up missing. This was on a Saturday night. Since I didn't know anybody and didn't have any place to go, I went to the airport police station and was told to go to Travelers' Aid. But I learned that they wouldn't be open until Tuesday, since this was a Labor Day weekend. So I waited around the airport until Tuesday and then went down to Travelers' Aid. They sent me to the Welfare Department. After spending four days waiting and filling out forms, I was told that I couldn't qualify for welfare because I wasn't a California resident.
>
> At this point I didn't know what to do. So I spent a few nights at the Midnight Mission, and then decided to go to the Santa Monica Beach. That evening while I was walking around downtown Santa Monica, this guy came up to me and started talking about Nam-Myoho-Renge-Kyo (the Nichiren Shoshu chant), and asked if I would like to go to a meeting. Since I didn't have anything else to do, I went along.
>
> All of a sudden I find myself at this meeting where everybody was chanting. I didn't have the faintest idea about what was going on. I had never heard of the chant before, and didn't even know there was such a group as NSA. But since everybody was telling me to give chanting a try, I figured why not. I literally didn't have anything to lose. So I joined, and I've been chanting ever since—which is about four months.

Although this account differs somewhat from the stories of other street-recruits in its particulars, it underscores what was common to the life-situation of the 85 street-recruits about whom we were able to gather information.[9] They were either recent arrivals to or passing through the area in which they were recruited, or they were minimally involved in proximal and demanding social relationships.[10] Although one might argue that these street-recruits were "susceptible" to the "appeals" of the movements they joined because they were "social isolates," "loners," or "outcasts," we do not subscribe to such an interpretation. Rather, we think it is sociologically more compelling to argue that individuals who join social movements share the kinds of demographic and social characteristics that allow them to follow their interests and/or engage in exploratory behavior to a greater extent than individuals who are bound to existing lines of action by such extraneous commitments as spouse, children, debts, job, and occupational reputation.[11]

---

[9] The information pertaining to street-recruits is based on the testimonies of 61 Nichiren Shoshu members recruited off the street and on in-depth interviews with two street recruits to NSA and with 24 Hare Krishna devotees.

[10] It thus follows that relatively few people are recruited directly off the streets for two basic reasons: First, most people walking the street are engaged in the business of completing a particular line of action—such as shopping, going to a movie, and the like—and, therefore, have little if any unscheduled or free time when confronted by movement recruiters. Second, most individuals walking the street in most urban areas have a number of fairly strong, proximal social relationships that function as countervailing influences, at least when confronted by a proselytizing stranger.

This microstructural interpretation is also suggested by our findings regarding the movement sympathizers in our sample of university students. By movement sympathizers, we refer to those individuals who indicate verbal support of or agreement with the goals of a movement, but who do not devote any time, energy, or other resources to advancing the movement's objectives. The major reasons given by the sympathizers for remaining on the sidelines and not getting involved are indicated in Table 4. Most relevant to the present discussion is the finding that nearly two-thirds indicated that they did not have enough time to participate. Had their lines of action not been constrained by competing, extra-movement commitments and demands, and had they been asked to participate by a member with whom they were acquainted, then presumably they would have become a participant or "constituent" rather than just a sympathizer or "adherent."

Table 4. Reasons for Not Participating Given by Movement Sympathizers (Percentage)

	Movements		
Reasons	Political (N:115) Percent	Religious (N:50) Percent	Totals (N:165) Percent
1. Didn't know anyone actively involved	73	66	71
2. Not enough time	64	61	63
3. Wasn't asked	57	47	54
4. Just didn't want to get involved	18	16	18
5. Fear of negative reaction by nonmovement significant others	7	12	9
6. Don't know/other	4	4	4

In light of these corroborating findings and observations, it seems reasonable to suggest that, given the existence of a social tie to one or more movement members, differential recruitment is largely a function of differential availability, which is best conceptualized as a microstructural phenomenon. That is, it is a function of how tightly individuals are tied to alternative networks and thus have commitments that hinder the recruitment efforts of social movement organizations. The following propositions summarize the argument. The first pertains to the relation between social networks and availability for movement participation. The second concerns the connection between structural availability and the probability of actual participation.

> *Proposition 2A:* The fewer and the weaker the social ties to alternative networks, the greater the structural availability for movement participation.
>
> *Proposition 2B:* The greater the structural availability for participation, the greater the probability of accepting the recruitment "invitation."

Here it might be argued that even though a social bond with one or more movement members and structural availability increase the probability of movement participation, they are not sufficient conditions. We would agree, especially since social action on behalf of noncoercive organizations is unlikely in the absence of instrumental, affective, or ideological alignment (Etzioni, 1975; Kanter, 1972; Parsons and Shils, 1962). However, it is important to emphasize that people seldom initially join movements per se. Rather they typically are asked to participate in movement activities.[12] Furthermore, it is during the course of initial participation that they are provided with the "reasons" or "justifications" for what they have already done and for continuing participation. As C. Wright Mills emphasized some time ago (1940), vocabularies of motives are frequently supplied "after the act" to explain the "underlying causes of the act," even though they have little to say about how the act came about. We would thus argue that the "motives" for joining or continued participation are generally emergent and interactional rather than prestructured. That is, they arise out of a process of ongoing interaction with a movement organization and its recruitment agents. Although this alignment process has not received much empirical attention, its salience in relation to movement recruitment has been illustrated by Lofland's (1977a; 1977b) reexamination of the process by which one becomes a "Moonie" and by Snow's (1976) description of how Nichiren Shoshu strategically goes about the business of "luring" and "securing" recruits. In both cases the recruitment process is organized so as gradually to "sell" prospects on the "benefits" of participation and to provide them with "reasons" for remaining a member. This is not to suggest that prestructured cognitive states and tensions are irrelevant for understanding movement joining. Rather, it is to emphasize that they must be aligned with the movement's value orientation, given specific forms and means for expression and amelioration, and put into the service of the movement (Zygmunt, 1972).[13] In light of these observations, we argue that initial and sustained participation is largely contingent on the countervailing influence of alternative networks and intensive interaction with movement members. Whereas the first factor determines whether one is structurally available for participation, the second factor gives rise to the rationale for participation.

---

[11] This interpretation is suggested by McCarthy and Zald (1973) in their initial discussion of resource mobilization, and by McPhail and Miller's (1973) treatment of the assembling process. Although one must be cautious about assuming that the dynamics underlying participation in civil disorders and social movements are the same, it is worth noting that this interpretation is consistent with McPhail's (1971) reinterpretation of the riot participation literature, Lachman and Singer's (1968) findings regarding participation in the 1967 Detroit riot, and Snyder and Kelly's (1979) discussion of the development of urban disorders. (Also see National Advisory Commission on Civil Disorders, 1968:123.) Darley and Batson's (1973) work on helping behavior also suggests that unscheduled or discretionary time is perhaps the most important determinant of Good Samaritanism.

Structural Influences on Movement Recruitment Strategies and Patterns

In analyzing the recruitment process from the point of view of the movement itself, the relationship between movement structure (as determined by its network attributes) and recruitment opportunities and patterns requires attention. As indicated earlier, there are four general outreach and engagement channels that movements can exploit for information dissemination and recruitment: (1) They can channel their promotion and recruitment efforts among strangers in public places by face-to-face means; (2) They can promote via the institutionalized, mass communication mechanisms; (3) They can recruit among strangers in private places by such means as door-to-door canvassing; (4) They can promote and recruit through members' extramovement social networks. The question thus arises as to what determines the patterning and channelling of a movement's recruitment efforts.

Depending on their resource base and strategy, all nonsecretive movement organizations interested in expanding their ranks can exploit the first three recruitment possibilities. However, not all such movements are structurally able to use the network channel to the same extent since they do not all constitute open networks. Some movements are more restrictive and exclusive than others in that membership eligibility is contingent on the possession of certain ascribed or achieved attributes. In other movements, such as Hare Krishna, core membership may even be contingent upon the severance of extramovement interpersonal ties. Since movement organizations can vary in the extent to which they are linked to or isolated from other networks of social relations, it follows that their recruitment opportunities can vary considerably. Accordingly, it seems reasonable to suggest the following propositions regarding the relation between a movement's network attributes and the channelling and patterning of its recruitment efforts:

---

[12] This has been amply illustrated by a number of studies. In his examination of recruitment and conversion to the Unification Church, Lofland (1977a:306) found that initial contact "commonly involved an invitation to dinner, a lecture, or both" (see also Bromley and Shupe, 1979:171–4). Snow's (1976) findings regarding the recruitment practices of the Nichiren Shoshu movement reveal similar patterns. When recruiting in public places, Nichiren Shoshu members coaxed prospects to attend discussion meetings, chanting sessions, and other movement activities. Our observations of the recruitment efforts of Krishna devotees also indicate that the first step was to get prospects to the local temple. They were invited to attend a devotional session, to listen to discussion of the philosophy, or to attend a Sunday "love feast" (see also Judah, 1974).

[13] In arguing that the "motives" for movement participation are largely emergent and interactional in character, we are at least implicitly questioning the rational calculus assumptions accepted by resource mobilization theorists. That perspective is most clearly represented in the work of McCarthy and Zald (1973; 1977) and Oberschall (1973). For an extensive critique of this "utilitarian" strand of resource mobilization theory, see Fireman and Gamson (1979) and Zurcher and Snow (forthcoming).

> *Proposition 3A:* Movements requiring exclusive participation by their members in movement activities will attract members primarily from public places rather than from among extramovement interpersonal associations and networks.
>
> *Proposition 3B:* Movements which do not require exclusive participation by their members in movement activities will attract members primarily from among extramovement interpersonal associations and networks, rather than from public places.

These two propositions are suggested when we compare our findings regarding recruitment into Nichiren Shoshu with those pertaining to Hare Krishna. As mentioned earlier, more than three-quarters of our sample of NSA members reported that they were recruited into the movement by relatives, friends, or acquaintances. In contrast, only one of the twenty-five Krishna devotees we interviewed was recruited by a former acquaintance. Judah (1974:162) similarly found that only 3.32% of the Krishna devotees he interviewed "learned about it through a friend who was a devotee." The majority (66%) came into contact with the movement by encountering a public chanting session or a devotee in the street. Judah (1974:162–3) concludes that

> . . . the results seem to indicate rather clearly the importance of the Movement's method of proselytizing. It has attracted the attention of many by its practice of chanting the Hare Krishna mantra in public. During these public ceremonies, other devotees sell the literature and engage the curious in conversation about Krishna.

Anyone with firsthand knowledge of the Krishna movement will find little reason to quibble with Judah's (1974) observations. However, he fails to note that the movement in general and devotees in particular have little choice other than to turn to the streets in search of recruits. Since core membership in Hare Krishna requires an austere communal lifestyle and the severance of extramovement interpersonal ties, the movement is structurally compelled to concentrate its recruitment efforts in public places. Consequently, most of its members are recruited "off the streets."

In contrast, the Nichiren Shoshu movement does not involve communal life or require its members to sever their extramovement interpersonal ties. It is, therefore, structurally able to recruit both in public places and through members' extramovement networks. Consequently, it draws the bulk of its membership from those networks.

Implications of Recruitment Strategies for Movement Spread and Growth

The differential spread and growth of social movements has generally been analyzed in terms of the appeal of movement goals and value orientations to various target populations in the ambient society. That a movement's organizational structure, and particularly its network attributes, might function as an important determinant of its spread and growth has received only brief attention in the literature (Curtis and Zurcher, 1973; 1974; Messinger, 1955; Zald and Ash, 1966; Zurcher and Kirkpatrick, 1976). We now address this issue. Specifically, do differences in network attributes and recruitment opportunities have a significant impact on the success of a movement's recruitment efforts, as measured by the number of outsiders actually recruited? The answer hypothetically depends not only on a movement's value orientation, but also on whether recruitment among social networks typically yields a greater return than does recruitment "off the street" or through the media. If the latter is generally more productive, then a movement's network attributes would be of little relevance to its overall growth. However, if recruitment among strangers is not as effective as recruitment among movement members' acquaintances, friends, and relatives, then a movement's network attributes would constitute a significant and important variable in relation to its growth and spread.

Our findings indicate that for those movements which recruit both in public places and through networks, recruitment among acquaintances, friends, and kin is generally more successful than recruitment among strangers. We thus conclude that a movement's network attributes and corresponding recruitment patterns do indeed make a significant difference in a movement's recruitment efforts and growth. Hence, the following propositions:

> *Proposition 4A:* The success of movement recruitment efforts, measured by the numbers of outsiders actually recruited, will vary with the extent to which movements are linked to other groups and networks via members' extramovement interpersonal ties, such that:
>
> *Proposition 4B:* Movements which are linked to other groups and networks will normally grow at a more rapid rate and normally attain a larger membership than will movements which are structurally more isolated and closed.

Aside from a few exceptions (Craven and Wellman, 1974; Curtis and Zurcher, 1973; Turk, 1970), most social network analyses have focused on the individual and his or her interpersonal ties, with a paucity of social ties taken as evidence of objective or structural social isolation. That focus may be useful for understanding the structural determinants of individual behavioral patterns and propensities. However, it obscures the fact that social isolation can also occur at the group or movement level, as when a movement has few, if any, direct links with other groups and therefore constitutes a closed network or insulated system of social relations. When this occurs, Propositions 4A and 4B suggest that such movements will differ markedly from structurally less isolated movements in recruitment opportunities and patterns, and overall growth.

Empirical support is provided for the propositions by a comparison of the membership claims of the Nichiren Shoshu and Hare Krishna movement organizations in the United States. Since 1970 Nichiren Shoshu's membership claims have consistently hovered between 200,000 and 250,000. In contrast, members of the Los Angeles and Dallas Krishna communes have reported that the number of communal Krishna devotees throughout the country totals no more than 4,000. While efforts to reach an objective estimate of Nichiren Shoshu's membership suggest that it is about half of what the movement claims (see Snow, 1976:137–44), the point still remains that Nichiren Shoshu's membership far exceeds that of Hare Krishna. Given the fact that both are active, proselytizing movements, the question arises as to why the spread and growth of Nichiren Shoshu have far outdistanced those of Hare Krishna. Differences in the value orientation, promises, and membership demands of the two movements certainly constitute important variables in accounting for the difference in their spread and growth.[14] However,

---

[14] Regarding the differences in the value orientations and promises of the two movements, Nichiren Shoshu is more practical and this-worldly in orientation. Rather than calling for a renunciation of worldly attachments and promising a transcendence of the material world, as is the case with Hare Krishna, Nichiren Shoshu promises the realization of personal enlightenment and happiness in the immediate here-and-now. It is thus reasonable to assume that Nichiren Shoshu would appeal to a greater number of people than would Hare Krishna. Relatedly, membership in Nichiren Shoshu is much less demanding. Not only are Krishna devotees required to surrender to the movement their personal possessions and desiderata, but they are also required to sever their preexisting, extramovement interpersonal associations and ties. In contrast, Nichiren Shoshu members are merely required to surrender some time and energy, and perhaps be willing to risk a negative

Propositions 4A and 4B point to a perhaps more significant determinant of the differential spread and growth not only of these two movement organizations, but also of social movement organizations in general: the extent to which a movement organization is linked to or structurally isolated from other groups and networks within its environment of operation.

SUMMARY AND CONCLUSIONS

In recent years the study of differential recruitment to and the differential spread and growth of social movements has been characterized by concern with the process through which movement organizations strategically expand their ranks and mobilize support for their cause. Yet, as both Useem (1975:43) and Zald and McCarthy (1979:240) have noted, there has been relatively little systematic research conducted on the details of the influence process. In order to shed greater empirical and theoretical light on the recruitment process, we have presented data derived from three sources. Our findings indicate that the probability of being recruited into a particular movement is largely a function of two conditions: (1) links to one or more movement members through a preexisting or emergent interpersonal tie; and (2) the absence of countervailing networks. The first condition suggests who is most likely to be brought directly into a movement organization's orbit of influence and thereby subjected to its recruitment and reality-construction efforts. The second indicates who is structurally most available for participation and therefore most likely to accept the recruitment invitation. Our findings also indicate that a movement's recruitment strategies and its resultant growth will vary considerably in the degree to which it constitutes a closed or open network of social relations. Taken together, these findings indicate that both the network attributes of movement organizations and members function as important structural determinants of differential recruitment to and the differential growth of movement organizations.[15]

Several theoretical and empirical implications are suggested by our findings and correspondent propositions. First, in contrast to the traditional assumption that movement recruitment efforts are largely a function of goals and ideology, our findings indicate that the mobilization process in general and the recruitment process in particular are likely to vary significantly with changes in organizational structure. Indeed, our findings indicate that movement goals and organizational structure may occasionally contradict each other. When this occurs, as in the case of Hare Krishna, the organizational structure will function as the more important determinant of recruitment patterns.

Second, since social networks constitute microstructures, the findings suggest that microstructural variables are of equal, and perhaps greater, importance than dispositional susceptibilities in the determination of differential recruitment. We do not urge that dispositions be wholly ignored in attempts to understand the recruitment process. The notion "disposition" perhaps usefully could be integrated with the network perspective by conceptualizing it in terms of the normative, instrumental and affective ties among current and potential participants. People can become encapsulated (Lofland, 1977a; 1977b) by a movement when they are "dispositionally" linked to the proselytizing network, and when that connection becomes expanded by exposure to increasingly broadened socialization messages.

---

reaction by nonmovement significant others. In short, not only is it easier to be a Nichiren Shoshu member than a Krishna devotee, but Nichiren Shoshu adherents also live in and are "into" the here-and-now material world to a much greater extent than are the saffron-robed chanters and promoters of the Krishna mantra.

[15] Our concentration upon microstructural factors in movement recruitment is not intended to detract scholars from equally necessary macrostructural probings of movement emergence, growth and decline. Blumer's (1951) identification of general social movements as seedbeds for specific movements remains heuristically important. The shifts in societal trends, especially in values and their interpretations, can influence the relevance of networks (and rhetoric in networks) for potential movement recruits.

Third, our analysis suggests that the question of "why" people join social movements cannot be adequately understood apart from an examination of the process of "how" individuals come to align themselves with a particular movement. Indeed, it is our contention that the "whys" or "reasons" for joining arise out of the recruitment process itself. We would thus argue that further examination of movement joining and participation should give more attention to how movements solicit, coax, and secure participants, and more attention to the factors that account for variations in recruitment strategies and their efficacy. An examination of such factors should move us beyond our current knowledge about the recruitment process, which has, according to Marx and Wood (1975:393), reached "a point of diminishing returns."

REFERENCES

Aberle, David
1965 "A note on relative deprivation theory as applied to Millenarian and other cult movements." Pp. 537–41 in W. Lessa and E. Vogt (eds.), Reader in Comparative Religion: An Anthropological Approach. New York: Harper and Row.
1966 The Peyote Religion Among the Navaho. Chicago: Aldine.

Becker, Howard S.
1960 "Notes on the concept of commitment." American Journal of Sociology 66:32–40.

Bibby, Reginald W. and Merlin B. Brinkerhoff
1974 "When proselytizing fails: an organizational analysis." Sociological Analysis 35:189–200.

Blumer, Herbert
1951 "Collective behavior." Pp. 166–222 in A. M. Lee (ed.), New Outline of the Principles of Sociology. New York: Barnes and Noble.

Bolton, Charles D.
1972 "Alienation and action: a study of peace group members." American Journal of Sociology 78:537–61.

Bromley, David A. and Anson D. Shupe, Jr.
1979 "Moonies" in America. Beverly Hills: Sage.

Cantril, Hadley
1941 The Psychology of Social Movements. New York: Wiley.

Craven, Paul and Barry Wellman
1974 "The network city." Pp. 57–88 in M. Effrat (ed.), The Community: Approaches and Applications. New York: Free Press.

Curtis, Russell L., Jr., and Louis A. Zurcher, Jr.
1973 "Stable resources of protest movements: the multi-organizational field." Social Forces 52:53–61.
1974 "Social movements: an analytical exploration of organizational forms." Social Problems 21:356–70.

Daner, F. J.
1976 The American Children of Krishna: A Study of the Hare Krishna Movement. New York: Holt, Rinehart and Winston.

Darley, John M. and C. Daniel Batson
1973 "From Jerusalem to Jericho: a study of situational and dispositional variables in helping behavior." Journal of Personality and Social Psychology 27:100–8.

Dator, James
1969 Soka Gakkai: Builder of the Third Civilization. Seattle, Wash.: University of Washington Press.

Davies, James C. (ed.)
1971 When Men Revolt and Why. New York: Free Press.

Etzioni, Amatai
1975 A Comparative Analysis of Complex Organizations. New York: Free Press.

Feuer, Lewis
1969 The Conflict of Generations. New York: Basic Books.

Fireman, Bruce and William A. Gamson
1979 "Utilitarian logic in the resource mobilization perspective." Pp. 8–44 in M. Zald and J. McCarthy (eds.), The Dynamics of Social Movements. Cambridge, Mass.: Winthrop Publishers.

Freeman, Jo
1973 "The origins of the women's liberation movement." American Journal of Sociology 78:792–811.

Gerlach, Luther P. and Virginia H. Hine
1970 People, Power and Change: Movements of Social Transformation, Indianapolis: Bobbs-Merrill.

Glock, Charles Y.
1964 "The role of deprivation in the origin and evolution of religious groups." Pp. 24–36 in R. Lee and M. Marty (eds.), Religion and Social Conflict. New York: Oxford University Press.

Goffman, Erving
1963 Behavior in Public Places. New York: Free Press.

Gurr, Ted R.
1970 Why Men Rebel. Princeton: Princeton University Press.

Harrison, Michael L.
1974 "Sources of recruitment to Catholic Pentecostalism." Journal for the Scientific Study of Religion 13:49–64.

Heirich, Max
1977 "Change of heart: a test of some widely held theories about religious conversion." American Journal of Sociology 83:653–80.

Hoffer, Eric
1951 The True Believer. New York: Harper and Row.

Jackson, Maurice, E. Peterson, J. Bull, S. Monsen and P. Richmond
1960 "The failure of an incipient social move-

ment." Pacific Sociological Review 3:35–40.

Judah, J. Stillson
1974 Hare Krishna and the Counterculture. New York: Wiley.

Kanter, Rosabeth M.
1972 Commitment and Community: Communes and Utopias in Sociological Perspective. Cambridge, Mass.: Harvard University Press.

Klapp, Orrin
1969 Collective Search for Identity. New York: Holt, Rinehart and Winston.

Lachman, Sheldon J. and Benjamin Singer
1968 The Detroit Riot: 1967. Detroit: Behavior Research Institute.

Leahy, Peter J.
1975 "The anti-abortion movement: testing a theory of the rise and fall of social movements." Ph.D. dissertation. Department of Sociology, Syracuse University.

Lee, Robert
1967 Stranger in the Land. London: Lutterworth.

Lewis, Steven H. and Robert E. Kraut
1972 "Correlates of student political activism and ideology." Journal of Social Issues 28:131–49.

Lipset, S. M.
1960 Political Man. New York: Doubleday and Company

Lipset, S. M. and Earl Raab
1973 The Politics of Unreason: Right-Wing Extremism in America, 1790–1970. New York: Harper and Row.

Lofland, John
1966 Doomsday Cult. Englewood Cliffs, N.J.: Prentice-Hall.
1977a Doomsday Cult. Enlarged Edition. New York: Irvington Publishers.
1977b "Becoming a world-saver revisited." American Behavioral Scientist 20:805–19.

Lyman, Stanford M. and Marvin B. Scott
1967 "Territoriality: a neglected sociological dimension." Social Problems 15:236–48.

Marx, Gary T.
1969 Protest and Prejudice. New York: Harper and Row.

Marx, Gary T. and James Wood
1975 "Strands of theory and research in collective behavior." Pp. 363–428 in A. Inkeles et al. (eds.), Annual Review of Sociology I. Palo Alto, Calif.: Annual Reviews.

McCarthy, John and Mayer Zald
1973 The Trend of Social Movements in America: Professionalization and Resource Mobilization. Morristown, N.J.: General Learning Press.
1977 "Resource mobilization and social movements: a partial theory." American Journal of Sociology 82:1212–41.

McPhail, Clark
1971 "Civil disorder participation: a critical examination of recent research." American Sociological Review 36:1058–73.

McPhail, Clark and David Miller
1973 "The assembling process: a theoretical and empirical examination." American Sociological Review 38:721–35.

Messinger, Sheldon L.
1955 "Organizational transformation: a case study of a declining social movement." American Sociological Review 20:3–10.

Mills, C. Wright
1940 "Situated actions and vocabularies of motive." American Sociological Review 5:404–13.

Moinat, Sheryl, W. Raine, S. Burbeck and K. Davison
1972 "Black ghetto residents as rioters." Journal of Social Issues 28:45–62.

Murata, Kiyoaki
1969 Japan's New Buddhism: An Objective Account of Soka Gakkai. New York: John Weatherhill.

National Advisory Commission on Civil Disorders
1968 Report of the National Advisory Commission on Civil Disorders. New York: Bantam Books.

Oberschall, Anthony
1973 Social Conflict and Social Movements. Englewood Cliffs, N.J.: Prentice-Hall.

Oppenheimer, Martin
1968 "The student movement as a response to alienation." Journal of Human Relations 16:1–16.

Orum, Anthony
1972 Black Students in Protest: A Study of the Origins of the Black Student Movement. Washington, D.C.: American Sociological Association, Arnold M. and Caroline Rose Monograph Series.

Parsons, T. and E. A. Shils
1962 Toward a General Theory of Action. New York: Harper and Row.

Petras, James and Maurice Zeitlin
1967 "Miners and agrarian radicalism." American Sociological Review 32:578–86.

Pinard, Maurice
1971 The Rise of a Third Party: A Study in Crisis Politics. Englewood Cliffs, N.J.: Prentice-Hall.

Portes, Alejandro
1971 "On the logic of post-factum explanations: the hypothesis of lower-class frustration as the cause of leftist radicalism." Social Forces 50:26–44.

Ransford, H. Edward
1968 "Isolation, powerlessness and violence: a study of attitudes and participation in the Watts riot." American Journal of Sociology 73:581–91.

Sills, Davis L.
1957 The Volunteers. Glencoe, Ill.: Free Press.

Snow, David A.
1976 The Nichiren Shoshu Buddhist Movement in America: A Sociological Examination of Its Value Orientation, Recruitment Efforts and Spread. Ann Arbor, Mich.: University Microfilms.
1979 "A Dramaturgical Analysis of Movement Accommodation: Building Idiosyncrasy Credit as a Movement Mobilization Strategy." Symbolic Interaction 2:23–44.

Snow, David A. and Cynthia L. Phillips
1980 "The Lofland-Stark conversion model: a critical reassessment." Social Problems 27:430–47.

Snyder, David and William R. Kelly
1979 "Strategies for investigating violence and social change: illustrations from analyses of racial disorders and implications for mobilization research." Pp. 212–37 in M. Zald and J. McCarthy (eds.), The Dynamics of Social Movements. Cambridge, Mass.: Winthrop Publishers.

Snyder, David and Charles Tilly
1972 "Hardship and collective violence in France, 1830–1960." American Sociological Review 37:520–32.

Tilly, Charles
1978 From Mobilization to Revolution. Reading, Mass.: Addison-Wesley Publishing Co.

Toch, Hans
1965 The Social Psychology of Social Movements. Indianapolis: Bobbs-Merrill.

Turk, Herman
1970 "Interorganizational networks in urban society: initial perspectives and comparative research." American Sociological Review 35:1–20.

Turner, Ralph H. and Lewis M. Killian
1972 Collective Behavior. Englewood Cliffs, N.J.: Prentice-Hall.

Tygart, Clarence E. and Norman Holt
1972 "Examining the Weinberg and Walker typology of student activists." American Journal of Sociology 77:957–66.

Useem, Michael
1975 Protest Movements in America. Indianapolis: Bobbs-Merrill.

Von Eschen, Donald, Jerome Kirk, and Maurice Pinard
1971 "The organizational substructure of disorderly politics." Social Forces 49:529–44.

White, James W.
1970 The Sokagakkai and Mass Society. Stanford: Stanford University Press.

Wilson, John
1973 Introduction to Social Movements. New York: Basic Books.

Wilson, Kenneth and Tony Orum
1976 "Mobilizing people for collective political action." Journal of Political and Military Sociology 4:187–202.

Woelfel, Joseph, John Woelfel, James Gillham and Thomas McPhail
1974 "Political radicalization as a communication process." Communication Research 1:243–63.

Zald, Mayer N. and Roberta Ash
1966 "Social movement organizations: growth, decay and change." Social Forces 44:327–41.

Zald, Mayer N. and John D. McCarthy (eds.)
1979 The Dynamics of Social Movements: Resource Mobilization, Social Control, and Tactics. Cambridge, Mass.: Winthrop Publishers.

Zurcher, Louis A. and R. George Kirkpatrick
1976 Citizens for Decency: Antipornography Crusades as Status Defense. Austin, Texas: University of Texas Press.

Zurcher, Louis A. and David A. Snow
Forthcoming "Collective behavior: social movements." In Morris Rosenberg and Ralph Turner (eds.), Sociological Contributions to Social Psychology. New York: Basic Books.

Zygmunt, Joseph
1972 "Movements and motives: some unresolved issues in the psychology of social movements." Human Relations 25:449–67.

THE DIFFERENTIAL SENTENCING OF WHITE-COLLAR OFFENDERS IN TEN FEDERAL DISTRICT COURTS*

JOHN HAGAN
University of Wisconsin–Madison
University of Toronto

ILENE H. NAGEL (BERNSTEIN)
Indiana University

CELESTA ALBONETTI
Indiana University

American Sociological Review 1980, Vol. 45 (October):802–820

While sociologists have long debated the relationship between the status characteristics of criminal offenders and the sentences they receive, they have done so with data sets drawn from state courts whose prosecutorial resources are focused almost entirely on low status defendants. Qualitative and quantitative data analyzed in this paper are drawn from ten federal district courts whose statutes and resources provide greater potential for the prosecution of the white-collar crimes of higher status offenders. Three questions are addressed: (1) Are there substantial jurisdictional differences in the prosecution of white-collar cases? if so, (2) Are there corresponding jurisdictional differences in the sentencing of white-collar cases? and (3) Within jurisdictions, are there further differences in the factors that influence sentencing decisions in white-collar as compared to other kinds of cases? The data are analyzed from a perspective that emphasizes organizational considerations: we conceptualize the criminal justice process as a loosely coupled system and the use of prosecutorial resources as proactive and reactive. We argue that the expanded prosecution of white-collar persons for their white-collar crimes requires a proactive prosecutorial policy and a tightening of the coupling between plea negotiations and sentencing decisions in the prosecutorial and judicial subsystems. Our quantitative analysis reveals that one district follows a uniquely proactive pattern. As expected, this proactive district also exhibits a unique leniency in the sentencing of college educated white-collar criminals that is related to earlier plea and charging decisions. A rather different and unanticipated pattern of leniency is found in this district for less educated white-collar offenders. A conclusion of this study is that there may be an inverse relationship between the volume of white-collar prosecutions and the severity with which they are sentenced.

The existence of systematic links between the status characteristics of criminal offenders and the sentences they receive has been debated for some time (see Hagan, 1974). This debate is grounded in issues of theory and policy. For example, Chiricos and Waldo (1975) regard a relationship between class position and sentencing as crucial to one prominent version (Chambliss and Seidman, 1971) of a conflict theory of crime, while the absence of such a relationship is usually deemed essential to notions of "equality before the law." It therefore is not surprising that this type of debate has generated a large volume of research (see Nettler, 1979:40–6). What *is* surprising is the inconclusiveness of the findings that flow from this work.

The problem is not simply that these findings are inconsistent, although they are that. For all the studies that find little (e.g., Bernstein et al., 1977) or no (e.g., Chiricos and Waldo, 1975) relationship between status characteristics and sentencing, there are still those that find this relationship to be substantial (e.g., Swigert and Farrell, 1977; Lizotte, 1978). However, the larger issue is that the data sets on which these studies are based are widely thought to be inadequate. Specifically, critics (e.g., Greenberg, 1977; Hopkins, 1977; Reasons, 1977) note that the samples considered in these studies consist almost entirely of low status defendants, making this research mainly a matter of within- rather than between-class comparisons. Thus, while (to date) research of this type has focused on the sentencing of "traditional" or "common"

---

* Direct all correspondence to: John Hagan; Department of Sociology; Erindale College; University of Toronto; Mississauga, Ontario L5L-1C6.

Research reported in this paper was funded by the Crime and Delinquency section of the National Institute of Mental Health. The authors wish to thank the anonymous reviewers of this manuscript for their helpful comments.

crimes (e.g., murder, robbery, larceny, etc.), Hopkins (1977:177) points out that "The illegal activities of the middle and upper classes are typically such things as income tax evasion and, in the case of businessmen, price fixing, violation of pollution laws and misrepresentation in advertising."

Three problems account for the failure of recent research to consider the latter kinds of crimes, which usually are designated vaguely as "white-collar crimes." First, there is an uncertainty about what, and who, to study. The difficulty is that not all white-collar crimes (e.g., income tax violations) are committed by white-collar persons, and not all white-collar persons commit white-collar crimes (e.g., the crimes of Patty Hearst). We respond to this problem in this paper by cross-classifying indicators of the status of the offender *and* the offense. This allows us to compare the prosecution and sentencing of the white-collar crimes of white-collar persons with the prosecution and sentencing of other offender-offense combinations (see later pages, herein, for further elaboration of this point). Second, the former kinds of white-collar crimes are prosecuted with their greatest frequency in the American federal courts. To date, most sociological research on sentencing has been done in the state courts. Third, much of the white-collar crime of white-collar persons is "beyond incrimination" (Kennedy, 1970). That is, much of this white-collar "indiscretion" is handled in the civil courts, or not dealt with at all. However, this situation shows signs of change.

Some U.S. Attorney offices recently have begun to take increased initiative in the prosecution of white-collar persons and their white-collar crimes. The quantitative data analyzed in this paper consist of cases prosecuted and sentenced over a several-year period in ten federal district courts, including one of the first U.S. Attorney offices in this country reputed to have made the prosecution of white-collar cases a high priority; qualitative data, as well, were gathered through observations and interviews in these courts. The latter data are used first to ground the following discussion of "white-collar justice."

THE SOCIAL ORGANIZATION OF WHITE-COLLAR JUSTICE

To understand the sanctioning of white-collar persons and their white-collar crimes we must consider the system in which this occurs. In this section, we develop a perspective for viewing the operations of this system. The perspective we offer is derived from the work of others and from the interviews and observations gathered in site visits to the ten district courts. The districts and their principal cities are Eastern and Southern New York (Brooklyn and Manhattan), Northern Illinois (Chicago), Eastern Pennsylvania (Philadelphia), Maryland (Baltimore), Northern Texas (Dallas), Western Missouri (Kansas City), Northern Georgia (Atlanta), Central California (Los Angeles), and Eastern Michigan (Detroit). During the site visits we observed approximately 200 hours of court proceedings and conducted approximately 600 hours of interviews with the following court personnel: 9 Chief Judges and 42 Presiding Judges, 8 United States Attorneys and 48 Assistant United States Attorneys, 14 Probation Officers, 15 Administrators of Pre-Trial Services Agencies, 31 Magistrates, and 10 Chiefs of Public Defender Offices.[1]

Reiss (1971; 1974) conceptualizes the criminal justice system as a loosely articulated operating network of input-output relationships among a series of subsystems; a set of relationships that we will refer to as a "loosely coupled system" (Hagan et al., 1979; see also Meyer and Rowan, 1977). Discretion is dispersed throughout this system, and mechanisms for systematizing this discretion appear to be the exception as much as the rule. Thus, Gibbs (1978:105) observes that "... the system actually appears to be an ungoverned mishmash," and Eisenstein and Jacob (1977:37) note that even "the judge does not rule or govern; at most, he manages, and often he is managed by others." Reiss (1971:120) goes on to suggest that "the major means of control among the subsystems is *internal* to each" with the significant consequence that "each subsystem creates its own system of justice." This situation becomes problematic when the attempt is made in such a system to establish or shift policies and priorities. Indeed, one of the fascinating features of loosely coupled systems is their ability to circumvent such changes (see Meyer and Rowan, 1977).

---

[1] These interviews were conducted over a ten-week period, with one week spent in each of the ten districts. The ten jurisdictions comprise a purposive sample selected by the Supreme Court (under provisions of the Speedy Trial Act; see fn. 4) and intended to maximize the representation of major metropolitan and geographic areas across the United States. The first two authors of this paper conducted the interviews together, using a set of structured, open-ended interview schedules that are available on request. Our purpose was to interview a cross-section of court personnel across the ten districts. Unedited excerpts from these interviews are quoted in this article. One Chief Judge refused to be interviewed and two U.S. Attorneys were not available for interviews. However, the First Assistant to each of the latter U.S. Attorneys was interviewed and our coverage otherwise was quite comprehensive. These qualitative data were generated to correspond to the quantitative data discussed later in this paper.

Yet important changes in policies and priorities sometimes do occur, with systemwide repercussions. Within the criminal justice system, such changes are often implemented through the prosecutorial subsystem. Reiss (1974:690) indicates why and how:

> By legal authority and by practice, prosecutors have the greatest discretion in the formally organized criminal justice network. . . . The way that prosecutors exercise discretion over input and output varies considerably among jurisdictions. This variation is due partly to the organized forms of discretion available to a prosecutor in a given jurisdiction and partly to historical practice within that office. The discretionary decisions of prosecutors whether or not to file information can exercise substantial control over input into the system, while the quantity and quality of output are determined mainly by their decisions to nol pros or to plea bargain.

Drawing from Reiss (1971), Black (1973) and our own interviews, we suggest that this prosecutorial power can be exercised in two principal ways.

On the one hand, prosecutors can follow a *reactive* policy of law enforcement. Following this strategy, prosecutors can respond to police initiatives in the same way in which the police do to citizen complaints. That is, prosecutors can simply respond to what the police bring to their attention. Indeed, the influx of cases, the absence of ready avenues of diversion, and the scarcity of resources is such that most *state* courts can do little but react, and slowly at that. These are "courts of last resort." Many federal prosecutors also are reactive. Their assumption is often that court resources are most efficiently organized to satisfy the demands of enforcement agencies. This assumption is reflected in the explanation given us by a U.S. Attorney for his reluctance to pursue one type of white-collar crime.

> It would be nice to investigate, let's say, public corruption. "Okay, FBI, I want you to go out and develop snitches in all the HEW places where they might be taking bribes" . . . but God knows how much time [that would take] and we don't have the resources to do that. If I had some prosecutors or some agents to whom I could say, "Okay, I don't mind you wasting a year investigating this because we want it looked into," then I could see doing that. But if you don't have the resources to do it, I just don't feel you are using your resources right.

This reactive view is summarized in a quote from another U.S. Attorney interview: "In my opinion, any time you deny an agency the right to enforce its federal laws, you are not doing the job you are put here for."

Other federal prosecutors find, in their roles, sources of flexibility that allow the development of *proactive* policies. First, federal prosecutors have jurisdiction over a broad range of white-collar offenses. Second, federal prosecutors can decline cases or defer them to state courts, reserving resources for cases they assign higher priority. Third, the ratio of personnel to cases is usually more favorable in federal than in state courts. Fourth, federal prosecutors often have investigatory resources—particularly, federal agencies like the Federal Bureau of Investigation, the Postal Service and the Internal Revenue Service—that the state courts do not. Consequently, U.S. Attorney offices in several large jurisdictions allocate their resources to proactively initiate and encourage the investigation and prosecution of high priority cases. Thus, one U.S.

Attorney interviewed noted that "In terms of the decision-making process of how and what cases will be handled and what will be done, a lot [of decisions] are being made here and not in the agency." One way this is done is as follows.

> The U.S. Attorney's office can investigate cases in the grand jury, where it generates information. In other words, we can initiate an investigation. What we do often is we initiate investigations and then bring in the agency that will have jurisdiction in that area and be working with that agency in terms of developing the grand jury investigation; but we are not limited to only acting on cases that come into the office after an agency has done an investigation. We ourselves can initiate the investigation, through the grand jury.

This proactive attitude is summarized in the observation of another U.S. Attorney that: "We don't sit back and wait for cases to walk in the door. We go out and make them."

However, use of grand juries is not sufficient to build important white-collar cases in a proactive fashion. Information and evidence are needed to begin building these cases. While in other prosecutions such information frequently comes from victims and witnesses, the white-collar crimes of white-collar persons usually are different: the complexity and diffuseness of the victimizations reduce the visibility of these crimes and, therefore, the likelihood of obtaining evidence from nonimplicated persons.[2] A U.S. Attorney explains the problem this way:

> . . . in these sort of activities, the only people with the information that you are going to have to convict are the participants. It is not like a bank robbery where innocent people watch and see and identify. The only way you can get these kinds of criminals is through information supplied by participants. You have to peel off the layers . . . and that is difficult. That is a burden that is tougher than the burden prosecutors in the past had to deal with.

The questions that follow are What is the exact nature of this burden? and Why is this burden so difficult to bear? Our interviews suggest that the nature of the burden is negotiation: the development of cooperative witnesses through plea negotiation. The process and problems involved are suggested in another segment of the above interview.

> The negotiation proceeds within the confines of reality. That is the advantage I think good defense counsel has. . . . He knows and can tell his client what is likely to happen, and how likely it is to happen. If we've got a weak case, we've got a weak hand. If you've got a really strong case, you hold four aces on the top and he doesn't have any choice. Well, he does have a choice. Some people just go down. They take their lumps and they don't want to talk. . . . One of the last chips we have is that we can say not only are we going to convict you and send you to prison and you aren't going to get anything. After that is all done, then we will put you in the grand jury and we will get the information anyway and you won't get any credit for it. Indeed at sentencing we will stand up and make a point of the fact that you refused to cooperate and that will be held against you and you will get even more time.

The proactive prosecution of white-collar persons and their white-collar crimes, therefore, comes down to the problem of how to get the leverage required to "turn witnesses," and the key to obtaining this leverage is to forge a connection between plea negotiations and concessions and coercion in sentencing. In other words, prosecutors must overcome the tendency toward loose coupling between most parts of the criminal justice system, establishing instead a direct connection between plea negotiations and sentencing decisions in white-collar cases. This can be accomplished in at least two ways: by carefully managing the severity of the charges in these cases, so that judges can use statutory guidelines in arriving at lenient sentences, and by getting judges to reward negotiated pleas directly. That such sentencing decisions *are generated* is suggested by the following observation of an Assistant U.S. Attorney in charge of a subsection responsible for the prosecution of official corruption in the district best known in our sample for its early emphasis on white-collar crime: "I would say most judges understand that in order to expose official corruption you do have to give some concessions to people who are involved. Again, because only those people who are involved know and can testify about it."[3] Depending on how extensive these understandings are, one implication of the connection we have drawn between negotiation and sentencing will be an overall tendency toward the lenient sentencing of white-collar persons convicted of white-collar crimes. In the remainder of this paper we use our quantitative data to examine evidence of this and related possibilities outlined in the perspective we have developed.

---

[2] Beyond this, even when such evidence might be obtained through record searches, accounting procedures, and the analysis of documents, the amounts of material and the methods of investigation are so costly in man-hours and resources, both to develop and to present in court, that such efforts rarely are undertaken.

THE PROSECUTION AND SENTENCING OF WHITE-COLLAR CRIME IN TEN FEDERAL DISTRICT COURTS

The quantitative data we will consider consist of 9,068 cases prosecuted, and 6,562 cases sentenced, in our ten district courts over a period beginning in 1974 and ending in 1977. These data were collected by the administrative office of United States Courts, and our use of the data is premised on an agreement not to identify individual districts in our analysis.[4] Before beginning this analysis, we must specify our operationalization of the concept of white-collar crime.

Although the term "white-collar crime" has gained currency in several languages and in popular thought, there is disagreement about its definition (see Geis and Meier, 1977). As indicated earlier, much of the problem involves the kinds of *crimes* and kinds of *people* involved. Our analysis is based on an operational definition that allows consideration of both the offender *and* the offense. We began with a listing of all offenses in the United States

---

[3] The federal law itself can be an effective means toward this end. For example, the Mail Fraud Statute and the Travel Act have been used with considerable "legal craftsmanship" to successfully prosecute important white-collar cases (see Henderson, 1977). An indication of the flexible way in which these statutes have been used to generate cooperation and achieve convictions is suggested by the following rather sardonic excerpt from a dissenting appeal court opinion. This appeal resulted from the successful prosecution (during the period of our study in what we later identify as a very proactive district) of a major bribery case.

> I conclude by depicting a scenario which I have little doubt approximates the facts of this prosecution. Federal officials, getting wind of a deal between . . . state legislators and the . . . industry, assign agents to investigate. In due course, immunity is promised to some of the involved legislators and company officials in return for their cooperation and testimony. Recording devices are placed on some of the immunized persons to obtain inculpating admissions from those who are the targets of the prosecution. Once the investigation is completed, consideration is then given to what federal offense, if any, has been committed. The Mail Fraud Statute? The Federal Travel Act? The investigation files are searched to find some mailings or evidence of interstate travel. The United States Attorney's office sifts through the mailings in the file and then constructs a legal theory in order that they may be used to form the basis for a charge of mail fraud. One fortuitous trip, totally incidental and unforeseen, by an unindicted coschemer forms the basis of the two travel-act counts. A conspiracy count is, of course, added. In this fashion, the Mail Fraud Statute and the Travel Act are subverted to purposes for which they were never intended. No longer are the mailings and travel considered essential or an integral part of the scheme; they are seen and used to obtain federal jurisdiction.

In the above case, the central "unindicted coschemer" was separately charged with one count of making a false statement on his Income Tax return (that is, the bribe), to which he pleaded guilty. The statutory maximum sentence for the latter offense was three years imprisonment, and this offender received one year on probation. The other legislators in this case were charged with mail fraud, conspiracy, and travel-act violations, exposing them to potential concurrent sentencing and a statutory maximum sentence of five years imprisonment on one count of mail fraud alone. All pleaded not guilty; one was acquitted, while the others received three-year prison sentences and fines. A remaining group of corporate executives was charged with the same offenses, pleaded guilty, and received one to two years' probation and fines. This case usefully illustrates the important role which plea and charge negotiation can play in a proactive district in the prosecution and sentencing of important white-collar cases.

[4] These data were collected with special provisions for quality control and comprehensiveness made possible through a mandate of the Speedy Trial Act of 1974 to evaluate the experimental bail reform program established under this Act. All but 88 cases (which came into the data set in 1974) are from the years 1975 through 1977. The provisions of the evaluation were that a population of cases was to be collected during this period. Our interviews in all ten districts indicated full cooperation in the fulfillment of this mandate.

Code that could plausibly be thought of as white-collar crimes.[5] We then refined this classification by asking U.S. Attorneys in the ten districts to approve or disapprove of the inclusion of offenses, asking each to identify offense codes that were almost always white-collar, sometimes white-collar, and almost never white-collar. Thirty-one offenses that elicited considerable consensus as white-collar crimes were retained,[6] and, for the purposes of this research, the other offenses were designated as common crimes. We proceeded by cross-classifying the dichotomy with two dichotomized measures of the offender's social standing: education (high school or less, and college or more) and income (less than $13,777, and $13,777 or more, per year, in 1974–1977 dollars). These are the most direct measures of social standing available in our data, and we have dichotomized them on the basis of Featherman and Hauser's (1978) recent replication of the work of Blau and Duncan (1967). Thus, our cut points correspond to the highest grouping of occupations considered in these studies. Analyses reported in this paper have been done separately for the education and income measures, with, as we will see partially in Table 1, substantially similar results. However, in deference to space limitations, we present only the results involving education beyond Table 1. Education was selected over income for two reasons: it allows us to consider women without reported incomes in a more meaningful manner, and it avoids the problem of correcting for a deflating dollar over the several-year period. The resulting cross-classification that forms a central part of our analysis includes the following four kinds of cases: the common crimes of the less educated; the common crimes of the college educated; the white-collar crimes of the less educated; and the white-collar crimes of the college educated. As indicated earlier, the latter type of case is of greatest interest to us because it is the "purest" form of white-collar crime we can identify and because we expect prosecution of this type of case to exhibit the most interesting variation across districts.

The first step in our analysis was to examine the distribution of prosecutions and dispositions for the four offender-offense combinations in all ten districts. What we found was a striking similarity in these distributions for nine of the ten districts. The tenth, which we call District C,

---

[5] The original listing was made available to the authors by Stanton Wheeler and John Cardascia and consists of 53 offenses abstracted from the Code.

[6] A short description follows of 27 of these offenses, with the Title number, Section number, and number of cases involving a college educated offender indicated in parentheses: trust, etc., in restraint of trade (15:1:51); bankruptcy—concealment of assets, false oaths and claims, bribery (18:152:1); bribery, graft, and conflicts of interest—bribery of public officials and witnesses (18:201:5); bribery, graft, and conflict of interest—salary of government officials and employees payable only by U.S. (18:209:1); bribery, graft, and conflict of interest—offer to procure appointed public office (18:210:1); claims and services in matters that affect government—conspiracy to defraud the government in respect to claims (18:286:1); claims and services in matters that affect the government—false or fraudulent claims (18:287:12); embezzlement and theft—accounting for public money (18:643:1); embezzlement and theft—custodians generally misusing public funds (18:648:1); embezzlement and theft—theft, embezzlement or misapplication by bank officers or employees (18:656:57); embezzlement and theft—lending, credit, and insurance institutions (18:657:6); embezzlement and theft—property mortgaged or pledged to farm credit agencies (18:658:1); embezzlement and theft—theft or embezzlement from employee benefit plan (18:664:1); fraud and false statements—statements or entries generally (18:1001:23); fraud and false statements—bank entries, reports, or false transactions (18:1005:2); fraud and false statements—federal crime institutions entries, reports, and transactions (18:1006:1); fraud and false statements—Department of Housing and Urban Development and Federal Housing Administration transactions (18:1010:2); fraud and false statements—Department of Housing and Urban Development transactions (18:1012:1); fraud and false statements—loan and credit applications generally, also renewals and discounts, crop insurance (18:1014:14); mail fraud—frauds and swindles (18:1341:56); mail fraud—fictitious name or address (18:1342:2); mail fraud—fraud by wire, radio, or telephone (18:1343:6); attempt to evade or defeat tax (26:7201:11); failing to file tax return (26:7203:28); fraud and false statements (26:7206:23); fiduciary responsibility of officers of labor organizations (29:501:1); fraudulent acceptance of payments—veteran's benefit (38:3502:6). Four additional offenses (18:2073; 26:7207; 26:7262; 49:322) were designated as white-collar by U.S. Attorneys in our interviews. However, these offenses did not result in convictions for college educated offenders in our data.

Table 1. Prosecutions and Dispositions for Offender-Offense Combinations in District C and Other Districts*

	District C			Other Districts		
			Prosecutions			
	Education			Education		
Crime	High School	College		High School	College	
Common	83.9%	16.1%		83.3%	16.7%	
Crime	86.3%	56.7%	79.6%	91.6%	80.5%	89.5%
	66.7%	12.8%		74.6%	15.0%	
	(598)	(115)	(713)	(6094)	(1222)	(7316)
White-	51.9%	48.1%		64.4%	34.6%	
Collar	13.7%	43.3%	20.4%	8.4%	19.5%	10.4
Crime	10.6%	9.8%		6.5%	3.6%	
	(95)	(88)	(183)	(560)	(296)	(856)
			Convictions			
	Education			Education		
Crime	High School	College		High School	College	
Common	83.7%	16.3%		83.9%	16.1%	
Crime	84.1%	51.5%	76.2%	90.7%	78.2%	88.4%
	63.8%	12.4%		74.2%	14.3%	
	(443)	(86)	(529)	(4352)	(838)	(5190)
	$\bar{X}$ sentence = 8.04	$\bar{X}$ sentence = 5.60		$\bar{X}$ sentence = 7.35	$\bar{X}$ sentence = 6.59	
White	50.9%	49.1%		65.6%	34.4%	
Collar	15.9%	48.5%	23.8%	9.3%	21.8%	11.6%
Crime	12.1%	11.7%		7.6%	4.0%	
	(84)	(81)	(165)	(445)	(233)	(678)
	$\bar{X}$ sentence = 4.13	$\bar{X}$ sentence = 3.49		$\bar{X}$ sentence = 4.43	$\bar{X}$ sentence = 4.64	
			Convictions			
	Income (1974–1977 $)			Income (1974–1977 $)		
Crime	13,776−	13,777+		13,776−	13,777+	
Common	89.8%	10.2%		92.9%	7.1%	
Crime	85.4%	39.1%	76.2%	90.0%	72.3%	88.4%
	68.4%	7.8%		82.1	6.3%	
	(475)	(54)	(529)	(4,819)	(371)	(5,190)
	$\bar{X}$ sentence = 7.60	$\bar{X}$ sentence = 8.11		$\bar{X}$ sentence = 7.31	$\bar{X}$ sentence = 6.18	
White	49.1%	50.9%		79.1%	20.9%	
Collar	14.6%	60.9%	23.8%	10.0%	27.6%	11.6%
Crime	11.7%	12.1%		9.1%	2.4%	
	(81)	(84)	(165)	(536)	(142)	(678)
	$\bar{X}$ sentence = 4.10	$\bar{X}$ sentence 3.55		$\bar{X}$ sentence = 4.38	$\bar{X}$ sentence = 4.98	

* Each cell of each cross-classification is percentaged first by row, second by column, and third in relation to the full cross-classification.

was distinguished by its disproportionate prosecution of white-collar persons involved in white-collar crimes. Table 1 presents the distribution of prosecutions and dispositions, by offender and offense, in District C as compared to the other nine districts.

Table 1 reveals that while 9.8% of the prosecutions and 11.7% of the convictions in District C include college educated persons involved in white-collar crimes, the respective figures for the remaining districts are 3.6% (ranging from 2.7% to 4.5%) and 4% (ranging from 2.4% to 5.4%). When income is used as the measure of social standing in Table 1, the disparity in convictions between District C and the other districts increases slightly to 12.1% as compared with 2.4%. At a minimum, then, District C prosecutes and convicts more than twice the proportionate number of white-collar persons for white-collar crimes as do the other districts. Thus, although the caseloads of none of the districts are overcrowded with the latter types of cases, both in prosecutions and convictions, District C is clearly the deviant case. Yet another way of making this point is to indicate that, while District C prosecutes only 9.9% of the cases handled in all ten districts, it prosecutes 22.9% of the white-collar cases involving college educated persons. The respective conviction figures are 10.6% and 25.8%, and 10.6% and 37.1% when income replaces education as the measure of social standing. These data suggest that whether education or income is used as the measure of social standing, District C has followed a uniquely proactive pattern in its prosecution of white-collar crime.[7]

Returning to Table 1, we should also note that in none of the districts does the prosecution of white-collar crime predominately involve what might conventionally be regarded as white-collar persons. Approximately half of the white-collar crimes prosecuted (48.1%) and convicted (49.1%) in District C involve college educated persons, compared to about one-third in the other districts (34.6%; 34.4%). A similar pattern prevails in the part of Table 1 that replaces education with income. Thus, although District C again demonstrates a more proactive emphasis on the white-collar crimes of white-collar persons, this emphasis is by no means exclusive. This finding is consistent with the observation of Katz (1979:433) that "There are relatively few crimes that can be committed only by those in white-collar occupations" and the observation of Geis (1974:284) that "white collar crimes can be committed by persons in all social classes." Federal prosecutors not only believe, but also act on, this assumption—pursuing, for example, large numbers of bank clerks for small-scale embezzlements and less educated citizens for relatively small-scale income tax violations (see Long, 1979). The other side of this situation is that college educated persons, also, of course, are prosecuted and convicted for common crimes. In fact, there is rather striking consistency in the finding that approximately 16% of the prosecutions as well as of convictions for common crimes, in District C as well as in other districts, involve college educated persons. Although the figures for income are smaller (10.2% and 7.1%), there is consistency here as well. This consistency is in contrast to the variation we find in the prosecution and conviction of white-collar persons involved in white-collar crimes.

We have also included in Table 1 mean sentence scores (see Table 2 for the scale used) for the various offender-offense groupings. Several things are apparent in these figures.

First, although there is a general tendency across districts for white-collar crimes to result in lighter sentences than common crimes, the disparities involved are greater in District C than in the other districts.

Second, in District C, whether education or income is the measure of social standing, white-collar persons prosecuted for white-collar crimes receive the most lenient sentences. (By contrast, in the other districts, the lightest mean sentences are received by less educated and lower income persons prosecuted for white-collar crimes.)

Third, with the exception of instances when income is considered in District C, the common crimes of common criminals result in the most severe sentences.

Fourth, the largest disparity in mean sentences reported in Table 1 is within District C, and it involves the common crimes of the less educated ($\overline{X} = 8.04$) and the white-collar crimes of the college educated ($\overline{X} = 3.49$).

Fifth, when comparisons are made between the same offender-offense groupings in District C and the remaining districts, the most consistent disparity observed across the districts is in the mean sentences received by college educated white-collar criminals ($\bar{X}$ = 3.49 and 4.64) and high income white-collar criminals ($\bar{X}$ = 3.55 and 4.98). A possible implication of these findings, consistent with the perspective outlined earlier in this paper, is that the proactive prosecution of white-collar crime in District C results in more lenient sentencing, particularly for white-collar persons. However, before inferences are drawn from these findings, it is necessary to consider a number of other relevant variables. We do this in the multivariate analyses presented in following sections of this article.

---

[7] District C is uniquely proactive in the sense that, by the measures applied, it is the *most* proactive district in our data. This said, we should also note that while the *volume* of white-collar cases prosecuted and convicted in District C is not dramatic, the reallocation of resources required to pursue even this number of cases is probably substantial. Our point is simply that white-collar cases require a disproportionate investment of resources and that, therefore, the doubling of the proportionate number of prosecutions and convictions (albeit still not large) in District C is probably a more dramatic difference than it might otherwise seem. This point is addressed again, in a somewhat different way, in the conclusion to this article.

Finally, although we might have expected more variation across districts in our data, we should note that the unique position of District C is not surprising. This is the District in our data set that is best known for its early emphasis on the prosecution of white-collar crime. This tradition was established most visibly by a U.S. Attorney who made the proactive pursuit of white-collar crime and political corruption a focal point of his work. The impression formed in our interviews, in the public statements of officials in the Department of Justice, and in recent highly publicized cases being pursued in the federal courts is that this is a style of administration that is going to become more prominent in the federal system. Thus, the following comparative analysis of sentencing patterns in District C and the other nine districts may be important to not only the debate on status characteristics and sentencing but to our understanding of the shape of things to come, as well.

METHODS AND MEASUREMENT

Table 2 presents the variables and their codings to be considered in this analysis. Variables are included on the basis of the perspective outlined above, prior research (e.g., Hagan, 1974; Chiricos and Waldo, 1975; Burke and Turk, 1975; Bernstein et al., 1977; Nagel, 1980; Swigert and Farrell, 1977), concerns for multicollinearity, and suggestions arising from our interviews. Thus, we began with a traditional list of variables that includes prior adult felony convictions, statutory seriousness of the offense, number of charges for which the defendant was convicted, ethnicity, sex, age, and employment status; then, we added to these consideration of whether or not the defendant was undergoing treatment for physical and/or mental illness. Our interviews suggested that the latter two variables might be particularly relevant for white-collar defendants. In addition, our sensitivity to the impact of prior processing decisions on sentencing suggested the inclusion of bail status (ordinally ranked on the basis of our interviews and on an evaluation of the fiscal and personal constraints involved), whether the defendant pleaded guilty, and a consideration of charge reductions. Since multiple defendants are common in white-collar cases, we also included a measure of their presence in our analysis. Several of the variables introduced thus far have particular significance for this study and are discussed further, here.

For example, statutory seriousness is an important variable to the perspective developed in this paper. This variable is measured in terms of the maximum prison sentence provided in the United States Code for the charge initially placed against the offender (charge reductions are considered below). Since white-collar crimes generally are assumed to carry lower statutory maximums then common crimes,[8] and because prosecutors can also use their discretion in selecting the statutory seriousness of the charge, the statutory seriousness variable may be expected to form a particularly important link in the prosecution and sentencing of white-collar persons for white-collar crimes.

From the perspective on white-collar crime outlined earlier, the offender's plea is also expected to be a very significant link between prosecution and sentencing. At minimum, a guilty plea in such a case saves the resources otherwise required to establish guilt in court. Since important white-collar cases are characteristically complex, this is often a considerable savings. Beyond this, many guilty pleas in-

---

[8] Consistent with this assumption, there is a correlation of −.14 in our data between statutory seriousness and those white-collar crimes involving college educated persons.

Table 2. Variables, Values, and Descriptive Statistics

Variables	Values	$\bar{X}$	s	N	Adjusted %
Prior Convictions	Actual Number of Adult Felony Convictions	.99	2.05		
Statutory Seriousness	Maximum Sentence Allowed by Statute	7.74	5.81		
Number of Charges	Actual Number	2.11	1.91		
Multiple Defendants	No = −1	−.47	.88	4821	(73.5)
	Yes = 1			1741	(26.5)
Ethnicity	Nonwhite = −1	−.03	.99	3379	(51.5)
	White = 1			3183	(48.5)
Sex	Male = −1	−.68	.73	5509	(84)
	Female = 1			1053	(16)
Employment	Unemployed= −1	.01	1.00	3238	(49.3)
	Employed = 1			3324	(50.7)
Physical Illness	Not Under Treatment = −1	−.62	.79	5307	(80.9)
	Under Treatment = 1			1255	(19.1)
Mental Illness	Not Under Psychiatric Treatment = −1	−.94	.35	6359	(96.9)
	Under Psychiatric Treatment = 1			203	(3.1)
Age	17 = 1	2.33	.72	13	(.2)
	18–37 = 2			4797	(73.1)
	38–46 = 3			984	(15.0)
	47–59 = 4			659	(10.0)
	60+ = 5			16	(.2)
Bail Status		4.29	3.00		
	Personal Recognizance = 1			1337	(20.5)
	Unsecured Bond = 2			844	(12.9)
	Unsecured Bond plus supervision or other conditions of bail = 3			1670	(25.6)
	10% Cash deposit = 4			290	(4.4)
	10% Cash deposit plus supervision or other condition of bail = 5			410	(6.3)
	Collateral = 6			17	(.3)
	Collateral plus supervision or other conditions of bail = 7			33	(.5)
	Surety Bond = 8			1099	(16.8)
	Surety Bond plus supervision or other condition of bail = 9			706	(10.8)
	Remand = 10			128	(2.0)
Plea	Pleaded Not Guilty = −1	.76	.66	802	(12.2)
	Pleaded Guilty = 1			5760	(87.8)
Charge Reduction	Petty Offense to Felony = −3	.50	1.21	15	(.2)
	Misdemeanor to Felony = −2			309	(4.7)
	Petty Offense to Misdemeanor = −1			0	0
	No Change = 0			4522	(69.0)
	Misdemeanor to Petty Offense = 1			189	(2.9)
	Felony to Misdemeanor = 2			798	(12.2)
	Felony to Petty Offense = 3			723	(11.0)
College Educated White-Collar Criminals	Dummy Variable with Less Educated Common Criminals as Reference Category	.048	.213	314	(4.8)
Less Educated White Collar Criminals	Dummy Variable with Less Educated Common Criminals as Reference Category	.081	.272	529	(8.1)
College Educated Common Criminals	Dummy Variable with Less Educated Common Criminals as Reference Category	.14	.348	924	(14.1)
Income	0–$13,776	1.099	.299	5911	(90.1)
	$13,777+			651	(9.9)

Table 2. Continued

Variables	Values	$\bar{X}$	s	N	Adjusted %
Sentence Severity	Suspended Sentence or Probation w/o Supervision = 0	6.90	6.24	158	2.4
	Fine and/or Restitution = 1			143	2.2
	Probation *or* Probation plus fine and/or Restitution 1–12 months = 2			518	7.9
	Probation *or* Probation plus fine and/or Restitution 13–36 months = 3			1678	25.6
	Incarcerated in Custody of Attorney General 1–6 months *or* Incarcerated in Custody of Attorney-General 1–6 months plus fine and/or Restitution *or* Probation or Probation plus fine and/or Restitution 37 months or more = 4			876	13.3
	Split Sentence or Split Sentence plus fine and/or Restitution (6 months or less) = 5			751	11.4
	Incarcerated in Custody of Attorney General 7–12 months *or* Incarcerated in Custody of Attorney General 7–12 months plus fine and/or Restitution = 6			205	3.1
	Mixed Sentence 6–12 months = 7			139	2.1
	Incarcerated in Custody of Attorney General 13–24 months *or* Incarcerated in Custody of Attorney General 13–24 months plus fine and/or Restitution = 8			437	6.7
	Mixed Sentence 13–24 months = 9			61	.9
	Incarcerated in Custody of Attorney General 25–36 months *or* Incarcerated in Custody of Attorney General 25–36 months plus fine and/or Restitution = 10			394	6.0
	Mixed Sentence 25–36 months = 11			37	.6
	Incarcerated in Custody of Attorney General 37–48 months *or* Incarcerated in Custody of Attorney General 37–48 months plus fine and/or Restitution = 12			194	3.0
	Mixed Sentence 37 months or more = 13			51	.8
	Incarcerated in Custody of Attorney General 49–60 months *or* Incarcerated in Custody of Attorney General 49–60 months plus fine and/or Restitution = 14			249	3.8
	Incarcerated in Custody of Attorney General 61–84 months *or* Incarcerated in Custody of Attorney General 61–84 months plus fine and/or Restitution = 17			265	4.0
	Incarcerated in Custody of Attorney General 85–120 months with or without fine and/or Restitution = 21			219	3.3
	Incarcerated in Custody of Attorney General 120 months or more *or* Incarcerated in Custody of Attorney General 120 months or more plus fine and/or Restitution = 30			187	2.8

volve the cooperation of the offender in building a case against others. Thus, in these types of white-collar cases, we expect the plea entered to have a notable effect on the sentence imposed—particularly in a district where the prosecution of a large number of these cases is made a priority. This coupling between prosecutorial and judicial subsystems may be an important source of the differential sentencing of white-collar offenders. In this data set, guilty pleas are coded 1, pleas of innocence −1.

Charge reductions may also influence sentence severity, as a way of rewarding cooperation in white-collar cases. Charge reductions are coded from −3 to 3 in terms of all six possible movements up and down in severity among petty offense, misdemeanor, and felony. No change is coded 0. However, it should be noted that the possibilities for charge reductions in the federal courts are not as great as in the state courts:

> The reason for this difference is that state criminal codes include lesser offenses to which charges frequently can be reduced, whereas the federal criminal code typically does not. . . . A result is more explicit recourse to discussions of sentencing and related sanctions. (Hagan and Bernstein, 1979:470)

The implication is that in the federal system the plea itself may be a more potent variable than the charge reduction.

The next three variables in Table 2 involve the offender and offense components of white-collar crime. The three variables considered derive from a dummy coding (see Kerlinger and Pedhazur, 1973) of our earlier cross-classification of the offender's education and offense. Each of these variables, when included in the regression analyses that follow, yield a coefficient that represents the difference in sentence for cases classified as the specified offender-offense grouping compared to those that are classified as a designated "reference category." For the purposes of our analyses, we have designated the common crimes of less educated persons as our reference category. This will allow us to estimate an effect parameter for each of our offender-offense groupings, compared to the reference category, with all other variables in the regression equation taken into account. As an example, we will be able to examine whether the disparity—observed earlier in the sentences received by college educated white-collar criminals, as compared to less educated common criminals—persists when the other independent variables just discussed are taken into account.

The last variable in Table 2, sentence severity, is our dependent variable. The coding of this variable derives from the efforts of Tiffany et al. (1975) to devise an approximate interval scale of the severity of different sentencing options. Minimal revisions in this scale derive from our interviews and are intended to reflect further gradations in the severity of sentences imposed.

Our analysis of the variables described above is based on conventional regression procedures. The strategy followed is to focus first on District C, in comparison to all other districts. Our purpose is to examine comparatively the consequences of the proactive prosecution of white-collar crime in District C. The perspective developed earlier suggests that the proactive prosecution of white-collar persons for their white-collar crimes requires concessions in sentencing, and it is this possibility that we first consider. We then go on to examine more specifically the factors leading to severe or lenient sentencing for each of the four offender-offense combinations in District C, as compared to the other nine districts. This second phase of the analysis explores which of the other independent variables may contribute to the expected differential sentencing of white-collar offenders in District C, as compared to the other districts. The perspective developed earlier suggests that concessions in the sentencing of these cases may follow from lenient initial charges, the rewarding of guilty pleas, and, possibly, charge reductions. These are the factors judged important in tightening the connection between the prosecutorial and judicial subsystems, thereby circumventing the obstacles posed to proactive prosecution in a loosely coupled system. Furthermore, if this tightening of connections between the prosecutorial and judicial subsystems does occur, we should also expect an increase in our ability to explain sentencing decisions in these types of cases—a consequence of a recognized common goal in a system otherwise characterized by divergent subsystem concerns. We also examine these expectations in the second phase of our analysis.

We should note that all comparisons undertaken in the remainder of this article effectively involve populations or subpopulations. That is, we are not dealing with samples, but, rather, with the full volume of cases involved in any given comparison (see footnote 4). Nonetheless, we report tests of significance in the tables that follow. These tests can be interpreted as checks on other kinds of random errors (for example, random measurement error). Our approach is to consider effects statistically significant at the .05 level and better, with betas larger than .10. Although there has been some debate as to whether standardized or unstandardized regression coefficients are more appropriate for the types of comparisons made in this paper, the more recent judgment is that both have their place (Hargens, 1976). Therefore, both coefficients are reported in our tables. However, there are some instances where unstandardized coefficients will be more suitable for our purposes. For example, the standardization of dummy variable coefficients can be misleading, particularly when the distributions of these variables are skewed (Cohen and Cohen, 1975:184). Therefore, where dummy variables are involved, unstandardized coefficients will be emphasized in the analysis that follows.

THE ANALYSIS

The results of the first part of our analysis are presented in Table 3. This table reports the results of regressing sentence severity on our independent variables separately in District C, the proactive district, and in the remaining districts. Our primary interest in these regressions is to see if—other variables held constant—the sentencing of white-collar persons for their white-collar crimes varies among these districts. The results presented in Table III indicate that this group is indeed sentenced differently in District C than in the remaining districts. A comparison of unstandardized regression coefficients reveals that while in the remaining districts college educated white-collar criminals do *not* receive significantly more lenient sentences than the reference category (less educated common criminals), in District C the former group *does* receive sentences that in comparison to the latter group are nearly three points lower (r = −.21; b = −2.72; B = −.16) on the sentence severity scale. However, what we did not anticipate is that less educated white-collar criminals experience a nearly identical leniency (r = −.18; b = −2.70; B = −.16) in District C that, again, is not paralleled in the remaining districts. College and less educated common criminals are not dramatically differentiated in the sentences they receive in either District C or the remaining districts. Overall, then, Table 3 provides provisional support for the perspective we have proposed: with a variety of other important variables held constant, our proactive distirct does grant lenient sentences to college educated

Table 3. Regression of Sentence Severity on Independent Variables in District C and Remaining Districts

	District C (N=694)				Remaining districts (N=5868)			
Independent Variables	r	b	B	S	r	b	B	S
Prior Convictions	.23	.45	.14	.000	.23	.36	.12	.000
Statutory Seriousness	.19	.04	.05	NS	.29	.25	.22	.000
Number of Charges	−.02	.11	.04	NS	.07	.23	.07	.000
Multiple Defendants	−.03	.25	.04	NS	.10	.47	.06	.000
Ethnicity	−.04	.30	.05	NS	−.08	−.42	−.07	.000
Sex	−.10	−.50	−.05	.100	−.17	−.82	−.10	.000
Employment	−.25	−.60	−.12	.001	−.15	−.50	−.08	.000
Physical Illness	.08	.34	.05	NS	−.02	−.07	−.01	.000
Mental Illness	.04	.55	.04	NS	−.01	−.15	−.01	NS
Age	−.02	.25	.04	NS	−.03	−.07	−.01	NS
Bail Status	.37	.68	.28	.000	.50	.80	.37	.000
Plea	−.20	−1.02	−.13	.000	−.14	−.68	−.07	.000
Charge Reduction	.21	.58	.12	.001	.10	.15	.03	.009
College Educated White-Collar Criminals	−.21	−2.72	−.16	.000	−.07	.34	.01	NS
Less Educated White-Collar Criminals	−.18	−2.70	−.16	.000	−.11	.46	.02	.10
College Educated Common Criminals	−.08	−.15	−.09	.009	−.02	−.12	−.01	.000
	R^2 =.298 Intercept=4.65				R^2=.326 Intercept=−.80			

Table 4. Regression of Sentence Severity on Independent Variables for Offender-Offense Combinations in District C

	Less Educated Common Criminals (N=443)			Less Educated White-Collar Criminals (N=84)			College Educated Common Criminals (N=86)			College Educated White-Collar Criminals (N=81)		
Independent Variables	b	B	s	b	B	s	b	B	s	b	B	s
Prior Convictions	.55	.15	.001	.21	.20	.086	.15	.07	NS	−.07	−.02	NS
Statutory Seriousness	.01	.01	NS	.07	.09	NS	.36	.39	.004	.48	.42	.003
Number of Charges	−.01	−.01	NS	.06	.10	NS	.25	.17	NS	−.05	−.06	NS
Multiple Defendants	.43	.07	NS	−.24	−.14	NS	.14	.03	NS	−.41	−.18	NS
Ethnicity	.50	.08	.07	−.29	−.18	NS	−.37	−.09	NS	.14	.04	NS
Sex	−.68	−.06	NS	−.03	−.02	NS	−.46	−.07	NS	−.42	−.08	NS
Employment	−.78	−.13	.003	−.48	−.24	.043	.33	.07	NS	−.06	−.02	NS
Physical Illness	.71	.09	.05	−.01	−.01	NS	−.48	−.08	NS	.16	.05	NS
Mental Illness	.22	.01	NS	.93	.27	.024	1.46	.14	NS	.36	.07	NS
Age	.19	.02	NS	.28	.17	NS	.15	.03	NS	−.01	−.01	NS
Bail Status	.96	.34	.000	.05	.09	NS	.53	.24	.07	.15	.13	NS
Plea	−.84	−.10	.016	−.26	−.07	NS	−.88	−.13	NS	−1.26	−.24	.028
Charge Reduction	.72	.13	.002	.11	.08	NS	−.42	−.09	NS	.48	.16	NS
		$R^2=.27$			$R^2=.25$			$R^2=.28$			$R^2=.39$	
		Intercept=3.88			Intercept=3.85			Intercept=2.30			Intercept=3.27	

white-collar criminals, while the remaining districts do not. However, this same pattern is apparent for less educated white-collar criminals. Thus, the issue that remains is whether these two offender-offense groupings actually differ in ways consistent with the perspective we have proposed. This issue is pursued in the next phase of our analysis.

Tables 4 and 5 present the results of regressing sentence severity on our independent variables within each of the four offender-offense groupings, first in District C, then in the remaining districts. We note first in these tables that, consistent with the perspective outlined earlier, the highest explained variance involves the sentencing of college educated white-collar offenders in District C ($R^2 = .39$). We take this as one indication of a tightening of connections in an otherwise loosely coupled system. Beyond this, however, we are anxious to determine *where* such a tightening in subsystem op-

Table 5. Regression of Sentence Severity on Independent Variables for Offender-Offense Combinations in Remaining Districts

	Less Educated Common Criminals (N=4352)			Less Educated White-Collar Criminals (N=445)			College Educated Common Criminals (N=838)			College Educated White-Collar Criminals (N=233)		
Independent Variables	b	B	s	b	B	s	b	B	s	b	B	s
Prior Convictions	.34	.11	.000	.29	.11	.013	.59	.18	.000	.39	.21	.001
Statutory Seriousness	.24	.22	.000	.04	−.03	NS	.29	.23	.000	.04	.02	NS
Number of Charges	.30	.08	.000	.14	.15	.001	.17	.05	.058	.15	.14	.017
Multiple Defendants	.51	.07	.000	.24	.06	NS	.42	.06	.027	.26	.07	NS
Ethnicity	−.47	−.07	.000	.05	.02	NS	−.44	−.07	.010	−.03	.01	NS
Sex	−.90	−.10	.000	−.32	−.12	.011	−.63	−.07	.017	−.48	−.13	.047
Employment	−.54	−.08	.000	−.06	−.02	NS	−.51	−.09	.003	−.18	−.05	NS
Physical Illness	−.11	−.02	NS	.05	.02	NS	.15	.02	NS	.11	.03	NS
Mental Illness	−.38	−.02	NS	2.01	.23	.000	−.01	−.01	NS	.11	.01	NS
Age	−.19	−.02	NS	.12	.05	NS	−.05	−.01	NS	.56	.17	.007
Bail Status	.83	.38	.000	.30	.25	.000	.75	.39	.000	.40	.32	.000
Plea	−.81	−.08	.000	.27	.06	NS	−.43	−.05	NS	.12	.02	NS
Charge Reduction	.14	.03	.041	−.07	−.02	NS	.10	.02	NS	−.27	.07	NS
		$R^2=.32$			$R^2=.21$			$R^2=.35$			$R^2=.29$	
		Intercept=.537			Intercept=4.80			Intercept=.564			Intercept=1.33	

erations may occur. Thus, our next interest is in comparing the effects of our independent variables on sentence severity, particularly in cases of college and less educated white-collar offenders. Comparison of the latter offender-offense groupings will help to determine whether the lenient sentencing observed above for both of these groupings in District C follows from similar or different types of considerations.

Within District C, and in relation to the remaining districts, such comparisons reveal very different patterns of effects. For example, we find in Table 4 that, consistent with our perspective, in District C statutory seriousness (the seriousness of the initial charge) has the biggest effect on the severity of the sentences received by college educated white-collar defendants (B= .42, p= .003). In contrast, the effects of this variable for the comparable cases in Table 5, and for the cases of less educated white-collar criminals in Table 4, are negligible (B = .02 and .09).

To extend our understanding of this rather striking difference, we generated the frequency distributions of statutory seriousness in both District C and the remaining districts. These distributions are presented in Table 6. What we find is that, although this variable has approximately the same standard deviation in District C (s = 5.90) and in the other districts (s = 5.79), the difference between these distributions is nonetheless dramatic. In District C, the modal initial charge carries a maximum sentence of one year in prison; in the remaining districts, the modal initial charge carries a maximum sentence of five years. Beyond this, both distributions tend to be bimodal: each has a concentration of charges in the one- and five-year categories. The difference is that District C has a bigger concentration of cases toward the nonserious end of the continuum, while the remaining districts have their heaviest concentration of cases more toward the serious end. The bimodal character of these distributions is consistent with our suggestion that, to successfully build the evidence necessary to get convictions in major white-collar cases, it is necessary to offer concessions in other potentially serious cases. What is most interesting from our perspective, however, is that this pattern is particularly pronounced in District C, where white-collar crime has been pursued in a very proactive fashion.

Table 6. Frequency Distributions of Statutory Seriousness in District C and the Remaining Districts

Maximum No. of Years	District C No.	District C %	Remaining Districts No.	Remaining Districts %
1	49	60.5	31	13.3
2	2	2.5	15	6.4
3	5	6.2	25	10.7
5	24	29.2	156	67.0
10	1	1.2	2	1.0
15	0	0.0	4	1.7
	$\bar{X}=6.74$		$\bar{X}=7.87$	
	s=5.90		s=5.79	

Also consistent with our perspective is the effect of the next most influential variable, the plea, in the sentencing of college educated white-collar criminals in District C. Those who plead guilty in District C (b = −1.27, p = .028) are more likely to get lenient sentences, while no similar significant effect (b= .12) exists in the remaining districts, or for less educated white-collar criminals in District C (b = −.26). From our perspective, the implication is that prosecutors in the proactive district are successful in getting judges to reward and punish the cooperativeness of college educated white-collar offenders. As expected, then, the two most important variables explaining the tendency in District C to sanction more leniently college educated white-collar criminals are their pleas and the initial charges placed against them. We will also discuss a quite different pattern that is apparent in cases of less educated white-collar offenders.

One other variable, charge reduction, is noteworthy for its *absence* of effect on the sentencing of white-collar cases in all districts. As indicated earlier in our discussion, this may be a result of the limited opportunities for charge reduction in the federal system. The effect of this situation may be to relegate much prosecutorial negotiation to less visible stages that precede initial charging decisions. We have already noted that, in District C, the statutory seriousness of the initial charge has its most pronounced effect for college educated white-collar criminals. In contrast, in the more reactive remaining districts statutory seriousness has a prominent effect in the cases of less educated common criminals. The volume of these cases and the influence of this variable may reflect the tendency of these more reactive districts to balance the needs and wishes of the enforcement agencies that focus on more traditional crimes.

We turn now to a consideration of a very different pattern of effects apparent in the sentencing of less educated white-collar criminals in both District C and the remaining districts. At this point it may be useful to recall that these cases consist in large part of income tax violations and bank clerk embezzlements. Since we did not anticipate the lenient treatment that these offenders receive in District C, our interpretation of these effects is, clearly, ad hoc. Nonetheless, we will begin by offering one suggestion as to why college and less educated white-collar crininals receive a similar type of leniency in District C. This suggestion is no more complicated than the observation that similar types of charges are placed against these offenders and that, once concessions are granted to the higher status white-collar offenders, it may be difficult not to grant them more generally, at least where similar statutory provisions are involved. In more familiar terms, a privilege once granted is difficult to deny later. Beyond this, it seems plausible to interpret the pattern of effects for this group in District C in terms of a compliance, as much as a punishment, model. Thus, the two most prominent findings for this group in District C are that being employed results in more lenient sentences (B = –.24), while being under psychiatric treatment results in more severe sentences (B = .27). The former finding applies only in District C, while the latter finding applies in the remaining districts as well. This second counter-intuitive finding apparently results from a provision in the federal code which allows offenders thought mentally ill to be institutionalized for a 90-day "psychiatric study," and, then, to be resentenced at the end of this period. Alternatively, the finding in District C of greater leniency for those offenders who remain employed probably reflects a willingness to let these offenders work out arrangements to make restitution in lieu of more severe punishments. The concern, at least in District C, would seem to be as much with remedying the behavior as with punishing it. In any case, we clearly have demonstrated that the differential leniency experienced by college and less educated white-collar offenders involves some very different considerations. We take this as support for our argument that the lenient sentencing of white-collar persons for their white-collar crimes derives from a set of circumstances that distinguish the proactive prosecution of this type of case.

A final finding, consistent across all districts, merits our attention. This finding is that bail status has a substantial impact on sentencing that is particularly consistent for common criminals (the B's range from .24 to .39). The implication of this finding is that negative bail decisions have particularly disadvantaging consequences for all kinds of common criminals. This finding is not new, but it is nonetheless interesting to find that it holds in the federal courts, bound by the Federal Bail Reform Act, as well as in the state courts (see Bernstein et al., 1977).

CONCLUSIONS

Gilbert Geis (1974) has made two important points about the study of white-collar crime. The first point is that relatively few original pieces of research have been published on white-collar crime during the past two decades (see also Wheeler, 1976). The second point is that further study of white-collar crime is important because it can tell us a great deal about the way power is exercised in our society. To these important points, we would add another: examination of the prosecution and sentencing of white-collar crime can tell us much about how the social organization of a particular type of crime can influence the way it is controlled. In turn, this type of understanding may do much to enlighten a long tradition of research on status characteristics and sentencing.

We noted in the beginning of our discussion that the above type of sentencing research has suffered from inadequate data. The inadequacy of these data is less the failing of social researchers than it is a failure of the system to prosecute and sentence any large volume of important white-collar cases. The infrequency of these prosecutions derives not only from the power of the persons involved but also from the manner in which these criminal activities are organized. Most *common crime* involves *victims or witnesses* and is pursued *reactively* in response to their complaints. In contrast, most *white-collar crime* involving *white-collar persons* is characterized by a diffuseness of victimization and an absence of unimplicated witnesses. As a result, a *proactive* organization of legal resources usually is required to seek out and build these white-collar cases. Since, frequently, only the participants in these criminal events can provide the information necessary to build successful cases, prosecutorial negotiation becomes a key part of the proactive prosecution of these cases. Furthermore, to make this negotiation work, a connection must be forged between the prosecutorial and judicial subsystems, such that the promises and concessions offered white-collar offenders are actually confirmed at sentencing. We have argued that this type of connection is the exception more than the rule in a criminal justice process we have called a loosely coupled system.

The overall implication of the organizational relationships we have described is, of course, that white-collar persons will receive lenient sentences for their white-collar crimes. However, it is important to note that this disparity might be expected only where the prosecution of white-collar cases is proactive enough to generate a large volume of cases. This is, of course, exactly what we found in our data: college educated white-collar criminals received more-lenient sentences only in District C, our most proactive district.

What we did not anticipate on the basis of the perspective developed in this paper is that less educated white-collar criminals would also receive lenient sentences in District C. A question that follows from this finding is whether the white-collar crimes of more-highly educated persons are actually handled in the unique way our perspective suggests. However, our subsequent analysis of cases falling within the separate offender-offense groupings confirmed that quite different factors were involved in the sentencing of each, and that college educated white-collar persons indeed *were* sentenced on the basis of several of the considerations emphasized by our perspective. In an ad hoc fashion, we speculated that one reason less educated white-collar criminals may also receive lenient sentences in District C is because they are charged under similar statutes: once these statutes are used in a lenient fashion, it may be difficult to deny their wider application. This interpretation is consistent with our earlier findings that at a bivariate level it is college educated white-collar criminals who actually receive the *most*-lenient sentences in District C, *and* that it is a higher concentration of this latter type of case in District C that makes it most unique. Nonetheless, we take these findings collectively as indicating the importance of further research on the ways in which types of persons, crimes, and control strategies interact.

More generally, our findings have theoretical—as well as policy—implications. They suggest, on the one hand, why previous research has rarely found relationships between status characteristics and sentencing to be large: as critics have suggested, there simply may not have been enough variation in the types of offenders and offenses studied to generate such relationships. This does not make the prior research wrong; it simply makes it representative of the types of courts studied. On the other hand, the modest relationship reported in this study, for District C ($r = .21$; $b = -2.72$; $B = -.16$), may be all the more striking given that—even in this proactive district—college educated white-collar criminals make up only 11.7% of the population sentenced (cf. Cohen and Cohen, 1975:178). The interesting possibility this point raises is that the observed relationship may grow as the volume of cases (or, in other words, the level of proactive prosecution) increases. Put differently, there may be an inverse relationship between the volume of white-collar convictions and the severity of white-collar sentences. At this point, concerns about deterrence and equal treatment may come into conflict: convictions may be enough to deter some white-collar crime, but they are not enough to establish a sense of parity in the treatment of white-collar and common crime.

These concerns are aggravated by the further findings that the lenient sentencing of college educated white-collar criminals in our proactive district apparently is associated with the less-serious charges placed against these offenders and with a rewarding of their guilty pleas. The sense is of a style of prosecution that gives preferential treatment to highly educated white-collar offenders. That this may be what is required to successfully prosecute important white-collar cases is an uncomfortable paradox that policy makers as well as theorists will do well to consider.

We should also note other less central, but no less significant, findings of this research. Among the less educated white-collar criminals in District C, we found that employment and mental illness were prominent factors in determining sentences: employment led to leniency, while treatment for mental illness resulted in severity. The latter finding was explained in terms of a federal provision that allows offenders thought mentally ill to be placed in penal institutions for psychiatric study; the former finding probably reflects a willingness to allow offenders in this grouping who are still employed to work out arrangements for making restitution. We suggested that in cases of less educated white-collar criminals the concern may be as much with obtaining compliance as with imposing punishment. Finally, quite apart from the preceding group of offenders, we also found that bail decisions have pronounced effects on the sentencing of common criminals. A theoretical implication of this finding is that bail outcomes may constitute one of the most salient kinds of legal labelling that common criminals experience.

The findings of this research do not encourage a reliance on law reform as an effective means of reducing sentence disparities. Notwithstanding important reforms in the federal bail system, we have found that bail decisions still exert a persistent impact on the severity of the sentences received by common criminals. Even more significantly, however, we suggest that the independence and variability of prosecutors demonstrated in this study is a neglected consideration which should reduce widespread expectations that recodification of the criminal law, sentencing guidelines, and reform or elimination of parole will eliminate sentencing disparities. Such reforms can readily be counteracted by shifts in prosecutorial policies on charging and plea bargaining and this seems particularly likely in an area as socially and legally sensitive as white-collar crime. The differential sentencing of white-collar offenders is a social as well as a legal problem.

REFERENCES

Bernstein, Ilene Nagel, William R. Kelly, and Patricia A. Doyle
1977 "Societal reaction to deviants: the case of criminal defendants." American Sociological Review 42:743–55.

Black, Donald
1973 "The mobilization of law." Journal of Legal Studies 2:125–49.

Blau, Peter, and Otis Dudley Duncan
1967 The American Occupational Structure. New York: Wiley.

Burke, Peter, and Austin Turk
1975 "Factors affecting postarrest dispositions: a model for analysis." Social Problems 22:313–32.

Chambliss, William J., and Robert B. Seidman
1971 Law, Order and Power. Reading, Mass.: Addison-Wesley.

Chiricos, Theodore G., and Gordon P. Waldo
1975 "Socioeconomic status and criminal sentencing: an empirical assessment of a conflict proposition." American Sociological Review 40:753–72.

Cohen, Jacob, and Patricia Cohen
1975 Applied Multiple Regression/Correlation Analysis for the Behavioral Sciences. New York: Wiley.

Eisenstein, James, and Herbert Jacob
1977 Felony Justice. Boston: Little, Brown.

Featherman, David, and Robert Hauser
1978 Opportunity and Change. New York: Academic Press.

Geis, Gilbert
1974 "Avocational crime." Pp. 273–98, in Daniel Glaser (ed.), Handbook of Criminology, Chicago: Rand McNally.

Geis, Gilbert, and Robert F. Meier
1977 White Collar Crime. New York: Free Press.

Gibbs, Jack P.
1978 "Deterrence, penal policy, and the sociology of law." Pp. 101–14, in Rita Simon (ed.), Research in Law and Sociology. Greenwich, Connecticut: JAI Press.

Greenberg, David F.
1977 "Socioeconomic status and criminal sentences: is there an association?" American Sociological Review 42:174–6.

Hagan, John
1974 "Extra-legal attributes and criminal sentencing: an assessment of a sociological viewpoint." Law and Society Review 8:357–83.

Hagan, John, and Ilene Nagel Bernstein
1979 "The sentence bargaining of upperworld and underworld crime in ten federal district courts." Law and Society Review 13:467–78.

Hagan, John, John Hewitt, and Duane Alwin
1979 "Ceremonial justice: crime and punishment in a loosely coupled system." Social Forces 58:506–27.

Hargens, Lowell
1976 "A note on standardized coefficients as structural parameters." Sociological Methods and Research 5:246–56.

Henderson, Thomas
1977 "The expanding role of federal prosecutors in combating state and local political corruption." Cumberland Law Review 8:385–401.

Hopkins, Andrew
1977 "Is there a class bias in criminal sanctioning?" American Sociological Review 42:176–7.

Katz, Jack
1979 "Legality and equality: plea bargaining in the prosecution of white collar and common crimes." Law and Society Review 13:431–60.

Kennedy, Mark C.
1970 "Beyond incrimination." Catalyst 5:1–27.

Kerlinger, Fred, and Elazor Pedhazur
1973 Multiple Regression in Behavioral Research. New York: Holt, Rinehart, and Winston.

Lizotte, Alan J.
1978 "Extra-legal factors in Chicago's criminal courts: testing the conflict model of criminal justice." Social Problems 25:564–80.

Long, Susan
1979 "The Internal Revenue Service: examining the exercise of discretion in tax enforcement." Paper presented at the Annual Meeting of the Law and Society Association, San Francisco.

Meyer, J. W., and B. Rowan
1977 "Institutionalized organizations: formal structures as myth and ceremony." American Journal of Sociology 83:340–63.

Nagel, Ilene
1980 "The behavior of formal law: a study of bail decisions." Unpublished manuscript, Indiana University, Bloomington.

Nettler, Gwynn
1979 "Criminal justice." Annual Review of Sociology 5:27–52.

Reasons, Charles E.
1977 "On methodology, theory and ideology." American Sociological Review 42:177–80.

Reiss, Albert J., Jr.
1971 The Police and the Public. New Haven: Yale University Press.
1974 "Discretionary justice." Pp. 679–99, in Daniel Glaser (ed.), Handbook of Criminology. Chicago: Rand McNally.

Swigert, Victoria Lynn, and Ronald A. Farrell
1977 "Normal homicides and the law." American Sociological Review 42:16–32.

Tiffany, Lawrence P., Yakov Avichai, and Geoffrey W. Peters
1975 "A statistical analysis of sentencing in federal courts: defendants convicted after trial, 1967–1968." Journal of Legal Studies 4:369–90.

Wheeler, Stanton
1976 "Trends and problems in the sociological study of crime." Social Problems 23:525–34.

TESTING A STRUCTURAL THEORY OF CORPORATE COOPTATION: INTERORGANIZATIONAL DIRECTORATE TIES AS A STRATEGY FOR AVOIDING MARKET CONSTRAINTS ON PROFITS*

RONALD S. BURT
University of California, Berkeley

KENNETH P. CHRISTMAN
University of California, Berkeley

HAROLD C. KILBURN, JR.
State University of New York, Albany

American Sociological Review 1980, Vol. 45 (October):821–841

Moving away from description of directorate ties as a cooptive device, we test a theory explicitly predicting cooptive uses of corporate directorates from the structure of the market in which firms operate. The theory is based on a network model of structural autonomy. It takes as exogenous information the sales and purchase transactions between establishments in sectors of the economy, locates those sectors most constraining pricing discretion within each sector, then predicts where establishments should be connected by interorganizational relations if such relations are intended to coopt market constraints. Using data on interorganizational relations as directorate ties (establishments connected through corporate boards by ownership, direct interlocking, and/or indirect financial interlocking) in the 1967 American economy, we find the theory's predictions to be accurate. Each of the three types of directorate ties tends to occur where there is market constraint and tends not to occur in the absence of constraint. Further, the three types of ties are coordinated as multiplex directorate ties. Where establishments in one sector constrain those in another, there is a strong tendency for all three types of directorate ties to exist between the two sectors. Where there is no such constraint, all three tend to be absent. Support is weaker for intrasector in comparison to intersector cooptation via directorate ties. Whatever the cooptive intent of the directorate ties described, they are patterned as if they were intended to coopt market constraints on corporate pricing discretion.

If two firms have interlocked directorates (one or more persons sitting on the board of directors for both firms) and such a connection allows either firm to coopt the ostensibly competitive market transactions the firms have with one another, then the presence of directorate ties on a large scale within an economy is evidence of an integrated economic elite. Wary of such connections in the United States, social scientists have devoted years of research to describing the cooptive potential of the corporate board. A summary empirical generalization from this research is the following: There is extensive corporate interlocking in the United States, particularly in urban corporate centers such as New York, Chicago or Los Angeles, and those firms most likely to be involved in it are large firms operating in capital intensive industries where the firm is controlled by diffuse interest groups (for comprehensive literature review, see Aldrich and Pfeffer, 1976; Aldrich, 1979). Measuring firm size as dollars of assets or annual sales, a ubiquitous finding is that a firm's size is positively correlated with the number of different corporations represented on the firm's board of directors (Warner and Unwalla, 1967; Dooley, 1969; Pfeffer, 1972; Allen, 1974). Also, there is a positive correlation between a firm's capital

---

* Direct all communications to: Ronald S. Burt; Department of Sociology; University of California; Berkeley, CA 94720.

Writing and analysis for this discussion were accomplished while the senior author was on a leave of absence spent in the Department of Sociology, SUNY at Albany. The excellent research facilities made available there by R. Farrell, R. Hall, and N. Lin were greatly appreciated. As part of the ongoing work under the Project in Structural Analysis at the Survey Research Center, UC at Berkeley, this research was supported by a grant from the National Science Foundation (SOC77-22938). N. Anderson and M. Lundgren provided able research assistance in coding data on directors of firms and corporate establishments. An anonymous reviewer for this journal greatly improved the discussion by pointing out ambiguities in the initial manuscript.

requirements and its tendency to represent other firms on its board of directors.[1] Among the many firms represented on a typical board of directors, moreover, there is usually a representative from a major bank in the same geographical region as the firm itself (Warner and Unwalla, 1967; Dooley, 1969; Levine, 1972; Pfeffer, 1972; Allen, 1974; 1978; Bearden et al., 1975; Mariolis, 1975; Sonquist and Koenig, 1975; and for replication in Great Britain and the Netherlands respectively, see Stanworth and Giddens, 1975; Mokken and Stokman, 1979). Thirdly, few other firms are represented on a board dominated by a single interest group. Two kinds of groups in particular have been researched, management and kinship. The thesis advanced by Berle and Means (1932), and supported by subsequent research (Larner, 1970; Allen, 1976), is that corporate "management" in the guise of the board of directors and senior corporate officers has come to power as a result of the diffusion of shares among so many owners that individual owners no longer exercise control over the corporate use of their capital. Contradicting this management control thesis is research demonstrating the continued existence of powerful families as kinship groups whose extensive stock holdings in large corporations perpetuate their ultimate corporate control (Burch, 1972; Zeitlin, 1974; 1976). Whatever the assumed basis for directors reaching decisions and maintaining their positions as directors, empirical research on the question of corporate control has documented a negative correlation between the tendency for other firms to be represented on a corporate directorate and the percentage of directors who are officers in the corporation (Dooley, 1969; Pfeffer, 1972) as well as the extent to which a single family owns a controlling share of the corporation's stock (Allen, 1976).

These findings can be interpreted as support for an argument that directorates are used to coopt market constraints. The larger a firm is, then the more impact its actions have on other organizations and, accordingly, the more the firm needs representatives who can integrate and legitimate the firm in its external environment (cf. Dooley, 1969:316; Pfeffer, 1972:223; Allen, 1974:395). Arguing for a cooptive interpretation of capital intensive firms representing financial firms on their directorates, Allen (1974:395) suggests that capital is a ". . . very generalized resource with a very dispersed demand from such economic organizations as corporations" (cf. Pfeffer, 1972:224). Thirdly, the negative correlation between concentration of corporate control and interlocking can be interpreted in terms of cooptation. The more that control of a firm is concentrated in the hands of a single group (management or family), then the greater is the loss of control incurred with the addition of a single outside group on the firm's board. Selznick's analysis of the TVA demonstrates that cooptation can backfire. When the TVA brought representatives of local organizations into its own decision-making structure, the TVA was forced into some actions more beneficial to the local organizations than to the TVA (see Selznick, 1949:113–4, 145–53, 205–13, 217, 259–61), although the long-term benefit to the TVA of avoiding local conflicts is not to be belittled. Where control of a corporation is already dispersed such that each interest group has little control over decisions, on the other hand, the addition of yet another voice on the board entails little loss of control for each group currently on the board. In order to maintain a decision-making capacity unimpaired by the competitive interests of their corporate environment, therefore, a dominant kinship or management group in a directorate would be expected to oppose extensive interlocking with other firms (cf. Dooley, 1969:322; Pfeffer, 1972:224). In Zald's (1969) terms, concentration of corporate control in a kinship or management group results in the firm's board serving an internal control function rather than an external representation function.

---

[1] This correlation is observed in a comparison of financial versus nonfinancial firms and Pfeffer (1972:224) reports a positive correlation between interlocking and a firm's capital needs as the ratio of dollars of debt over dollars of equity. Among nonfinancial firms, however, the correlation is negative. Allen (1974:400) reports a negative correlation between interlocking and a firm's capital needs measured as the ratio of dollars of assets over number of employees. Using the traditional capital output ratio to measure capital intensiveness (dollars of assets over dollars of sales, e.g., Lustgarten, 1975), Burt (1980b) also reports low negative correlations between interlocking and capital requirements. In addition, however, the analysis of multiplexity reveals a significant tendency for capital intensive (manufacturing) firms to have a complete multiplex directorate tie to finance; such firms own a finance firm as a subsidiary (Burt, 1980b:Table 8). This could explain why capital intensive firms need not interlock more than noncapital intensive firms; the former own their own establishment in the finance sector.

These cooptively interpreted findings have yet to directly address the connection between market structure and cooptive interorganizational structure. Discussing the cooptive uses of directorates makes intuitive sense. Indeed, interlocking directorates would not be considered a threat to competition between establishments unless such linkages were potentially cooptive. By itself, however, interpreting interlocking directorates as cooptive devices does not advance theoretical understanding of interlocking so much as it provides a sociological label for interlocking. Like instinct theories of action in which a person's behaviors are explained by attributing to the person an instinct for those behaviors, cooptation is a rationalization for action without a specification of parameters for action. In other words, there is no theory predicting when an interlocking directorate between two organizations is not cooptive. Accordingly, there is no method of rejecting an argument interpreting such connections as a cooptive device. What is known is that some types of firms use their directorates in what can be interpreted as a cooptive manner. What is not known is whether these ostensibly cooptive uses of directorates are patterned by market constraints so as to reduce competition between firms or whether there are merely some types of firms that engage in representing the corporate environment on their boards of directors.

Our purpose here is to use a network model of constraint to predict where directorate ties should, and should not, occur, if they are intended to coopt market constraints on profits. We demonstrate that three types of such ties are patterned by market constraint. They occur together in the presence of market constraint and tend not to occur in its absence. We conclude that, whatever substantive meaning is given to interlock ties, they are patterned as if they were being used cooptively.

THEORY

We employ a network model of "structural autonomy" discussed in detail elsewhere (Burt, 1980a; 1981:chap. 6). The model takes the patterns of relations among occupants of each status in a system as input and predicts the relative freedom the status offers within the system as well as the extent to which that freedom is constrained by role relations with each other status. In a sentence, the occupants of a status enjoy high structural autonomy within their system (i.e., have an ability to pursue their interests without constraint within the system) to the extent that their relational patterns ensure low competition with one another while simultaneously ensuring high competition among the nonoccupant actors with whom they interact. Their structural autonomy is severely constrained by their relations with occupants of a particular other status to the extent that they have relations with occupants of that status only and those occupants are perfectly centralized so as to collectively pursue their own interests. When this constraint occurs, occupants of the first status are expected to establish informal relations with occupants of the second status so as to coopt the sources of constraint on their autonomy.

A slight indulgence in algebra greatly clarifies these ideas. The basis for interstatus constraint is given by the following term expressing the constraint relations between occupants of statuses S_i and S_j pose for the occupants of status S_j:[2]

---

[2] The structural autonomy of occupying S_j, a_j, is then given as a function of these coefficients; $a_j = \beta_0 y_j + \beta_g X_j + \beta_x (y_j - \bar{y}) (\bar{X} - X_j)$, where $X_j = \Sigma_i x_{ji}$ for $j \neq i$, the positive effect of oligopoly on autonomy is given by β_0 and the constraining effect of relations with other status groups is given by β_x which is negative (cf. Burt, 1980a:Eq. 1; 1981a:Eq. 6.2b). This equation can be disaggregated to estimate the contribution relations between occupants of S_i and S_j make to the structural autonomy of S_j's occupants;

$$x_{ji} = [(z_{ji}/\Sigma_q z_{jq})^2 + (z_{ij}/\Sigma_q z_{qj})^2]y_i, \qquad (1)$$

where z_{ji} is the strength of the relation from S_j to S_i and y_i measures the extent to which occupants of S_i constitute an oligopoly in the sense that their decision making is centralized, enabling them to act as a single body to further their collective interests—at the potential expense of actors occupying S_j. If occupants of status S_j have relations only with occupants of S_i, then the bracketed term of the above equation equals 2. If they have no relations with the occupants of S_i, then the term equals 0 (specifically, $x_{ji} = z_{ji} = z_{ij} = 0$). In other words, x_{ji} is high to the extent that two things occur simultaneously: (1) occupants of S_j have relations only with occupants of S_i, and (2) the occupants of S_i are perfectly centralized (whereupon y_i equals its maximum value arbitrarily set at 1.0). Status S_j offers its occupants structural autonomy within their system to the extent that y_j is high and all x_{ji} are close to 0. A high value of x_{ji} indicates that status S_i poses a constraint for the autonomy of S_j's occupants. If some potentially cooptive type of relationship is being used within the system to eliminate these constraints, then a strong cooptive relation from status S_j to status S_i, call it w_{ji}, should be observed when x_{ji} is high, and should not be observed when x_{ji} is 0.

These simple ideas have been used to describe market constraints on corporate profits. An economy can be discussed as a network of interorganizational market transactions, sales to consumers and purchases from suppliers. The division of labor ensures considerable redundancy in such a network. Establishments producing similar types of commodities have similar relations from other establishments (i.e., will require similar proportions of commodities as inputs from suppliers) and to other establishments (i.e., will offer similar types of commodities as outputs to consumers). As commonly discussed in economics, these establishments producing similar goods fall into a single "sector" of the economy. The sales and purchase transactions can be aggregated into intersector relations since establishments within a sector have similar tendencies to purchase inputs from each sector as a supplier and have similar tendencies to sell outputs to each sector as a consumer. The resulting aggregate representation of interorganizational relations as intersector relations is discussed as an input-output table containing dollar flow coefficients, z_{ji} being the millions of dollars of sales by establishments in sector J to those in sector I (e.g., Leontief, 1966). In terms of the developments in network analysis upon which Burt (1980a) builds a theory of constraint, the fact that establishments producing similar goods have similar patterns of market relations with economic sectors means that the establishments are structurally equivalent within the economic market. The "sector" of an input-output table corresponds to the "status" as a jointly occupied network position in a density table (e.g., Burt, 1976; 1977; 1980c). Given data on the dollar flow coefficients among sectors of an economy and the extent to which establishments within a sector can act collectively as an oligopoly, therefore, the structural theory of constraint can predict the relative freedom enjoyed in each sector and the extent to which establishments in any one sector are constrained by those in each other sector.

Assuming that establishments would charge high prices (and accordingly make high profits) to the extent that they are free to do so, then the highest average profits should be observed in those sectors with the highest predicted freedom from other sectors. For American manufacturing industries, this is precisely what is observed and reported elsewhere (Burt, 1979a; 1980a). Values of Equation 1 were computed for each of 20 two-digit and each of 335 four-digit Standard Industrial Classification (SIC) manufacturing industries from z_{ji} as the millions of dollars of sales from establishments in industry J to those in sector I according to

---

$a_{ji} = [\beta_s x_{ji} + \beta_x(y_j - \bar{y})(\bar{x}_{j\cdot} - x_{ji})]|a_j|$, where $\bar{x}_{j\cdot}$ is the mean constraint imposed on status S_j (cf. Burt, 1980a:Eq. 2). It is these a_{ji} that capture market constraints on profits in manufacturing industries so as to distinguish significant and negligible market constraints (Burt, 1980a:Eq. 5). However, we have focused on x_{ji} in the text since it clearly shows the connection between constraint and market structure without requiring an extensive aside on the autonomy concept or industry profits.

the 1967 Input-Output Study of the American economy where y_i was measured as the concentration of 1967 sales in the largest four establishments in each sector corresponding to SIC industries. Following the lead of economic research on industry profits, adjusted price-cost margins were computed as the relative average profits establishments obtained in 1967 within each manufacturing industry (Collins and Preston, 1969). As one indicator of the portion of a dollar of sales that was profit in an industry, this margin was close to 1 to the extent that total sales by the industry minus direct material and labor costs equalled total sales itself. As expected, those industries with high price-cost margins tended to be concentrated (high values of y_J) and be subject to low constraint from other economic sectors (low values of x_{JI} summed across all manufacturing and nonmanufacturing sectors I).

Assuming that establishments would establish cooptive relations with sectors constraining their pricing discretion within the market, then the structural autonomy model provides a structural theory of corporate cooptation: Interestablishment cooptive relations should be observed where they can eliminate constraint and should not be observed where there is no constraint to eliminate. This theory was supported in an analysis of merger rates among establishments in the two-digit SIC manufacturing industries. Based on the analysis of market constraints confronting the 20 two-digit SIC industries from establishments in 51 manufacturing and nonmanufacturing sectors in 1967, three types of market constraints among establishments can be distinguished: intra-industry, industry constraint, and industry nonconstraint.[3] These types of market relations offer three hypotheses on corporate cooptation.

(H_1) Of the 20 industries, establishments in 16 could increase their freedom from other sectors if they could establish cooptive ties with one another so as to raise y_J for the industry. In these 16 industries, establishments should be connected by cooptive relations ($w_{JJ} \neq 0$).[4] Antitrust legislation poses a bias against support for this hypothesis depending on the type of cooptive relation being considered. Cooptive ties between competitive firms within an industry are less likely than would be predicted by market constraint since such ties are rather flagrant violations of antitrust. This issue is raised again in our conclusions section.

(H_2) There were 86 intersector relations in the 1967 economy where establishments in industry J had extensive transactions with those in sector I and sector I was highly concentrated so that x_{JI} in Equation 1 was high. These 86 industry constraint relations should be associated with cooptive relations intended to eliminate constraints, establishments in industry J creating cooptive relations with those in sector I ($w_{JI} >> 0$ given $x_{JI} >> 0$).

(H_3) The remaining market relations referred to a lack of constraint on establishments in each industry. Either the establishments in sector I were not highly concentrated (meaning that there were many independent competitors within the sector) or industry J had no market transactions with the sector. For either reason, x_{JI} in Equation 1 was close to zero so that 794 industry nonconstraint relations should be associated with a lack of cooptive relations intended to eliminate constraints, establishments in industry J not creating cooptive relations with those in sector I ($w_{JI} \simeq 0$ given $x_{JI} \simeq 0$).[5]

---

[3] In order to distinguish industry constraints from nonconstraints, a 95% confidence interval is placed around 0 constraint using the standard error of the mean constraint on profits in an industry (the mean a_{JI} for industry J from all 51 economic sectors I, see fn. 2). The sector I exercised high market constraint on industry J if x_{JI} was sufficiently different from 0 to make them one of the highest constraints the industry faced in the sense that their constraint was beyond the .95 confidence interval around 0 (see Burt, 1980a:Eq. 5).

[4] Deleted are the textile, apparel, rubber and fabricated metals industries. Establishments in these industries confront such a high level of market constraint that a small decrease in competition within each industry need not decrease the overall market constraint they face. Nevertheless, there are intra-industry connections here. There are mergers in all four industries and at least two establishments in all but the apparel industry are connected by interlocking, direct as well as indirect through financial institutions.

[5] There are 794 rather than 914 (20 × 51 − 20 − 86)

These hypotheses do not describe the manner in which corporate cooptation operates. They merely locate those places in the economy where cooptive relations should and should not occur if they are intended to eliminate market constraint. In order to know what types of connections among establishments are cooptive, connections falling under hypothesis H_3 can be compared with those falling under hypotheses H_1 and H_2. A discretionary connection, tending not to occur in the absence of market constraint (H_3) but tending to occur in the presence of market constraint (H_1, H_2), is probably being used as a cooptive relation eliminating market constraint. The data on mergers as of 1967 suggest mergers to be cooptive relations in this sense. Establishments within industries had a statistically significant tendency to merge with one another and establishments in industry J tended not to merge with those in sector I if that sector exercised no market constraint on the industry. Given the commonly held assumption that interlocking directorates are cooptive relations, we expect interestablishment connections through corporate boards of directors to show the same patterning.

MEASURING INTERESTABLISHMENT CONNECTIONS THROUGH BOARDS OF DIRECTORS

Measuring the w_{JI} as interestablishment connections through boards of directors requires decisions on three issues. The issues deserve brief discussion as explanation for the decisions made here in order to operationalize the w_{JI}.

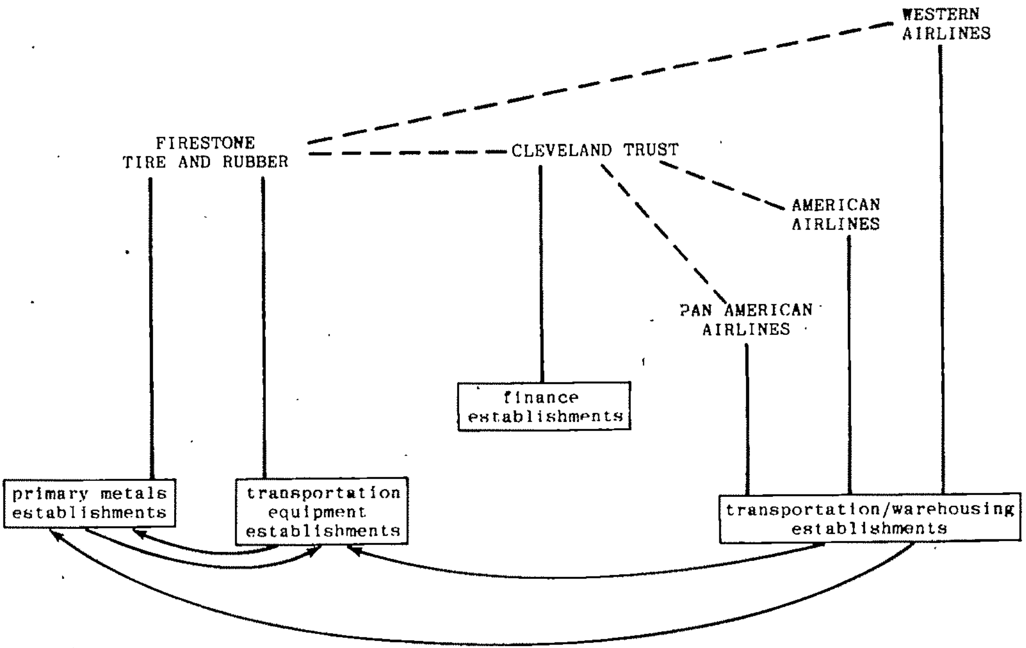
(1) Different organizational units are connected by market constraints and interlocking directorates. Market constraints are defined at the establishment level of analysis but boards of directors are defined at the corporation level of analysis. A firm is a socially defined entity created by corporate law in order to determine legal liabilities. An establishment is an economically defined entity created by the Bureau of the Census in order to describe market structure. For the purposes here, a single firm's holdings in an economic sector is one establishment. Following the Department of Commerce an industry in the economy consists of all establishments engaged in the manufacture of a single type of good. The tobacco industry consists of all establishments engaged in the manufacture of tobacco products, the food industry consists of all engaged in manufacturing food products, and so on. If a corporation owns two factories, one manufacturing bread and the other cigars, then it owns two establishments, one in the food industry and another in the tobacco industry. If a corporation owns three factories at separate locations, two engaged in the production of bread and one manufacturing cigars, then it owns two establishments, one in the food industry and another in the tobacco industry.[6] Some firms own an establishment in only one economic sector. In this circumstance market constraints among establishments and interlocking directorates among firms can be compared directly. The typical firm owns establishments in many sectors, however, so constraints and interlocking cannot be compared directly. Further, there is no obvious method of comparing constraint and interlocking by assigning firms to their most important sector or aggregating market constraints to the corporation level of analysis.[7] For this analysis, a cor-

---

nonconstraint relations because six nonmanufacturing sectors have been deleted as artificially supporting this hypothesis. The six sectors are: federal government, state/local government, gross imports, expense accounts, office supplies, and scrap. There are no SIC categories given in the 1967 Input-Output Study for these sectors, so there would have been no directorate ties to establishments in these sectors as such ties are operationalized here. Since the six sectors exercise no market constraint on any two-digit industry, considering them would have artificially raised the number of nonconstraint relations associated with a lack of interlock connections.

[6] This treatment differs slightly from that employed by the Department of Commerce. The census separates each factory within a sector as a separate establishment whether or not the factories are owned by a single corporation. Since establishment level data are not available for a national sample of firms and since the concern here is with coordination of establishments in separate sectors, two or more census establishments owned by a single firm in a single sector are treated here as a single establishment.

[7] Unfortunately, there are no reliable data available to the general public on the proportion of sales


WESTERN AIRLINES
FIRESTONE TIRE AND RUBBER
CLEVELAND TRUST
AMERICAN AIRLINES
PAN AMERICAN AIRLINES
finance establishments
primary metals establishments
transportation equipment establishments
transportation/warehousing establishments

Note: Arrows represent market constraint, solid lines represent ownership and dashed lines represent interlocking.

Figure 1. Illustrating Three Types of Directorate Ties Involving Two Firestone Tire and Rubber Establishments in 1967

porate board of directors is assumed to represent each establishment it owns no matter how small the establishment. The corporation is viewed less as the unit of cooptation than the means to cooptation. To be sure, when a representative from another firm sits on a corporate board, he represents the other firm. More important to an analysis of coopting market constraints, however, he represents the establishments owned by the other firm. It is access to those represented establishments that might allow the corporation's own establishments to coopt the market constraints they confront.

(2) Establishments are connected in multiple manners by corporate boards of directors. Three manners in which establishments can be connected through boards are illustrated in Figure 1 with selected connections involving Firestone Tire and Rubber in 1967.[8] Sectors of establishments are represented by squares and solid lines represent ownership (e.g., Firestone owned an establishment in the primary metals industry and another in the transportation equipment industry as well as several not presented in the diagram). A dashed line between two firms represents an interlocking directorate (e.g., one

---

and profits a firm obtains from its operations in each of multiple sectors. Even if such data were available, there is no obvious criterion after which a firm should be assigned to a single sector. Is a firm in industry J when one-third, one-half, or three-fourths of its income results from its operations in the industry? The task of assigning firms to specific sectors seems implausible at this time. Similarly, aggregating market constraints to the corporate level seems implausible. The typical firm operates in multiple economic sectors. Each establishment it owns is subject to the market constraints characterizing its industry. The firm itself is subject to some mixture of these market constraints collectively confronting its multiple establishments. Further complicating this mixture is the lack of data on the extent to which the firm's sales and profits result from each of its establishments. For the purposes here, it seemed preferable to oversimplify the situation by treating each of a firm's establishments as equally represented by the firm's board. This is probably not true empirically, but it has the advantage of a clear interpretation pending the development of some sophisticated method for quantitatively disaggregating firms into their component establishments.

[8] A detailed discussion of these three types of connections as cooptive relations is given elsewhere (Burt, 1980b). Also given there is a more complete picture of the establishments represented on Firestone's board of directors in 1967 by the three types of connections.

or more of Firestone's directors also sit on the board of Cleveland Trust). Finally, arrows represent market constraint relations (e.g., establishments in the primary metals industry are constrained by their market relations with those in the transportation equipment industry and those in the transportation/warehousing sector as well as some not presented in the diagram) and the absence of an arrow represents a lack of constraint (e.g., establishments in the financial sector exercise market constraint on no sector presented).

Put yourself in the position of the head executive for Firestone's establishment in the primary metals industry. Among other market constraints his establishment confronts are constraints from the transportation equipment industry and the transportation/warehousing sector. To the extent that Firestone's board of directors is a cooptive device, it should be used to give this executive access to decision making in those sectors constraining his establishment in the market. One form of access could be provided by direct ownership. The fact that Firestone owns an establishment in the transportation equipment industry means that the executive has access through Firestone's board to at least one establishment in that industry constraining him in the market. Two establishments in separate sectors can be connected by having a single firm's board own them both. A second form of access could be provided by direct interlocking. Two establishments, owned by separate firms, can be connected by a direct interlock between the two firms. In Figure 1, for example, Firestone is interlocked with Western Airlines. Since the latter owns an establishment in the transportation/warehousing sector, the executive in Firestone's primary metals establishment has a connection through Firestone's board and Western's board to at least one establishment in that sector constraining him in the market. Moreover, he has a connection indirectly via Cleveland Trust to two more establishments in that sector, one owned by American Airlines and another owned by Pan American Airlines. This illustrates a third type of interestablishment connection through the corporate board. Two establishments, owned by separate firms, can be connected by an indirect financial interlock, their respective firms both being interlocked with a bank or insurance company as a third party.[9]

In short, there is not a single type of connection between establishments through corporate boards. Multiple types of connections can occur separately. They can also occur concomitantly as multiplex connections. Ownership, direct interlocking, and indirect financial interlocking are distinguished here as very basic types of connections. However, others are possible such as indirect interlocking through a holding company or indirect ownership through a third firm owning a controlling proportion of stock in two other firms.

(3) A third problem consists of aggregating these interestablishment connections into a potentially cooptive intersector relation, w_{ji}. This problem has two parts: translating interestablishment connections into a level of potential intersector cooptation and estimating interestablishment connections involving American manufacturing industries.

There is no obvious answer to the first problem. When have the establishments in an industry coopted those in some sector as a function of the number of connections between establishments in the industry and sector? How many connections would be required for the industry to have coopted this sector? Does each establishment in the industry need to have a connection to each establishment in the sector (i.e., an industry to sector relational density of one) or is it sufficient for each to have a connection to any one establishment in the sector? Lacking guidance from theory or available research on corporate directorates, we have chosen to avoid this measurement issue by turning it around to make it more tractable. We focus on the absence of connections. We can very clearly measure the lack of a connection from each observed establishment in industry J and each observed establishment in sector I. In the absence of any connection, we know that establishments in the industry are ignoring that sector. According to the theory, this should not occur among establishments in an industry; it should not occur when the sector is a market constraint on the industry; but it should occur when the sector is not a market constraint on the industry. If there is a connection between any one establishment in the industry and any one establishment in the sector, then we shall assume the presence of some industry to sector connection without attempting to measure the level of potential cooptation it represents. This operationalization is clearly less than perfect. Potentially cooptive connections between sectors of the economy through corporate boards are reduced to binary code; w_{jl} equals present or absent. However, it corresponds to the measurement of market constraint and has the advantage of a reasonable interpretation, the absence of directorate ties clearly indicating the absence of cooptive intent if such connections are cooptive.[10]

---

[9] There have been arguments for viewing financial firms as intermediaries coordinating a group of firms (e.g., Patman, 1968; Zeitlin, 1974). This possibility is of special interest here since the finance sector exercises no market constraint on the manufacturing industries (as illustrated in Fig. 1), yet manufacturing establishments (as well as other types of establishments) are extensively interlocked with banks and insurance companies. One explanation for this contradiction to the view that interlocks are cooptive relations is the idea that finance firms are intermediaries coordinating the accounting and capital investment activities of multiple interdependent firms. Like the firms themselves, financial instituitions can be seen as the means to cooptation rather than the object of cooptation.

This still leaves the problem of estimating interestablishment connections involving American manufacturing. Beginning with the *Fortune* listing of 500 American manufacturing firms with the highest sales in 1967, the four largest firms in each of the 20 two-digit SIC manufacturing industries have been located using the SIC listings in *Poor's Directory of Corporations, Directors and Executives*. The resulting 42 firms represent large firms engaged in American manufacturing. Since a single firm can be the largest firm in multiple industries, there are 42 firms rather than 80 (four different firms in each two-digit industry). These 42 firms were not the largest manufacturing firms in the nation, although they are quite large. Liggett and Myers Tobacco is the smallest firm in the sample with its sales rank of 220 in the nation, yet it sold an impressive $384,801,000 worth of product in 1967. The sample represents American manufacturing rather than large firms, per se.[11] Members of corporate boards in all

---

[10] A more technical discussion predicts the number of connections from industry J to sector I using the level of market constraint the sector poses for the industry (Burt, 1980d). For all three types of connections through corporate boards, intraindustry connections increase exponentially with increasing levels of industry concentration and increase linearly with increases in the sector to industry constraint.

[11] Typically, firms are sampled in terms of firm size when interlocking directorates are analyzed. The pace-setting study by the National Resources Committee (Means, 1939) analyzes the 200 largest nonfinancial firms and the 50 largest financial corporations. Dooley (1969) and Allen (1974) analyze identical samples observed later (1964 and 1970 respectively). In a similar vein, Representative Patman's Subcommittee on Domestic Finance, Committee on Banking and Currency (Patman, 1968) analyzes the 500 largest industrial corporations and 200 largest nonindustrial firms in the 1966 *Fortune* listing together with 49 banks in urban centers, Mariolis (1975) analyzes the 500 largest manufacturing firms and the 247 largest nonmanufacturing firms in the 1969 *Fortune* listing, and Bearden et al. (1975) analyze the 1,131 largest firms drawn principally from the *Fortune* listings between 1963 and 1974. This sampling on a size criterion is justified using Berle and Means's (1932) finding that the top 200 nonbanking firms in 1929 controlled between 45% and 53% of the assets of all nonbanking firms. There is little need, therefore, to analyze firms beyond the largest firms if the analysis is concerned with the control of assets. Nevertheless, an innovative twist on this size criterion is given by Warner and Unwalla (1967). Defining a population of 20,989 firms listed in the 1961 *Fortune* compilation of the 750 largest American firms and the 1961 *Million Dollar Directory* from Dun and Bradstreet, a stratified random sample of 500 firms was drawn with the aid of *Moody's Industrial Manual*. The sample was stratified into eleven SIC categories and the number of sampled firms in each category approximated the proportion of total net worth of firms in the category (Warner and Unwalla, 1967:24, 124). Our concern here is less with describing interlocking among American firms than it is with testing predictions of interindustry interlocking. The sampling plan used here is similar to Warner and Unwalla's stratified random plan except that 20 two-digit SIC categories have been distinguished rather than 11 and the four largest firms in each category have been sampled rather than randomly selecting firms in each category. Since large firms are not equally distributed across manufacturing industries (large firms tending to occur in durable industries such as primary metals or fabricated metals, and in energy related industries

250 nonmanufacturing firms in the *Fortune* listing for 1967 were coded from *Poor's* and compared to the boards of the 42 manufacturing firms. Of the top 250, 110 interlocked with one or more of the 42 and have been included in the analysis. The sample of 42 firms representing manufacturing industries has been snowballed into a sample of 152 firms representative of large firms involved in American manufacturing in 1967.[12] Although small, this sample is representative of its population. An analysis of the directorates of these firms replicates past research findings discussed in our introductory remarks (see Burt, 1980b). There is extensive interlocking among the firms. Those firms most likely to be involved in interlocking are large firms controlled by diffuse interest groups. This same conclusion holds when firms or establishments are taken as the object of potentially cooptive interlocking.

Bringing the above discussion together, three binary estimates have been generated for w_{JI} as a tie from establishments in industry J to those in sector I through boards of directors:[13]

(O) There is a direct ownership tie from industry J to sector I if one or more of the 42 manufacturing firms owns an establishment in J and I (e.g., there is a tie between the primary metals and transportation equipment industries in Figure 1).[14]

(D) There is a direct interlock tie from industry J to sector I if one or more of the 42 manufacturing firms owns an establishment in industry J and interlocks with any one of the other 151 firms which in turn owns an establishment in sector I (e.g., there is a tie between the primary metals industry and finance sector in Figure 1).[15]

---

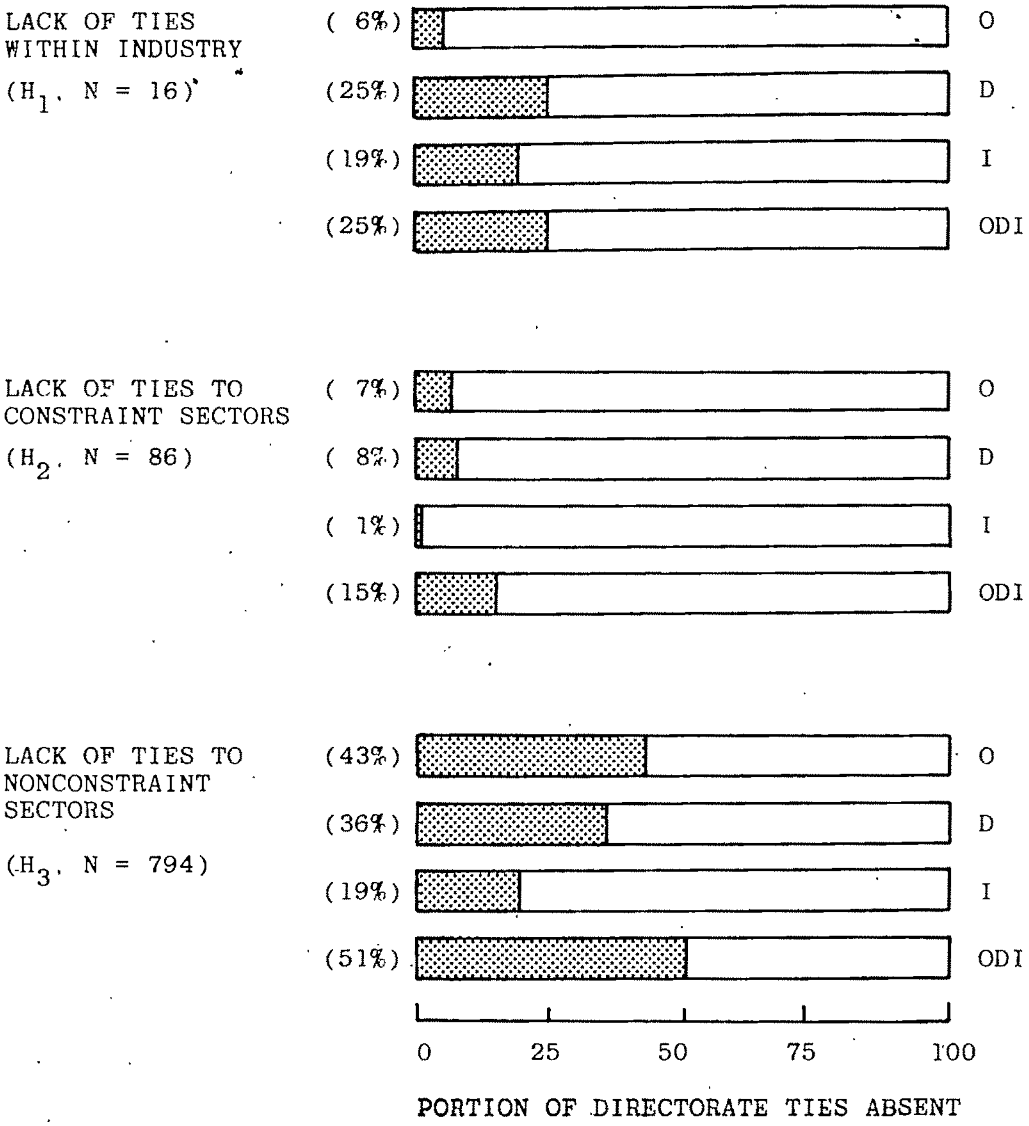
such as petroleum) an analysis of interlocking by large firms is weighted toward firms in specific industries. Given research demonstrating the different market constraints to which different industries are subject, and given interlocking as a potentially cooptive response to market constraints, firms have been sampled so as to represent all industries rather than just those with the largest firms. This sampling allows inferences to be made concerning large firms in manufacturing from an analysis of a relatively modest number of strategically selected firms.

[12] Of the 110 nonmanufacturing firms, 32 are banks, 22 are insurance firms, 21 are transportation firms, 20 are utilities (including 2 communication firms and 4 holding companies), and 15 are merchandising firms.

[13] More specifically, these directorate ties have been derived from two types of information coded for each firm from the 1968 *Poor's Directory of Corporations, Directors and Executives:* names of persons sitting on the boards of directors for each firm and the SIC categories in which each firm (including the firm's divisions and subsidiaries) owns an establishment. Numerical illustration of the manner in which these relations have been obtained is given in Burt (1979b). It may seem curious to code extensive data on all 152 firms and then only analyze the directorates of the 42 manufacturing firms. This has been done for reasons of inference. The 42 manufacturing firms represent manufacturing industries. The 110 nonmanufacturing firms have not been sampled to similarly represent nonmanufacturing sectors of the economy. Rather, they have been sampled so as to represent the extent to which the 42 manufacturing firms interlock with large corporations in nonmanufacturing sectors. Succinctly stated, the sample of 152 firms is not designed to allow a description of corporate directorates in the American economy. It is designed to make inferences about vertical integration in American manufacturing.

[14] Using the 1967 Input-Output Study's list of SIC categories corresponding to input-output sectors (*Survey of Current Business*, 54, February, pp. 34–7), a (152,45) binary matrix, D, was constructed where element (i,j) is 0 unless firm J is listed in *Poor's* as selling goods in sector I, whereupon the element was set equal to 1. There were only 45 of the 51 possible sectors considered since no SIC categories corresponded to 6 input-output sectors, so none of the firms could be listed as owning establishments in the sectors (federal government, state/local government, gross imports, expense accounts, office supplies, and scrap). D_m is then a (42,45) binary matrix consisting of the rows of D corresponding to the 42 manufacturing firms sampled. The number of manufacturing firms that own establishments in sectors I and J is then element (i,j) of the matrix: $D_m'D_m$. By definition this means that all establishments within an industry are connected since they are each owned by a corporation. In order to avoid this circularity, the intraindustry ownership relations are based on the Federal Trade Commission's *Report on Large Mergers in Manufacturing Industries and Mining*, 1948–1969. There is a direct ownership tie within industry J if there have been large mergers of establishments in the industry. These data are analyzed as corporate mergers by Burt (1980a).

[15] Using the list of directors in *Poor's*, a (152,152) binary, symmetric interlock matrix, B, was constructed in which elements b_{IJ} and b_{JI} equal 0 unless a director in firm J is also a director in firm I. The number of establishments in sector I that are accessible to manufacturing firms in industry J via direct interlocking with other firms is then given as element



Note: O refers to ownership, D refers to direct interlocking, I refers to indirect interlocking via financial firms, ODI refers to the absence of O and/or D and/or I; e.g., the 25% for ODI under hypothesis one means that establishments in four industries are not connected by ownership and/or direct interlock and/or indirect financial interlock.

Figure 2. Directorate Ties Tend Not to Occur in the Absence of Market Constraint

(I) There is an indirect financial interlock tie from industry J to sector I if one or more of the 42 manufacturing firms owns an establishment in industry J and interlocks with a bank or insurance company which in turn interlocks with any one of the other 151 firms which, in turn, owns an establishment in sector I (i.e., there is a tie between the primary metals industry and transportation/warehousing sector in Figure 1).[16]

(j,i) of the matrix: $D_m'(B_m - I_m)D$, where B_m is the (42,152) matrix consisting of the 42 rows of B corresponding to the 42 manufacturing firms sampled. I_m is a (42,152) matrix of 0's except that element (i,i) equals 1.

[16] The number of manufacturing firms in industry J that interlock via financial firms with any separate firms owning an establishment in sector I is element

RESULTS

Figure 2 presents bar graphs of the distribution of these three types of purportedly cooptive directorate ties (w_{JI}) across the known market constraints (the three hypotheses). Taking market constraint relations as an example (H_2, $x_{JI} >> 0$), there are four graphs of the proportion of the 86 constraint relations that are not countered by any interestablishment connections through corporate boards. In six cases (7%) there is an absence of ownership ties. In seven cases (8%) there is an absence of direct interlock ties. In one case (1%) there is an absence of indirect interlock ties through financial institutions. In thirteen cases (15%) one or more of the three types of directorate ties are absent simultaneously.

Overall, the data conform to the hypotheses. The absence of directorate ties is highest between establishments in an industry and those in a sector when the sector poses no market constraint on the industry (H_3). In comparison, there is a lower tendency for any one of the three types of directorate ties to be absent among establishments within an industry (H_1) and a very low tendency for any one of them to be absent between establishments in an industry and those in a sector constraining the industry (H_2).

There are some perturbations in the data, however, that make it premature to assume that the purportedly cooptive directorate ties are in fact cooptive as predicted by the theory. For one thing, there is a higher tendency for directorate ties to be absent among establishments within an industry than between establishments in the industry and those in a sector constraining the industry (H_1 versus H_2). This observation is complicated by differences in the extent to which directorate ties are absent between industries and nonconstraint sectors (H_3). Of the 794 nonconstraint relations on industry J from sector I, there are 453 instances (57%) in which establishments in J and I are connected by ownership ties versus 511 instances (64%) in which they are connected by direct interlock ties versus 647 instances (81%) in which they are connected by indirect interlock ties via financial institutions. In fact, there is an equal tendency for indirect interlock ties to be absent within an industry as well as between the industry and nonconstraint sectors (19%).

Fortunately, there is a mathematical model available for expressing the data so as to partition support for the hypotheses from differences in the extent to which each type of directorate tie is absent. For a given type of directorate tie (O, D, I or ODI), the frequency with which it is absent under hypothesis K (call this frequency f_k) can be expressed in terms of four parameters (cf. Goodman, 1970:228; 1972:1042):

$$f_k = \gamma\gamma^c\gamma_k^h\gamma_k^{ch}, \qquad (2)$$

where γ is a constant similar to the overall mean in an analysis of variance, γ^c and γ_k^h, respectively, describe the marginal tendencies for that type of tie to be absent and to fall under hypothesis K, while γ_k^{ch} describes the tendency for it to be absent under hypothesis K. A parameter is greater than 1 when it describes a condition that occurs more often than an average. If the directorate tie tends to be absent more often than it is present, for example, then γ^c will be greater than 1. If fewer relations fall under hypothesis one (H_1) than fall under all three hypotheses on the average, then γ_1^h will be less than 1. More importantly, if the directorate tie tends to be absent more often under hypothesis three (H_3) than it is across all three hypotheses, then γ_3^{ch} will be greater than 1.

The three hypotheses predict interaction terms in this model. Hypothesis one says that establishments within industries

---

(j,i) of the matrix: $D_m'(B_m^{**})D$, where B_m^{**} is a (42,152) matrix of indirect interlocking through financial institutions. Beginning with B from the previous note, all b_{IJ} are set equal to 0 if neither firm I nor firm J is a financial institution (a bank or an insurance firm). This new matrix with deleted b_{IJ}, call it B^*, is then squared (B^{*2}) so as to locate two-step chains between firms through banks and insurance firms. A (152,152) matrix, B^{**}, is obtained from the squared matrix such that b_{IJ}^{**} equals 0 unless element (i,j) of B^{*2} is greater than 0 and $J \neq I$, whereupon b_{IJ}^{**} is set equal to 1 since it represents an indirect connection between firms I and J via one or more financial firms. The submatrix B_m^{**} is then the (42,152) matrix consisting of the 42 rows of B^{**} corresponding to the 42 firms in the manufacturing sample.

Table 1. Predicting the Absence of Directorate Ties

	Cooptive Relations			
Parameters	0	D	I	ODI
γ	32.19	39.57	26.09	43.92
γ^c	.39	.50	.29	.63
γ_1^h	.12	.18	.24	.16
γ_2^h	.68	.59	.35	.70
γ_3^h	12.21	9:61	11.82	9.04
Interaction Terms (unit normal tests of significance in parentheses)				
γ_1^{ch}	.65	1.15	1.65	.92
	(1.2)	(.7)	(1.8)*	(.4)
γ_2^{ch}	.69	.59	.37	.67
	(1.6)*	(3.2)***	(2.8)**	(2.9)**
γ_3^{ch}	2.20	1.48	1.63	1.63
	(4.2)***	(3.3)***	(2.4)**	(4.4)***
$\chi^2(\gamma_1^{ch} = \gamma_2^{ch} = \gamma_3^{ch} = 1)$	61.23***	33.21***	25.60***	47.97***

* $p < .05$.
** $p < .01$.
*** $p < .001$.

will be connected by cooptive relations. If directorate ties are the cooptive relations they are purported to be, than γ_1^{ch} should be less than 1; there should be few industries in which directorate ties are absent. Hypothesis two (H_2) says that establishments in an industry will create cooptive relations with establishments in sectors constraining the industry. If directorate ties are cooptive, therefore, γ_2^{ch} also should be less than 1; there should be few industries from which directorate ties to constraint sectors are absent. In contrast, hypothesis three says that establishments in an industry will ignore nonconstraint sectors so γ_3^{ch} should be greater than 1 if directorate ties are cooptive; there should be many industries from which directorate ties to nonconstraint sectors are absent.

Using the data in Figure 2, Table 1 presents estimates of the gamma parameters for the different directorate ties.[17] Note that the frequency with which any type of tie is absent under any hypothesis can be perfectly reconstructed from the parameters in Table 1. The one case in which an industry is not connected by indirect interlocking (I) to a sector constraining profits in the industry, for example, consists of a mean effect ($\hat{\gamma} = 26.09$) multiplied by a tendency for indirect interlocking to be absent ($\hat{\gamma}^c = .29$) multiplied by a tendency for relations to fall under hypothesis two ($\hat{\gamma}_2^h = .35$) multiplied by a tendency for indirect interlocking to be absent under hypothesis two ($\hat{\gamma}_2^{ch} = .37$): $1 = 26.09(.29)(.35)(.37)$. In keeping with the obvious features of Figure 2, directorate ties consistently tend to be absent less often than they are present (all $\hat{\gamma}^c$ are less than 1) and relations more often fall under hypothesis three than the other hypotheses (all $\hat{\gamma}_3^h$ are much greater than 1 while all $\hat{\gamma}_1^h$ and $\hat{\gamma}_2^h$ are less than 1).

Also presented in Table 1 are statistical tests of the interaction parameters predicted under the three hypotheses.[18]

---

[17] Frequencies for each type of directorate tie can be computed directly from Figure 2. Let n_k refer to the number of relations falling under each hypothesis ($n_1 = 16$, $n_2 = 86$ and $n_3 = 794$). The f_k can then be computed from the percentages in Figure 2. For example, 6% of n_1 do not have ownership tie(s), so f_k for ownership is .06(16), or 1. The number of intra-industry relations for which ownership tie(s) are present is then $n_k - f_k$, or 15. As described by Goodman (1972:1046), the effects are computed as geometric means:

$$\gamma = (\Pi_k^3(n_k - f_k)f_k)^{1/6},\ \gamma^c = ((\Pi_k^3 f_k)^{1/3})/\gamma,$$
$$\gamma_k^{ch} = f_k/(\gamma^c \gamma_k^h \gamma),\ \gamma_k^h = ((f_k(n_k - f_k))^{1/2})/\gamma.$$

[18] Statistical interpretations of the gamma coefficients assume that each of the 896 w_{ij} is independently observed. As with much of network analysis, it is difficult here to determine the correct number of independent observations underlying second-order coefficients. The cooptive relations have been computed from 152 separate firms for which 2,903 different persons serve as directors and the firms together owned 414 of what we are treating here as establishments (a conservative estimate of what the Department of Commerce treats as establishments in con-

Completely eliminating interaction between market constraint and directorate ties (i.e., imposing the restriction that $1 = \gamma_1^{ch} = \gamma_2^{ch} = \gamma_3^{ch}$) reduces the ability of the model to recreate the observed data at well beyond the .001 level of confidence for each type of directorate tie. There is a strong interaction between market constraint and directorate ties. Looking at the individual parameters themselves, it is clear that this significant overall interaction is not a result of support for hypothesis one. Ownership ties (O) and multiplex directorate ties (ODI) have the predicted low tendency to be absent among establishments within industries; however, neither tendency is statistically significant.[19] Furthermore, the direct and indirect interlock ties (D and I) tend to be absent within industries, the indirect interlock ties significantly so ($\hat{\gamma}_1^{ch} = 1.65$, with a unit normal test of 1.8 indicating that the estimate is significantly greater than 1 at beyond the .05 level of confidence). The overall interaction between market constraint and directorate ties is a result of strong support for hypotheses two and three. As predicted by hypothesis two, each type of directorate tie has a low tendency to be absent between establishments in an industry and those in a sector when the sector constrains the industry (for O, D and I, $\hat{\gamma}_2^{ch}$ is significantly less than 1). As predicted by hypothesis three, each type of directorate tie tends to be absent when the sector does not constrain the industry ($\hat{\gamma}_3^{ch}$ is significantly greater than 1 for O, D and I). Beyond these tendencies for each type of directorate tie to be patterned by market constraints, Table 1 shows that the three occur together according to market constraints. If a sector constrains an industry, then all three types of directorate ties tend to exist between establishments in the industry and those in the sector ($\hat{\gamma}_2^{ch}$ for ODI is less than 1 at beyond the .01 level of confidence). In the absence of such constraint, one or more of the types of directorate ties tend to be absent ($\hat{\gamma}_3^{ch}$ for ODI is greater than 1 at well beyond the .001 level of confidence).

It is not surprising to find directorate ties coordinated as ODI multiplex ties if ties are intended to coopt sources of market constraint, multiplex relations typically being stronger than uniplex relations. But there is value in knowing the specific manner in which the three types of ties are coordinated since they constitute different levels of interorganizational control and so define different cooptive strategies when used in different combinations (see Burt, 1980b:Fig. 2, for detailed discussion). Of the relations that are not ODI multiplex in Figure 1, how many are null ties, how many represent individual ties (O, or D, or I), and how many represent coordinated pairs of directorate ties (OD, or OI, or DI)?

Table 2 presents the distribution of each observed combination of ties across the three hypotheses. Let f_{mk} be the frequency with which the m^th^ combination of directorate ties occurs under hypothesis K. For example: there are 142 cases in which establishments in an industry have no directorate ties to establishments in nonconstraint sectors ($f_{13} = 142$); there are 2 cases in which establishments within an industry are connected only by ownership ties ($f_{21} = 2$); there are 12 cases in which establishments within an industry are connected by all three types of directorate ties ($f_{81} = 12$); and so on. Just as the data in Figure 2 are expressed in terms of pa-

---

structing the input-output table, see fn. 5). Any one, or some mixture, of these numbers could be the correct number of independent observations. Further complicating the situation, the independence of observations is not equal across the three hypotheses since there is a more likely interdependence between w_{ii} and w_{jk} than between w_{jj} and w_{kk}, separate firms being more likely across two separate industries than within an industry. In the absence of a more appropriate treatment, we have merely assumed the w_{ii} to be independent and reported standard statistical interpretations of the gammas and their significance.

[19] Estimation of the gamma parameters can be improved (in terms of increased efficiency and decreased asymptotic bias) here by adjusting the observed f_k by a constant; Goodman (1970:229) suggests an increase of 1/2. So altering the observed f_k does not affect the conclusions based on Table 1. Slight modifications in the gammas and their error variances occur, but the modifications are minimal. The most serious change is an increase in the unit normal test of $\hat{\gamma}_2^{ch}$ for indirect interlocking (I) and multiplex directorate ties (ODI) from their current values of 2.8 and 2.9 to a value of 3.0. This would make both coefficients less than 1 at beyond the .001 level of confidence rather than the .01 level presented in Table 1.

Table 2. Multiplex Directorate Ties and Market Constraints

Combinations of Directorate Ties*	Market Constraint H_1	H_2	h_3	Total
null	1	1	142	144
O	2	0	2	4
D	0	0	3	3
I	0	0	70	70
OD	0	0	0	0
OI	1	7	68	76
DI	0	5	121	126
ODI	12	73	388	473
Total	16	86	794	896

* Null refers to the complete absence of any ties between establishments, O refers to one or more ownership ties, D refers to one or more direct interlock ties, and I refers to one or more indirect interlock ties via financial institutions. OD, OI, DI and ODI refer to the simultaneous presence of combinations of the three types of directorate ties.

rameters in Table 1, the data in Table 2 can be expressed in terms of the following parameters:[20]

$$f_{mk} \cong \gamma\gamma^{c}_{m}\gamma^{h}_{k}\gamma^{ch}_{mk}, \qquad (3)$$

where γ and γ^{h}_{k} respectively describe the overall mean and the tendency for relations to occur under hypothesis K. The tendency for the mth combination of directorate ties to occur is described by γ^{c}_{m} and its tendency to occur under hypothesis K is described by the interaction term γ^{ch}_{mk}. Table 3 presents estimates of the γ^{c}_{m} and γ^{ch}_{mk} for Table 2.[21] Recalling the discussion of Table 1, a coefficient greater than 1 indicates a condition that occurs more often than would be expected on the average.

Column one of Table 3 shows that the three types of directorate ties tend to occur together as multiplex directorate

---

[20] It might appear to be more straightforward to estimate the saturated model for the four-way table (hypothesis, O, D, I) directly here; however, such a model does not capture directorate tie multiplexity. The problem is that interactions in the model do not capture multiplexity. This is not obvious, so a quick justification is in order. Consider a three-way table cross-tabulating Ownership (no, yes), Direct interlock (no, yes), and Hypothesis (nonconstraint – H_3, constraint – H_1 and H_2). Ownership and direct interlock ties should occur as multiplex directorate ties in the presence of market constraint. The frequency with which the ith level of ownership and jth level of direct interlock occur under the kth level of hypothesis, f^{HOD}_{kij}, is given in terms of eight parameters:

$$f^{HOD}_{kij} = \gamma\gamma^{H}_{k}\gamma^{O}_{i}\gamma^{D}_{j}\gamma^{OD}_{ij}[\gamma^{HO}_{ki}\gamma^{HD}_{kj}\gamma^{HOD}_{kij}],$$

where the parameters in brackets capture the interaction of the two types of directorate ties with constraint. Note three points in this specification: (1) There is no parameter capturing the association between null ties (the simultaneous absence of O and D) and market constraint since this condition is absorbed into the other parameters. This means that there is no independent test for the hypothesis that directorate ties tend to be simultaneously absent in the absence of market constraint. (2) The parameter capturing an interaction between ownership and direct interlock, γ^{OD}, is not the tendency for OD multiplexity to occur—it is the tendency for ownership ties and direct interlock ties to occur together and/or to be simultaneously absent:

$$\gamma^{OD}_{11} = \gamma^{OD}_{22} = [\Pi^{2}_{k}(f_{k11}f_{k22}/f_{k21}f_{k12})]^{1/8},$$

which is the fourth power of the mean odds-ratio between O and D (e.g., Goodman, 1972:1042). In other words, if ownership ties and direct interlock ties tend to be simultaneously absent but only have a random tendency to occur together, this interaction parameter will be high. This is not what is meant by relational multiplexity (see Burt, 1980c:89–90, for review and references). A hermit has no relations with others and so at once has no social relation or economic relation to any particular other, but this does not mean that the hermit has coordinated his relations as multiplex ties to others. Rather, it means that he is an isolate. Multiplexity refers to the simultaneous presence of different types of relations from one actor to another. The simultaneous absence of relations is a wholly different concept. (3) The impropriety of γ^{OD} as a measure of multiplexity carries over to γ^{HOD} as the parameter capturing the interaction between constraint, ownership, and direct interlock. This third order interaction is the tendency for O and D to be simultaneously present or absent in the presence versus the absence of market constraint:

$$\gamma^{HOD}_{122} = \gamma^{HOD}_{111} = [(f_{111}f_{122}/f_{121}f_{112})/(f_{211}f_{222}/f_{221}f_{212})]^{1/8},$$

which is proportional to the odds-ratio between O and D in the absence of market constraint over the odds-ratio in the presence of constraint. Again, it can be high either because O and D tend to occur together in the absence of constraint or because they tend to be simultaneously absent in the absence of constraint.

[21] Since there are several empty cells in Table 2, parameters have been estimated from frequencies increased by 1/2. Deleting the nonexistent OD category, effects are computed in a manner only slightly more complicated than that used for Table 1. The overall mean effect is the geometric mean of frequencies in all 21 cells; $\hat{\gamma} = 5.27$. The tendency for relations to occur under hypothesis K is the geometric mean of frequencies under the hypothesis given the overall mean:

$$\gamma^{h}_{k} = [(\Pi^{7}_{m}f_{mk})^{1/7}]/\gamma,$$

so that $\hat{\gamma}^{h}_{1} = .26$, $\hat{\gamma}^{h}_{2} = .47$, and $\hat{\gamma}^{h}_{3} = 8.22$. The tendency for the mth combination of directorate ties to occur and its interaction with hypothesis K are then given as the following ratios of geometric means: $\gamma^{c}_{m} = [(\Pi^{3}_{k}f_{mk})^{1/3}]/\gamma$ and $\gamma^{ch}_{mk} = f_{mk}/(\gamma^{c}_{m}\gamma^{h}_{k}\gamma)$.

Table 3. Predicting the Multiplex Directorate Ties in Table 2

Combinations of Directorate Ties	Parameters			
	γ^{c}_{m}	γ^{ch}_{m1}	γ^{ch}_{m2}	γ^{ch}_{m3}
null	1.30	.85	.47	2.53
	(.7)	(.3)	(1.3)	(2.4)**
O	.28	6.60	.73	.21
	(2.5)**	(3.0)***	(.4)	(2.6)**
D	.18	2.02	1.11	.45
	(2.8)**	(.7)	(.1)	(1.2)
I	.49	.74	.41	3.30
	(1.2)	(.3)	(1.0)	(2.0)*
OD (no observations)	—	—	—	—
OI	1.74	.63	1.74	.91
	(1.8)*	(.8)	(1.3)	(.3)
DI	1.32	.28	1.69	2.13
	(.6)	(1.5)	(1.0)	(1.6)*
ODI	13.47	.68	2.21	.67
	(12.6)***	(1.1)	(2.6)**	(1.8)*

* $p < .05$.
** $p < .01$.
*** $p < .001$.

ties. Ownership ties (O), direct interlock ties (D), and indirect interlock ties via financial firms (I) tend not to occur independently ($\hat{\gamma}^{c}_{2}$, $\hat{\gamma}^{c}_{3}$ and $\hat{\gamma}^{c}_{4}$ are all less than 1). For ownership and direct interlock ties, this tendency is significant beyond the .01 level of confidence. Ties have various tendencies to occur together in pairs. While ownership and direct interlock ties (OD) never occur together, direct interlock ties occur with indirect interlock ties (DI) and ownership ties occur with indirect interlock ties (OI) more often than would be expected on average. However, only the OI multiplexity has a statistically significant tendency to occur ($p < .05$). Rather than occurring in pairs, the three types of cooptive ties tend to occur simultaneously. Far and away the strongest effect in Table 3 is the tendency for ODI multiplex directorate ties to occur (the unit normal test statistic of 12.6 is significant at well beyond the .001 level of confidence).

There is a significant overall interaction between market constraint and the different types of directorate ties. Imposing the restriction that all interaction parameters (the γ^{ch}_{mk}) be equal to 1 noticeably reduces our ability to predict the observed frequencies in Table 2. The likelihood-ratio chi-square of 70.73 with 12 degrees of freedom is significant at well beyond the .001 level of confidence.

Assessing each hypothesis individually, the second column of Table 3 offers meager support for hypothesis one. There is a significant tendency for establishments within an industry to be connected by uniplex ownership ties ($\hat{\gamma}^{ch}_{22}$ is 6.60 with a unit normal test indicating a significance at the .001 level of confidence). There are no other significant interaction effects for hypothesis one. Although establishments in 12 of the 16 industries are connected by all three types of directorate ties simultaneously (ODI multiplexity), this is not a statistically significant frequency since ODI multiplexity has such a high tendency to occur on the average across all three hypotheses.

In support of hypothesis two, all three types of directorate ties tend to occur simultaneously between establishments in an industry and those in a sector constraining the industry. Although having a high tendency to occur across all three hypotheses ($\hat{\gamma}^{c}_{8} = 13.47$), ODI multiplexity occurs under hypothesis two with an increased frequency significant at beyond the .01 level of confidence ($\hat{\gamma}^{ch}_{82}$ is 2.21 with a unit normal test of 2.6). There are no other significant interaction effects under hypothesis two; however, as a complement to the ODI multiplexity, it is unusual to find establishments in an industry unconnected by any of the three directorate ties to establishments in a sector constraining the industry ($\hat{\gamma}^{ch}_{12}$ is .47, but this is not significantly less than 1, even at the .05 level of confidence).

In support of hypothesis three, establishments in an industry tend to have none of the three directorate ties with establishments in a sector not constraining the industry. The tendency for null directorate ties (the simultaneous absence of ownership ties and direct interlock ties as well as indirect interlock ties via financial institutions) to occur in this absence of market constraint is significant at the .01 level of confidence ($\hat{\gamma}^{ch}_{13} = 2.54$). Also as would be expected under hypothesis three, there is a significant tendency for all three directorate ties not to be present simultaneously in the absence of market constraint. The interaction effect for ODI multiplexity and the absence of market constraint is less than 1 at beyond the .05 level of confidence ($\hat{\gamma}^{ch}_{83}$ is .67 with a unit normal test of 1.8).[22]

CONCLUSIONS

In conclusion, the network model of structural autonomy seems to be accurate in predicting where directorate ties occur in the American economy as cooptive relations, relations intended to eliminate market constraints. Each of the directorate ties considered (establishments connected through corporate boards by ownership, direct interlocking, and indirect financial interlocking) tends to occur where there is market constraint and tends to be absent in the absence of constraint. Moreover, the directorate ties occur simultaneously as multiplex directorate ties. If one or more of the establishments in an industry are connected by any one of the three types of directorate ties to establishments in some other economic sector, then it is likely that establishment(s) in the industry and sector are also connected by the other two types of directorate ties. This is particularly likely when the industry is constrained by its market transactions with establishments in the sector. In the absence of such market constraint, all three types of directorate ties tend to be simultaneously absent between establishments in the industry and sector.

These results suggest that the study of intraindustry cooptation through directorate ties should be treated as a special case apart from interindustry cooptation. Despite the strong support for intraindustry cooptation using merger data (Burt, 1980a), support using directorate ties is weak. There is a marked tendency for establishments within an industry not to be connected by interlock ties, direct or indirect. One plausible reason for this is federal regulation of the market. When establishments within an industry merge, or in some other manner create an ownership tie between them by coming under the jurisdiction of a single directorate, then they no longer exist as independent competitors within the industry. The creation of an interlock tie, in contrast, involves a connection between separate competitive firms operating in the industry, a connection easily visible to persons outside both firms and clearly a violation of antitrust legislation intended to prevent collusion between competitors (cf. Dooley, 1969:319). In short, cooptive relations that eliminate the corporate sovereignty of competing establishments within an industry are more likely to conform to hypotheses regarding intraindustry market constraint than are cooptive relations (such as interlock ties) which create institutionalized communication channels between separate corporations.

---

[22] Contrary to hypothesis three, however, there is a significant tendency for indirect interlock ties via financial institutions to occur in the absence of market constraint. Both uniplex indirect interlock ties (I) and multiplex direct/indirect interlock ties (DI) occur under hypothesis three with a frequency significant at the .05 level of confidence. Unfortunately, there is no method of estimating the extent to which these deviations from hypothesis three can be attributed to the lack of control firms have over selecting each establishment they reach by this type of directorate tie. Since interestablishment connections are measured here by treating each of a firm's establishments equally, one manufacturing establishment can be connected to a vast number of establishments through a single indirect interlock. For example, the two establishments American Airlines owned in the retail/wholesale and personal services sectors in 1967 are deleted from Figure 1. Neither of these sectors poses a market constraint for any of the industries in which Firestone owned an establishment. However, the indirect interlock that connected Firestone to American's establishment in the transportation/warehousing sector (a source of constraint on Firestone's establishments) brought with it unnecessary connections to the retail/wholesale and personal services sectors. Without better data on the links between establishments and their parent firms, direct and indirect interlock ties are likely to overstate interestablishment cooptive relations so that hypothesis three will be rejected spuriously. We have therefore focused on multiplex directorate ties in the text.

Returning to cooptation more generally, our results on directorate ties make even more apparent the need for an understanding of how, as opposed to why, cooptation can operate through directorate ties. We have focused on binary code, the absence versus presence, of directorate ties since we have no method of measuring the intensity of cooptation represented by an absolute number of such ties. How does the number and nature of directorate ties between establishments in two sectors translate into cooptation? There is no easy answer here since the importance of a potentially cooptive directorate tie is contingent on the individual constituting the tie. When an owner or senior officer in one firm sits on the board of another firm, the two firms could be considered to be more closely tied than would be the case if a consulting lawyer for both firms sat on their respective boards. The distribution of persons a firm uses to interface with its corporate environment has been studied. Indeed, "boundary" personnel are an important feature of Evan's (1965; 1972) discussion of the organization-set connecting a firm with its corporate environment (e.g., Hirsch, 1972, provides an application to some firms in the communication, broadcasting and amusement sectors). Questions on the roles of directors and officers as connections between establishments remain. For example, how are types of persons used to connect types of sectors?[23] Are directors randomly assigned to interlock with manufacturing firms versus financial firms versus other types of nonmanufacturing firms? If not, how does the systematic assignment of types of persons to types of directorate ties vary by the intensity of market constraint they are intended to coopt? If establishments in an industry are highly constrained by their market transactions with establishments in a specific sector, one might expect them to use their senior officers to represent them on the corporate boards of firms owning establishments in the problematic sector. This raises the issue of asymmetry in directorate ties. We have considered directorate ties only in their traditionally symmetric form. If firm J is interlocked with firm I, then firm I is interlocked with firm J. However, market constraint can be asymmetric. Establishments in industry J can be constrained by those in industry I without also constituting one of the market constraints faced by industry I (x_{JI} need not equal x_{IJ} in Equation 1). It is quite possible that our findings are the result of exploitation rather than cooptation. Given the multiplex directorate tie w_{JI} superimposed on a market constraint x_{JI}, have establishments in industry J attempted to coopt the constraint they face from sector I or have establishments in sector I exploited their market constraint on industry J? An answer to this question requires more detailed information on how the people providing the directorate tie function within the respective establishments.

Perhaps more important than an understanding of how cooptation might operate through directorate ties (although difficult to address without such an understanding) is a knowledge of the consequences of cooptively successful directorate ties. Systematic empirical research on corporate cooptation (that is to say, research generalizing beyond case studies of cooptive efforts by more than a handful of firms) has consisted of describing the extent to which potentially cooptive directorate ties occur among large firms. We reviewed examples in our introductory comments. Such description falls somewhat short of providing a theoretical understanding of the etiology or consequences of corporate cooptation.[24] Moving beyond a description of the distribution of directorate ties in the American economy, we have shown that such ties are patterned by market constraints and so appear to be cooptive relations. This is only half the battle. The immediate question raised by our results is whether or not such patterning is capable of eliminating market constraints. Merely having a multiplex directorate tie superimposed on a market constraint is not equivalent to eliminating the constraint. In fact, if every market constraint has superimposed on it a multiplex directorate tie coopting it, a whole new class of unanticipated constraints in a social market arises. In order to give establishments in one or more industries an unfair market advantage, directorate ties must be used with unequal effectiveness in coopting market constraints across industries. In the same manner that the existence of an oligopoly within an industry allows high profits in the industry, effective elimination of the market constraints faced by the industry allows high profits in the industry. But in a preliminary analysis of the effectiveness of merger ties, Burt (1980a) found establishments in each two-digit SIC industry to be connected to almost every sector constraining the industry. Our results extend these findings to the more general case of ODI multiplex directorate ties. In other words, strategic cooptation of market constraints could be a necessary precursor to corporate survival rather than an opportunity for increasing profits. If this is the case, then the original impetus for researching cooptive uses of directorates as a menace to the free market is lost. Rather, the widespread use of directorate ties to coopt market constraints could have created a dual market, an economic market consisting of sales and purchase transactions and a social market consisting of formal and informal interorganizational cooptive relations. We know directorate ties are patterned by market constraints. Needed here is research on the profits obtained by such patterning.

---

[23] Allen (1977) presents some results relevant to this question by describing some attributes of persons holding four or more positions as director in 250 large firms in 1935 versus 1970. These elite directors tended to be affiliated with financial institutions or manufacturing firms, tended not to come from the upper class (about 20% were upper class), and evidenced careers as managers rather than entrepreneurs or inheritors. Systematically comparing directors sitting on four or more directorates with those sitting on fewer, Useem (1979) shows that those directors holding multiple positions in 1969 were more likely to be involved than single position directors in the governance of nonprofit private organizations, in advising governmental agencies, and in participating in business policy associations. Still, the process by which directors might influence establishments and the "career" of being a successful director are only vaguely documented.

[24] This shortcoming is not invisible to the general public. In a recent compilation of top stockholders in each of 122 large American corporations (Metcalf, 1978), New York's Morgan Guaranty Trust was the number one stockholder in 27 corporations. A staff writer for *Time* (Vol. 111, no. 5, p. 50) seemed to agree with the bank's response: "Morgan Guaranty was quick to respond. Said a spokesman: 'Where are the examples of abuse?' Indeed, the study seemed to shed a lot more light on the extent of stock-power concentration than on its effects, good or ill."

We seem to be raising more questions than our results have answered, but anyone who has spent time reading through directorate memberships or spent time talking with directors themselves, knows that there is no single explanation for each director sitting where he does.[25] Some persons are directors because of connections to a firm's owners or because they participate in the day-to-day management of the firm. Others are directors because they are influential with other firms or because they have influence in a firm's community. Still others are directors because of the expertise they bring to the firm. The many reasons for holding a position as director make it difficult for social scientists to interpret, with any generality, potentially cooptive uses of the directorate. Each use seems to have its own ideographic explanation. Such difficulty notwithstanding, it seems clear, judging from the evidence that we have seen, that there is a pattern to directorate ties, a pattern predictable from the network of market constraints in which directorates operate.

REFERENCES

Aldrich, H. E.
1979 Organizations and Environments. Englewood Cliffs: Prentice-Hall.

---

[25] Domhoff (1967:51–2) distinguishes nine types of directors, excluding those recruited from the upper class, that he observed in a sample of 50 large American corporations in 1963; senior corporate officers, technical experts relevant to the firm's operations, college presidents, former military men, corporate lawyers, foundation executives, local businessmen, members of a "Jewish upper class," and foreign nationals.

Aldrich, H. E. and J. Pfeffer
1976 "Environments of organizations." Annual Review of Sociology 2:79–105.

Allen, M. P.
1974 "The structure of interorganizational elite cooptation: interlocking corporate directorates." American Sociological Review 39:393–406.
1976 "Management control in the large corporation: comment on Zeitlin." American Journal of Sociology 81:885–94.
1977 "Continuity and change within the core corporate elite." Paper presented at the annual meeting of the American Sociological Association, August.
1978 "Economic interest groups and the corporate elite." Social Science Quarterly 58:597–615.

Bearden, J., W. Atwood, P. Freitage, C. Hendricks, B. Mintz and M. Schwartz
1975 "The nature and extent of bank centrality in corporate networks." Paper presented at the annual meeting of the American Sociological Association, August.

Berle, A. A. and G. C. Means
1932 The Modern Corporation and Private Property. New York: Harcourt, World and Brace.

Burch, P. H.
1972 The Managerial Revolution Reassessed. Lexington: D. C. Heath.

Burt, R. S.
1976 "Positions in networks." Social Forces 55:93–122.
1977 "Positions in multiple network systems, part one: a general conception of stratification and prestige in a system of actors cast as a social topology." Social Forces 56:106–31.
1979a "Disaggregating the effect on profits in manufacturing industries of having imperfectly competitive consumers and suppliers." Social Science Research 8:120–43.
1979b "A structural theory of interlocking corporate directorates." Social Networks 1:415–35.
1980a "Autonomy in a social topology." American Journal of Sociology 85:892–925.
1980b "Cooptive corporate actor networks: a reconsideration of interlocking directorates involving American manufacturing." Administrative Science Quarterly 25:In Press.
1980c "Models of network structure." Annual Review of Sociology 6:79–141.
1980d "On the functional form of corporate cooptation: empirical findings linking the intensity of market constraint with the frequency of directorate ties." Social Science Research 9: In Press.
1981 Toward a Structural Theory of Action: Network Models of Stratification, Perception, Action and Equilibrium within a System of Actors. New York: Academic.

Collins, N. R. and L. E. Preston
1969 "Price-cost margins and industry structure." Review of Economics and Statistics 51:271–86.

Domhoff, G. W.
1967 Who Rules America? Englewood Cliffs: Prentice-Hall.

Dooley, P. C.
1969 "The interlocking directorate." American Economic Review 59:314–23.

Evan, W. M.
1965 "Toward a theory of inter-organizational relations." Management Science 11:217–30.
1972 "An organization-set model of interorganizational relations." Pp. 181–200 in M. F. Tuite, M. Radner and R. K. Chisholm (eds.), Interorganizational Decision-Making. Chicago: Aldine.

Goodman, L. A.
1970 "The multivariate analysis of qualitative data: interactions among multiple classifications." Journal of the American Statistical Association 65:226–56.
1972 "A general model for the analysis of surveys." American Journal of Sociology 77:1035–86.

Hirsch, P. M.
1972 "Processing fads and fashions: an organization-set analysis of culture industry systems." American Journal of Sociology 77:639–59.

Larner, R. J.
1970 Management Control and the Large Corporation. Cambridge: Dunellen.

Leontief, W.
1966 Input-Output Economics. New York: Oxford University Press.

Levine, J.
1972 "The sphere of influence." American Sociological Review 37:14–27.

Lustgarten, S. H.
1975 "The impact of buyer concentration in manufacturing industries." Review of Economics and Statistics 57:125–32.

Mariolis, P.
1975 "Interlocking directorates and control of corporations: the theory of bank control." Social Science Quarterly 56:425–39.

Means, G. C. (chairman)
1939 The Structure of the American Economy: Basic Characteristics. Washington, D. C.: U.S. Government Printing Office.

Metcalf, L. (chairman)
1978 Voting Rights in Major Corporations. Washington, D. C.: U.S. Government Printing Office.

Mokken, R. J. and F. N. Stokman
1979 "Corporate-governmental networks in the Netherlands." Social Networks 1:333–58.

Patman, W. (chairman)
1968 Commercial Banks and Their Trust Activities: Emerging Influences on the American Economy. Washington, D. C.: U.S. Government Printing Office.

Pfeffer, J.
1972 "Size and composition of corporate boards of directors." Administrative Science Quarterly 17:218–28.

Selznick, P.
1949 TVA and the Grass Roots. New York: Harper and Row.

Sonquist, J. A. and T. Koenig
1975 "Interlocking directorates in the top U. S. corporations: a graph theory approach." Insurgent Sociologist 5:196–229.

Stanworth, P. and A. Giddens
1975 "The modern corporate economy: interlocking directorships in Britain 1960–1970." Sociological Review 23:5–28.

Useem, M.
1979 "The social organization of the American business elite and participation of corporation directors in the governance of American institutions." American Sociological Review 44:553–72.

Warner, W. L. and D. B. Unwalla
1967 "The system of interlocking directorates." Pp. 121–257 in W. L. Warner, D. B. Unwalla, and J. H. Trimm (eds.), The Emergent American Society: Large Scale Organizations. New Haven: Yale University Press.

Zald, M. N.
1969 "The power and function of boards of directors: a theoretical synthesis." American Journal of Sociology 75:97–111.

Zeitlin, M.
1974 "Corporate ownership and control: the large corporation and the capitalist class." American Journal of Sociology 79:1073–119.
1976 "On class theory of the large corporation: response to Allen." American Journal of Sociology 81:894–903.

MANUSCRIPTS FOR THE
ASA ROSE SOCIOLOGY SERIES

Manuscripts (100 to 300 typed pages) are solicited for publication in the *ASA Arnold and Caroline Rose Monograph Series*. The Series welcomes a variety of types of sociological work—qualitative or quantitative empirical studies, and theoretical or methodological treatises. An author should submit three copies of a manuscript for consideration to the Series Editor, Professor Suzanne Keller, Department of Sociology, Princeton University, Princeton, NJ 08540.

THE EFFECT OF SEXUAL STRATIFICATION BY RACE ON OFFICIAL REACTIONS TO RAPE*

GARY D. LAFREE
University of New Mexico

American Sociological Review 1980, Vol. 45 (October):842–854

Collins argues that sexual access, like other valuable commodities, is distributed according to relative power within a sexual stratification system. He predicts more serious official reactions to violations of the sexual stratification system in which men from less powerful groups sexually assault women from more powerful groups. Race continues to be an obvious correlate of stratified sexual access in America. But empirical studies have not assessed the cumulative effect of racial composition of victim-suspect dyads on processing outcomes in sexual assault cases. The present study examined the effect of race composition on processing decisions—from case report to final disposition—for 881 sexual assaults in a large, midwestern city. Step-wise multiple regression analysis showed that, compared to other defendants, black men who assaulted white women received (1) more serious charges and (2) longer sentences, and were more likely to (3) have their cases filed as felonies, (4) receive executed sentences, and (5) be incarcerated in the state penitentiary. At the same time, black men who assaulted white women were no more likely than other suspects to be arrested or found guilty. The implications of the results for a sexual stratification theory of official reactions to sexual assault are discussed.

A central question in criminology since Sellin (1928) has been the extent to which the defendant's race affects processing decisions in criminal cases. Given the long history of black-white sexual segregation in the United States (Rowan, 1952; Woodward, 1955), sociologists have been especially concerned with assessing differential processing in sexual assault cases (e.g., Johnson, 1957; Partington, 1965; Wolfgang and Riedel, 1973). Recent research in victimology (e.g., Sebba and Cahan, 1975; Nelson and Amir, 1975) and the recognition of rape as a serious social problem (Battelle, 1975; Rose, 1977) have focused research attention on the effect of the victim's race and the racial composition of the victim-defendant dyad on official reactions to rape.

Contemporary conflict theorists (e.g., Turk, 1969; Quinney, 1970; Chambliss and Seidman, 1971; Collins, 1975) provide an explanation for racial discrimination in the application of the criminal law. They argue that political power, in close association with social class position, is the basic determinant of organizational rewards and constraints. Access to scarce resources is determined, in part, by membership in societal subgroups, defined especially by class and race. Conflict theorists argue that the criminal law is a tool created and enforced by powerful subgroups to maintain their own favorable access to organizational rewards and to limit access of less powerful subgroups to these rewards. Thus, Chambliss (1969:856) and Chambliss and Seidman (1971) argue that the "administration of the criminal law is a highly selective process that involves the use of a wide range of discretion" that results in "systematic bias in law enforcement."

Since Marx (in Mills, 1948), conflict theorists have emphasized differential access to economic rewards. But the conflict argument can be applied to other resources that are both valued and scarce. Davis (1949) and Collins (1975:228–34) argue that sexual access is such a resource. Like other scarce resources, sexual access is determined by power relationships within a highly stratified sexual market. The basic characteristic of this stratification system is the institution of sexual property, "the relatively permanent

---

* Address all communication to: Gary D. LaFree; Department of Sociology; University of New Mexico; Albuquerque, NM 87131.

Part of this research was completed while the author was supported by grant number R01 MH29727-01A1 National Center for the Prevention and Control of Rape, National Institute of Mental Health. I wish to thank Phil May, Art St. George, several anonymous reviewers, and, especially, Barbara Reskin for their helpful comments on earlier drafts of this paper.

claim to exclusive sexual rights over a particular person" (Collins, 1975:234). "Sexual-property" rules are the basic structure of kinship systems (Levi-Strauss, 1949); they determine who can mate with whom and under what circumstances. Definition of the concept is parallel to that of "economic property": property is not lands or buildings themselves but, rather, the social relationships that guarantee access to them (see Collins, 1975:234). Rules of sexual access, like class relationships, are not immutable. They are subject to constant challenge and must be maintained through bargaining, threats, alliances, and force. Collins (1975:282) expresses the relationship between power and reactions to sexual-property violations as a general proposition:

> The greater the power of dominant individuals to appropriate others as sexual property, the stronger the taboo and the greater the outrage at violations of these property rights.

In its extreme form during the period of American slavery, the sexual stratification system placed few constraints on sexual access to black women by white men. By contrast, sexual access to white women by black men was carefully constrained and sanctions for the violation of these constraints were harsh (Myrdal, 1944; Woodward, 1955; Wolfgang and Cohen, 1970).

To the extent that relationships between men and women in America are still defined by race-specific rules of sexual access, an implicit ordering of official reactions to sexual assault by race of the victim and offender is suggested. Assuming that blacks are the less powerful subgroup, (1) the assault of white women by black men should result in the most serious sanctions, followed by (2) the assault of white women by white men, (3) black women by black men and, finally, (4) black women by white men. The research that follows empirically examines this proposition for a set of forcible-rape cases.

PRIOR RESEARCH

A review of prior research indicates major disagreement about the effect of race on official reactions to rape. Research completed before 1970 (e.g., Lemert and Roseberg, 1948; Johnson, 1957; Bullock, 1961; Partington, 1965) generally concluded that black defendants received more-serious official sanctions than white defendants.[1] But in a comprehensive review, Hagan (1974) notes important problems with this research. Several studies (e.g., Johnson, 1957; Bullock, 1961) *rely* on tests of significance to interpret statistical results. Hagan, however, claims that significance tests should be interpreted cautiously because they do not indicate the *strength* of relationships and are dependent on sample size (a relevant criticism because many sentencing studies include large samples). Other studies (e.g., Bullock, 1961; Partington, 1965) do not control for theoretically important variables, such as defendant's criminal record. Hagan also notes that, with few exceptions (Judson et al., 1969), the effect of defendant's race on sentencing is examined only for southern states.

By contrast, recent research (e.g., Chiricos and Waldo, 1975; Bernstein et al., 1977; Hindelang, 1978) reports little racial discrimination against defendants in criminal processing. Hindelang compares black-white arrest rates from Uniform Crime Report Statistics to results from the National Crime Panel victimization survey. Finding virtually no difference between official- and self-report rates, he concludes that high black crime rates represent actual behavior and not selection bias. Similarly, Bernstein et al. (1977) examine data for a sample of male defendants charged with felonies in New York. They analyze three official processing decisions: (1) the decision to fully prosecute, (2) the decision to withhold formal conviction and sentencing, and (3) the severity of sentences. The researchers find no support for the argument that official reactions are harsher for black than for white defendants. They (p. 754) conclude that "the theoretical focus of [labelling theory] requires considerable shifting." Finally, to test the conflict proposition that the most-severe official sanctions are reserved for lower-class persons, Chiricos and Waldo (1975) examine prison sentences received by inmates from three southeastern states. Their results indicate that defendants' race has no effect on sentence severity. They (p. 769) claim that "the evidence . . . consistently fails to support the expectations of conflict criminologists with regard to the state's sanctioning machinery."

---

[1] For a representative sample of the research on racial discrimination in the criminal justice system, see Atkinson and Neuman (1970); Baab and Furgeson (1968); Burke and Turk (1975); Hagan (1974); and Gibson (1978).

Recent research, then, provides little support for the argument that official reactions to crime are affected by the defendant's race. This research is important because it avoids many of the methodological problems of prior studies. However, recent studies on racial discrimination in criminal processing are limited in two respects that are relevant here. First, with few exceptions (Wolfgang and Riedel, 1973), recent research ignores the racial *composition* of the victim-defendant dyad. Collins's (1975) theory of sexual stratification suggests that racial composition—and not individual attributes—of either defendant or victim is most important for predicting official reactions to sexual violation. Second, recent studies of criminal processing examine only one or two processing outcomes (e.g., arrest, sentencing). But, assuming that discriminatory treatment is cumulative, an examination of only one decision may not accurately reflect the effect of processing as a whole. Observing the same problem, Hagan (1974:379) notes: "What is required is longitudinal data, based on observations of defendants' experiences in transit through the criminal justice system" (see also Bernstein et al., 1977:754).

No research, to date, has systematically analyzed the effect of the racial composition of the victim-defendant dyad on processing decisions for the entire criminal selection process. In this study, I present longitudinal data on the determinants of official processing decisions in sexual-assault cases from initial police report to final disposition. My specific purpose is to assess empirically the proposition that the seriousness of reactions to reported sexual assaults will depend on the racial composition of the victim-defendant dyad.

DATA

Sample

The sample for this study consists of 881 suspects charged with "forcible sex offenses"[2] in a large, midwestern city, between January, 1970, and December, 1975.[3] Police, prosecution, and court records provided data on defendants' and victims' demographic characteristics. For each case, I recorded evidence from prosecution files, the defendant's criminal history from police records, and final disposition and sentence from court records. Qualitative data, in the form of observations and interviews with processing agents, supplement the case-specific data and are used to interpret statistical results. I completed observations and interviews over a 12-month period while employed as a research associate by the Prosecutor's Office.

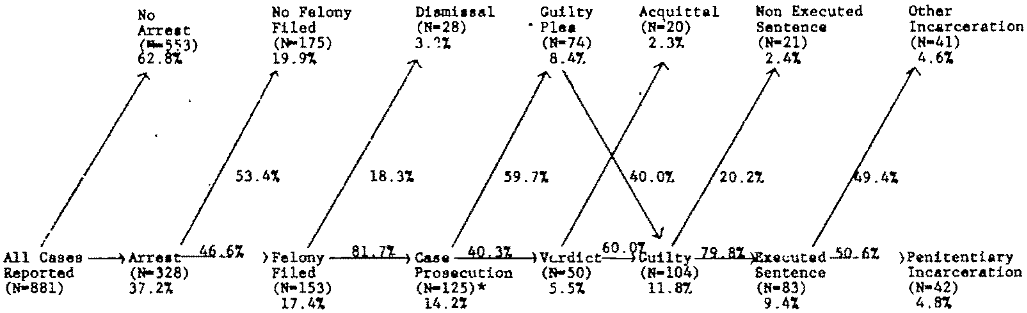
The Criminal Selection Process

Figure 1 presents the major processing decisions made in sexual-assault cases reported to police in the years examined. Two features of Figure 1 are worth noting. First, less than 10% of the 881 defendants received sentences that included incarceration. This means that research examining only sentencing outcomes (e.g., Bullock, 1961; Chiricos and Waldo, 1975) draws conclusions on the basis of a highly select group of cases. Second, while arrest is a consequential processing decision, less than 32% of the suspects arrested were eventually convicted. Thus, studies which

---

[2] "Forcible sex offenses" include rape, armed rape, sodomy, armed sodomy, assault and battery with intent to rape, and assault and battery with intent to gratify. The sample does not include male victims or nonforcible, statutory offenses.

[3] The earliest year with complete records available was 1970. Because cases take up to two years to be adjudicated, 1975 was the latest year I could include in this study. I included 1973 cases because several changes in the organization of the criminal justice system occurred in that year. Examination of time shifts (LaFree, 1979) indicated few significant differences.


No Arrest (N=553) 62.8%
No Felony Filed (N=175) 19.9%
Dismissal (N=28) 3.2%
Guilty Plea (N=74) 8.4%
Acquittal (N=20) 2.3%
Non Executed Sentence (N=21) 2.4%
Other Incarceration (N=41) 4.6%
53.4%
18.3%
59.7%
40.0%
20.2%
49.4%
All Cases Reported (N=881)
Arrest (N=328) 37.2%
46.6%
Felony Filed (N=153) 17.4%
81.7%
Case Prosecution (N=125)* 14.2%
40.3%
Verdict (N=50) 5.5%
60.0%
Guilty (N=104) 11.8%
79.8%
Executed Sentence (N=83) 9.4%
50.6%
Penitentiary Incarceration (N=42) 4.8%

* Data were missing for one case filed in the Prosecutor's Office. Percentages on arrows represent conditional probabilities for each stage.

Figure 1. Disposition of Forcible Rape Cases Reported to Police in 1970, 1973, and 1975

examine only arrest (e.g., Hindelang, 1978) ignore the possibility that variables which affect subsequent processing decisions may differ from those which affect arrest.

Variables

Table 1 shows the dependent and independent variables. The racial composition of the victim-defendant dyad is most central to the theoretical concerns outlined above. But only 11 (1.2%) of the cases involved white offenders and black victims.[4] The small number of these cases precluded meaningful analysis and they were excluded from the sample.[5] For the remaining 870 cases, race composition was dummy-coded as two vectors, with white intraracial assaults being the excluded category.

Dependent variables included in the analysis represent decisions made by (1) police, (2) prosecutor's office, and (3) criminal court. I included two police decisions: (1) *Arrest*, and (2) *Charge Seriousness*. I examined the arrest decision for all 870 reported assaults. And, for 339 suspects who were formally charged, I examined charge seriousness.[6] Charge-seriousness scores were assigned to each offense according to the mean prison term imposed by statute (e.g., 10- to 20-year sentence = a charge-seriousness score of fifteen; see LaFree, 1979).

After arrest, felony screeners in the Prosecutor's Office reviewed cases to determine whether they should be filed in criminal court. For 326 arrests, I examined *Felony Screening*, the decision to file or not file felony charges. Figure 1 shows that only 47% of the arrests for forcible sexual assault were filed as felonies.

For the 151 felony cases, I examined *Case Prosecution*, the decision to continue prosecution or dismiss (*nolle prosequi*) charges. Cases filed as felonies may be dismissed by the State or court at any

---

[4] Two (18.2%) of the 11 white suspects accused of sexually assaulting black women were arrested and had their cases filed as felonies. One man pleaded guilty; the prosecution dismissed the second case. The man who pleaded guilty received a 2-year executed sentence in a minimum security institution.

[5] Based on a study of rapes reported to Philadelphia police in the late 1950s, Amir (1971) concluded that rape was intraracial. Until recently, this conclusion was widely accepted. But several recent studies (e.g., Agopian et al., 1974; Curtis, 1976; Katz and Mazur, 1979; Law Enforcement Assistance Administration [LEAA], 1979) report much higher rates of black offender-white victim rape than those reported by Amir. Black suspect-white victim assaults were *nineteen* times more likely in this sample than white suspect-black victim assaults. Differences in crime reports by race have typically been explained by citing differential rates of reporting by black and white victims (e.g., Griffin, 1971; Savitz, 1973). But the recent availability of national victimization data (e.g., Hindelang, 1978; LEAA, 1979) indicates that differential reporting by race accounts for less of the discrepancy than has been commonly assumed. These findings emphasize the need for further longitudinal research on criminal victimization by race of the victim and offender.

[6] Thirteen men who were charged with an offense were not arrested, either because the charge was dismissed before they were apprehended or because the police were unable to locate them.

Table 1. Variables, Coding, and Frequencies

Variable	Coding	Distribution[a] N	%
Dependent Variables			
Arrest	0=No Arrest	544	62.5
	1=Arrest	326	37.5
Charge Seriousness	Interval Scale (1–22)	Median = 15.07	
Felony Screening	0=Dismissed, Reduced	175	53.7
	1=Filed in Criminal Court	151	46.3
Case Prosecution	0=Dismissed (*Nolle prosequi*)	28	18.5
	1=Prosecuted	123	81.5
Trial	0=Plea Agreement	73	59.4
	1=Court Trial	50	40.6
Verdict	0=Not Guilty	20	40.0
	1=Guilty	30	60.0
Sentence Type	0=Nonexecuted	21	20.4
	1=Executed	82	79.6
Place of Incarceration	0=Minimum Security	40	48.8
	1=Maximum, Medium Security	42	51.2
Sentence Length	Interval Scale (.08–100 years)	Median = 2.5 years	
Independent Variables			
Race Composition	Black Defendants and Victims (BB)	389	44.7
	Black Defendants and White Victims (BW)	200	23.0
	White Defendants and Victims (WW)	281	32.3
Defendant Age	0=21 or Older	226	26.9
	1=20 or Younger	614	73.1
		Median = 24.34	
Defendant Criminal Record	0=No Arrests	155	18.5
	1=Arrests, No Convictions	150	17.9
	2=Convictions, No Time Imposed	150	17.9
	3=Convictions, Time Imposed	384	45.7
Victim Prosecution	0=No Prosecution	179	20.9
	1=Prosecution	677	79.1
Eyewitness	0=No	737	88.2
	1=Yes	99	11.8
Weapon	0=None	569	68.6
	1=Firearm, Knife, Other	261	31.4
Offense Type	0=Attempted Sexual Assault	132	15.4
	1=Sexual Assault	725	84.6

[a] Variation in total number of cases due to missing data.

point, although dismissal was rare once trial procedures began. None of the 28 cases dismissed by the Prosecutor's Office at this stage was later refiled.

The remaining 123 cases were adjudicated through plea bargaining or trial. Two dependent variables measured conviction outcomes. *Trial* measured whether cases were fully prosecuted or resulted in guilty pleas. *Verdict* measured whether guilty or not guilty for all trials.

The court had primary responsibility for sentencing convicted offenders. Three variables measured sentencing outcomes. *Type of Sentence*, whether executed or nonexecuted, indicated whether the sentence included prison incarceration. Nonexecuted sentences represented some combination of probation, suspended sentence, and fine. While not included in earlier studies of sentencing, the place where an offender is incarcerated also reflects the severity of the sanction imposed on him. Incarceration in a local jail or minimum security institution generally represents a less serious sanction than incarceration in a medium or maximum security state penitentiary. Thus, for the 82 men who received executed sentences, *Place of Incarceration* measured whether they were incarcerated in the state penitentiary or elsewhere. Finally, for all men who were incarcerated, *Sentence Length* measured the sentence received.[7]

I identified independent variables from the literature on official reactions to rape (e.g., Sebba and Cahan, 1975; Amir, 1971; Holmstrom and Burgess, 1978). The analysis includes, in addition to race, the defendant's age and prior criminal record. Four variables indicate cases' legal and evidentiary characteristics. These are: (1) victim prosecution preference, (2) witnesses, (3) weapon, and (4) type of offense. Prior research (e.g., Skolnick and Woodworth, 1967; Black, 1971) suggests that more serious sanctions are likely when victims make clear their preference for prosecution of suspects. Although corroboration was not formally required in this jurisdiction, eyewitness identifications may clarify issues of consent and identification—issues that are especially problematic for sexual assault cases (Durham, 1952; Hibey, 1973; National Institute of Law Enforcement, 1977). Whether the assault involved a weapon relates both to the seriousness of the case and to the issue of consent. Likewise, type of offense—whether rape, or attempted rape—reflects the offense's seriousness.

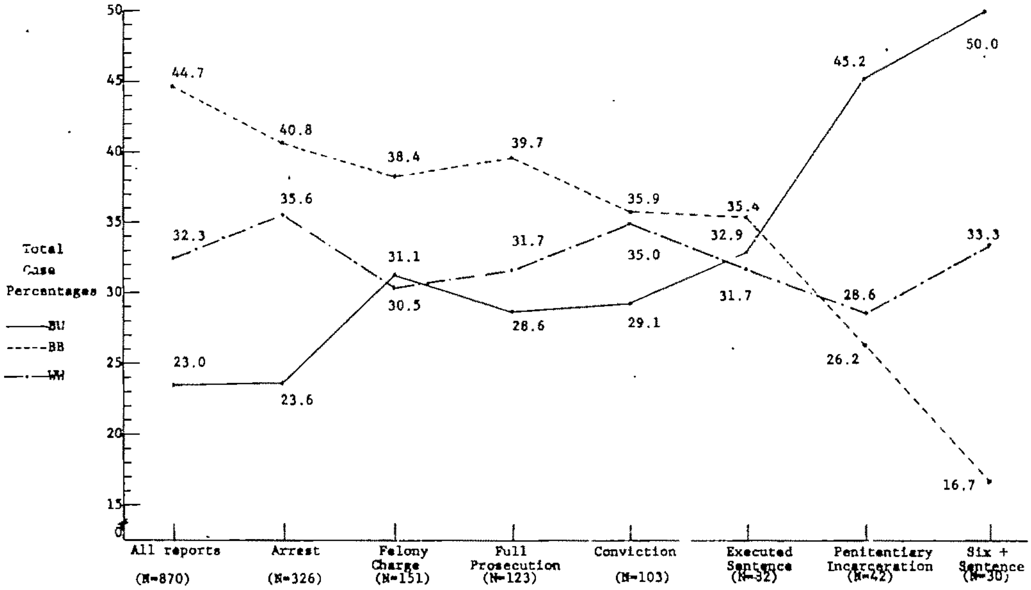
The design utilizes the longitudinal data in two ways. First, each outcome is based on the results of the preceding outcome. For example, only cases filed as felonies are analyzed for prosecution or dismissal. Second, results of earlier decisions are incorporated as independent variables for later decisions. For all defendants who were charged with felonies, charge seriousness is included in subsequent processing decisions. For all convicted defendants, type of conviction—whether guilty plea or verdict—is included in the estimation of sentencing decisions. And, for the analysis of verdict, I include a binary variable—jury or bench trial—to control for type of trial.

RESULTS

Figure 2 compares percentage distributions of black suspect-white victim, black intraracial, and white intraracial incidents for the major processing decisions measured by the dependent variables. Figure 2 shows that, from case report to final sentencing, (1) the percentage of black intraracial assaults steadily declined; (2) the percentage of white intraracial assaults remained relatively constant; and (3) the percentage of cases involving black suspects and white victims steadily increased. The cumulative effect of these changes was substantial. Black men accused of assaulting black women accounted for 45% of all reported cases, but for only 26% of all men sentenced to the state penitentiary and for only 17% of all men who received sentences of six or more years. By contrast, black men ac-

---

[7] A forcible-rape conviction in this jurisdiction carries a penalty of from 2 to 21 years, or life imprisonment if the victim is under the age of twelve. Armed rape and armed sodomy carry sentences of from 10 to 30 years. Men convicted of forcible sodomy may be fined from $100 to $1,000 and imprisoned for from 2 to 14 years. Assault and battery with intent to rape carries a penalty of from 1 to 10 years. Punishment for assault and battery with intent to gratify sexual desires depends on the age of the victim. For victims between 12 and 17, the penalty is from 1 to 5 years. If the victim is under 12 the penalty is from 2 to 21 years. Kidnapping was frequently included with charges for forcible sex offenses. The penalty for kidnapping is life imprisonment. Indeterminate sentences were estimated by taking the mean of the high and low sentence (e.g., 2–21 = 11.5 years). Life sentences were rare, but, when encountered, they were "assigned" 40 years, as did Chiricos and Waldo (1975). For concurrent sentences, I included only the most serious charge.



	All reports	Arrest	Felony Charge	Full Prosecution	Conviction	Executed Sentence	Penitentiary Incarceration	Six + Sentence
BW	23.0	23.6	31.1	28.6	29.1	32.9	45.2	50.0
BB	44.7	40.8	38.4	39.7	35.9	35.4	26.2	16.7
WW	32.3	35.6	30.5	31.7	35.0	31.7	28.6	33.3
N	870	326	151	123	103	82	42	30

N indicates total cases reaching each processing stage. Two dependent variables, Trail (whether trial or guilty plea) and Verdict (guilty or not guilty for court trial) are subsumed here by Conviction. Variation in total number of cases reported due to missing data.

Figure 2. Percentage of Black Suspect-White Victim, Black Intraracial and White Intraracial Incidents for Eight Processing Stages

cused of assaulting white women accounted for 23% of all reported rapes, but for 45% of all men sent to the state penitentiary and for 50% of all men who received sentences of six or more years. Percentages of cases involving white victims and defendants fluctuated between a high of 35.6% (arrest) and a low of 28.6% (penitentiary incarceration). The percentage of white intraracial assaults resulting in prison sentences of more than six years (33.3%) was similar to the percentage of white intraracial assaults among all reported cases (32.3%).

In addition to the overrepresentation of black suspect-white victim cases and the underrepresentation of black intraracial cases, Figure 2 shows substantial variation in the size of percentage differences between processing stages. In fact, black intraracial incidents increased and black suspect-white victim incidents declined for the decision to fully prosecute.[8] Also, the percentage change between report and arrest for black suspect-white victim cases was small: .6%. The corresponding percentage change for black intraracial incidents was 3.9%. In sum, Figure 2 is consistent with the sexual stratification argument but suggests that race composition is differentially important across processing outcomes. I turn now to more elaborate measures of these relationships.

---

[8] Rather than dismiss cases outright against the wishes of the victim, prosecutors preferred to screen cases through different filing options. For example, prosecutors sent cases that they felt were less likely to result in conviction to municipal court bind-over hearings or to the grand jury. Each additional processing stage was perceived as a "dress rehearsal" to determine the strength of the victims' commitment to prosecution. Long prosecution delays give the victim ample opportunity to drop the case without requiring the prosecutor to deny, formally, her claim (for a similar observation, see Holmstrom and Burgess, 1978:127).

Table 2 shows the product-moment correlation (Pearsonian r) between the racial composition of the victim-defendant dyad and the nine sequential processing decisions. Also shown is the proportion of variance (r^2) in one variable that is explained by the other, as well as the sample size (N) for each outcome. Levels of statistical significance for computed correlation coefficients are shown where relevant.

As noted above, the sexual stratification theory predicts that assaults by black men on white women will result in the most serious sanctions (given that white defendant-black victim comparisons are not available). Generally, the results in Table 2 are consistent with this theory. Eight of nine outcomes indicate that black-white interracial assaults are likely to result in *more* serious sanctions for offenders; five of these relationships are statistically significant. Seven of nine outcomes indicate that black intraracial assaults are likely to result in *less* serious sanctions; one of these outcomes is statistically significant. Of the three outcomes which contradict the sexual stratification argument, none are significant.

Several of the relationships that support the sexual stratification interpretation suggest that the differences are not only statistically—but also *substantively*—significant (see Hagan, 1974:361). Assaults involving black offenders and white victims account for 27% of police charge seriousness, 9% of sentence length, 7% of place of incarceration, and 3% of felony screening and sentence type. Black intraracial assaults account for 7% of the variation in verdict and 3% of the variation in place of incarceration. By contrast, the most important of the contradictory findings (case prosecution for black-white interracial offenses) accounts for only 1% of the variance in that outcome.

Table 2. Product-Moment Correlations between Racial Composition and Nine Official Processing Decisions in Forcible Sexual Assault Cases

	Black-White Interracial		Black Intraracial		
	r	r^2	r	r^2	N*
Arrest	.01	.00	−.04	.00	870
Charge Seriousness	.17[c]	.27	−.06	.00	339
Felony Screening	.17[c]	.03	−.04	.01	326
Case Prosecution	−.11	.01	.06	.00	151
Trial	.08	.00	.06	.00	123
Verdict	.12	.01	−.26[a]	.07	50
Sentence Type	.16[a]	.03	−.03	.00	103
Place of Incarceration	.27[b]	.07	−.17	.03	82
Sentence Length	.30[b]	.09	−.13	.02	82

[a] $p<.05$; [b] $p<.01$; [c] $p<.001$.

* Variation in total number of cases due to missing data.

Table 2 shows why studies of official discretion that measure only the defendant's race (and not the race composition of the victim-defendant dyad) may be misleading. According to Table 2, black suspects were less likely to receive executed sentences when the victim was black and more likely to receive executed sentences when the victim was white. Of the sexual assaults, by black men, reaching this stage, 55% were intraracial. Thus, despite the significance of race composition for the decision, black suspects *as a group* are no more likely than white suspects to receive executed sentences (r = .06, n.s. at $p \leq .05$; not presented in tabular form). I turn now to the question of whether zero-order relationships apply when other attributes of the defendant—and the legal and evidentiary characteristics of the case—are considered.

Table 3 presents stepwise multiple correlations between the nine processing outcomes and the independent variables. In this phase of the analysis I am particularly interested in how early in the process racial composition variables enter each equation and, further, how much of an additional contribution each variable makes to the proportion of variance explained in the processing outcome. Table 3 shows R and R^2 values at each step in the process. Immediately below these values I list the variable added at each step. For example, prosecution preference (PREF) was the variable most highly correlated with the decision to arrest and it shows up in the zero-order column with an R = .39. By the fifth-order, weapon,

Table 3. Stepwise Multiple Correlations with Nine Processing Outcomes As Dependent Variables for Forcible Sexual Assault Cases

	Zero-Order		1st-Order		2nd-Order		3rd-Order		4th-Order		5th-Order	
	R	R²	R	R²	R	R²	R	R²	R	R²	R	R²
Arrest	.39	.16	.41	.17	.42	.18	.42	.18	.42	.18	.42	.18
(N=780)	PREF		WEAP		WITNS		TYPE		RACE(BW)		RACE(BB)	
Charge Seriousness	.41	.16	.49	.24	.51	.27	.52	.27	.52	.27	.52	.27
(N=341)	TYPE		WEAP		RACE(BW)		PREF		WITNS		RACE(BB)	
Felony Screening	.18	.03	.23	.05	.25	.06	.25	.06	.26	.06	.26	.07
(N=324)	CHARGE		WITNS		RACE(BW)		PREF		WEAP		DAGE	
Case Prosecution	.12	.01	.16	.03	.18	.03	.19	.04	.20	.04	.20	.04
(N=151)	CHARGE		RACE(BW)		TYPE		WEAP		WITNS		RACE(BB)	
Trial	.15	.02	.18	.03	.19	.04	.22	.05	.24	.06	.24	.06
(N=125)	CHARGE		WITNS		RACE(BB)		RACE(BW)		DAGE		WEAP	
Verdict	.34	.11	.47	.22	.52	.27	.56	.31	.57	.32	.58	.33
(N=49)	CRIMREC		DAGE		WEAP		WITNS		TYPE		BENCH	
Sentence Type	.34	.11	.39	.15	.41	.17	.43	.19	.43	.19	.44	.19
(N=103)	CRIMREC		CHARGE		RACE(BW)		TYPE		WITNS		RACE(BB)	
Place of Incarceration	.39	.15	.46	.21	.48	.23	.50	.25	.52	.27	.54	.29
(N=81)	GILTYPE		RACE(BW)		WEAP		CRIMREC		CHARGE		TYPE	
Sentence Length	.41	.17	.54	.30	.58	.34	.60	.35	.60	.36	.60	.36
(N=82)	GILTYPE		CHARGE		RACE(BW)		DAGE		WEAP		WITNS	

Race composition variables are underlined to facilitate comparisons.

Item identifications are: RACE(BW)=black defendants and white victims; RACE(BB)=black defendants and victims; DAGE=defendant age; CRIMREC=defendant criminal record; PREF=victim prosecution preference; WITNS=eyewitness; WEAP=weapon; TYPE=offense type; CHARGE=charge seriousness; GILTYPE=conviction by plea or trial; BENCH=trial by jury or judge.

Table 4. Beta Coefficients and Standard Errors for Independent Variables with Processing Outcomes in Forcible Sexual Assault Cases ($p < .10$)

				R^2
Arrest	.408(.038) PREF	−.120(.033) WEAP	.079(.047) WITNS	.177
Charge Seriousness	−.393(.497) TYPE	.265(.378) WEAP	.146(.399) RACE(BW)	.265
Felony Screening	.157(.007) CHARGE	.127(.073) WITNS	.105(.066) RACE(BW)	.063
Verdict	.305(.058) CRIMREC	.330(.141) DAGE	−.243(.136) WEAP	.275
Sentence Type	.349(.032) CRIMREC	.173(.010) CHARGE	.140(.083) RACE(BW)	.170
Place of Incarceration	.380(.109) GILTYPE	.235(.108) RACE(BW)		.209
Sentence Length	.352(3.42) GILTYPE	.342(.479) CHARGE	.210(3.36) RACE(BW)	.340

Race composition variables are underlined to facilitate comparisons.
See Table 3 for item identifications.

eyewitness, type of assault, black-white interracial and black intraracial have been added, providing an $R = .42$ with six independent variables in the equation.

According to Table 3, the black-white interracial composition variable enters six of the nine outcomes by the second-order. The largest increment in explained variance (R^2) produced by adding race composition was for place of incarceration (R^2 increment = .06); followed by sentence length and case prosecution (R^2 increment = .04); police charge (R^2 increment = .03); and sentence type and felony screening (R^2 increment = .02). Overall, the inclusion of racial composition does improve the prediction of outcomes appreciably, with the greatest increases occurring for later, sentencing outcomes.

Table 4 presents the beta coefficients and standard errors for all variables which had significant net effects on processing outcomes. The prediction equations for two outcomes (case prosecution and trial) were not significant ($p \leq .10$) and results are not presented. Thus, there is no indication that racial composition had a net effect on these two outcomes. Racial composition had a significant effect on five of the remaining seven outcomes. Black offender-white victim cases were the most important racial consideration in all five of these outcomes. This variable enters the equation on the second-order for four of the outcomes and on the first-order for one outcome. All the significant results are in the direction predicted by the sexual stratification theory. Compared to other defendants, blacks who assault white women receive more serious charges, are more likely to have their cases filed as a felony, are more likely to receive executed sentences, are more likely to be incarcerated in the state penitentiary, and receive longer sentences. By contrast, black men charged with assaulting white women are *not* more likely to be arrested or found guilty in court than other men. And, contrary to the zero-order correlation results, black intraracial assaults have no effect on verdict, net of defendants' criminal record and age or whether incidents included weapons.[9]

Taken together, the results indicate that (1) processing decisions in sexual assault cases are affected by the racial composition of the victim-defendant dyad, (2) the cumulative effect of race composition is substantial, but (3) the importance of race composition varies across processing outcomes. Decisions in the criminal justice system may be classified according to whether their primary purpose is to determine (1) whether the defendant is guilty or (2) how serious the offense is. For example, arrest, verdict, and the prosecutor's decision to dismiss are all decisions related primarily to the suspect's perceived guilt.[10] By contrast, charge seriousness, sentence type, place of incarceration, and sentence length all measure the *seriousness* of the sanction imposed; they do not address the question of whether the defendant is guilty or innocent.

---

[9] Part of the reason that black intraracial assaults did not have a significant effect on verdict is due to small sample size (N = 50). In a related study (LaFree, 1980), which included different independent variables, assaults on black victims were significantly less likely to result in guilty verdicts.

Application of these two categories to the results indicates that race composition of the victim-defendant dyad has a greater effect on decisions measuring seriousness of the offense than on those indicating guilt or innocence. Racial composition was most important in predicting charge seriousness, felony screening, sentence type, place of incarceration, and sentence length. With the exception of felony screening, which relates to the issue of guilt (through dismissal) and severity (through reduction of charges), all of these decisions measure perceived offense seriousness. By contrast, racial composition was unrelated to arrest, case prosecution, trial, and verdict. With the exception of trial, which probably reflects the prosecutor's perceptions of the defendant's guilt and seriousness of the offense as well as the defendant's willingness to bargain, all of these decisions focus on the defendant's guilt.

It appears, then, that officials were reluctant to rely on racial characteristics to totally deny sexual assault complaints. By contrast, the victim and offender's race clearly affected official perceptions of offense seriousness.

SUMMARY AND CONCLUSIONS

The findings suggest, rather conclusively, that black men accused of sexually assaulting white women receive more serious sanctions than other sexual assault suspects. This conclusion is given added credence by the fact that racial composition had significant effects on five of nine processing decisions, net of evidence and legal severity measures. The results are generally consistent with the proposition that American society is characterized by a sexual stratification system which imposes more serious sanctions on men from less powerful social groups who are accused of assaulting women from more powerful social groups.

Two important differences between this and previous studies account for the results. Given the history of sexual segregation by race in this country, the racial composition of the victim-defendant dyad—and not the individual race of either offender or victim—is the most important racial consideration in processing decisions. Few previous studies have conceptualized the problem in this way. Also, the longitudinal nature of this research was unique. While evidence of discrimination by race in any single outcome was not overwhelming, the cumulative effect of race was consistent and substantial. Thus, studies examining only one or two processing outcomes (e.g., Chiricos and Waldo, 1975; Hindelang, 1978) may or may not support a view of discriminatory processing, depending on independent variables included, methods used, and specific outcomes examined.

Recent critics (e.g., Taylor et al., 1973; Chiricos and Waldo, 1975; Hindelang, 1978) argue that conflict theorists often do not include systematic empirical evidence to support their theoretical claims. Turk (1979:463) suggests that the validity of such partisan research cannot be assessed without sharing or rejecting its authors' ideological assumptions. This tendency has made conflict theory particularly vulnerable to empirically derived criticisms. These criticisms suggest that, for conflict theory to be viable, researchers must accumulate empirical evidence relevant to existing conflict propositions. The present study was an attempt to do this with regard to a specific conflict proposition: the severity of official sanctions imposed on men for the sexual violation of women will depend on the relative power of the victim and of the suspect, determined in part by the race of each. No conclusions can be reached about white offender-black victim cases. Nonetheless, the results indicate that official reactions (of criminal justice personnel) to rape are affected by the racial composition of the victim-offender dyad. Specifically, the results were consistent with the sexual stratification argument that more powerful groups in society enforce sexual-property rules, in part, by the differential application of the criminal law. And, more generally, the findings support conflict theory's longstanding claim that race and other legally irrelevant variables significantly affect official reactions to criminal offenses.

---

[10] The emphasis here is on *primary* purpose of the decision. Clearly, decisions like arrest (Piliavin and Briar, 1964) and verdict (Kalven and Zeisel, 1966) may reflect not only official presumptions of guilt or innocence but also perceived severity of offenses. The important distinction is that arrests and verdicts, unlike charges and sentences, cannot be more or less serious. By contrast, a more or less serious charge cannot distinguish guilty persons from innocent persons.

REFERENCES

Agopian, Michael W., Duncan Campbell, and Gilbert Geis
1974 "Interracial forcible rape in a North American city." Pp. 93–102 in Israel Drapkin and Emilio Viano (eds.), Victimology. Lexington, Mass.: Lexington.

Amir, Menachem
1971 Patterns in Forcible Rape. Chicago: University of Chicago Press.

Atkinson, David N., and Dale A. Neuman
1970 "Judicial attitudes and defendant attributes: some consequences for municipal court decision-making." Journal of Public Law 19:69.

Baab, G. A., and W. R. Furgeson, Jr.
1968 "Texas sentencing practices: a statistical study." Texas Law Review 45:471.

Battelle Law and Justice Study Center
1975 Rape Legislation: A Digest of Its History and Current Status.

Bernstein, Ilene N., William R. Kelly, and Patricia Doyle
1977 "Societal reaction to deviants: the case of criminal defendants." American Sociological Review 42:743–55.

Black, Donald
1971 "The social organization of arrest." Stanford Law Review 23:1087–110.

Bullock, Henry
1961 "Significance of the racial factor in the length of prison sentences." Journal of Criminal Law, Criminology, and Police Science 52:411.

Burke, Peter, and Austin Turk
1975 "Factors affecting postarrest dispositions: a model for analysis." Social Problems 22:313.

Chambliss, William
1969 Crime and the Legal Process. New York: McGraw-Hill.

Chambliss, W. J. and R. B. Seidman
1971 Law, Order and Power. Reading, Mass.: Addison-Wesley.

Chiricos, T. G., and G. P. Waldo
1975 "Socioeconomic status and criminal sentencing: an empirical assessment of a conflict proposition." American Sociological Review 40:753–72.

Collins, Randall
1975 Conflict Sociology. New York: Academic Press.

Curtis, Lynn A.
1976 "Rape, race and culture: some speculations in search of a theory." Pp. 117–34 in M.J. Walker and S. L. Brodsky (eds.), Sexual Assault. Lexington, Mass.: D.C. Heath.

Davis, Kingsley
1949 Human Society. New York: Macmillan.

Durham, James A.
1952 "Forcible and statutory rape: an exploration and objectives of the consent standard." Yale Law Journal 62:55–83.

Gibson, James L
1978 "Race as a determinant of criminal sentences: a methodological critique and a case study." Law and Society Review 12:455–78.

Griffin, Susan
1971 "Rape: the all-American crime." Ramparts 10:26–36.

Hagan, John L.
1974 "Extra-legal attributes and criminal sentencing: an assessment of a sociological viewpoint." Law and Society Review 8:357–84.

Hibey, Richard A.
1973 "The trial of a rape case: an advocate's analysis of corroboration, consent and character." American Criminal Law Review: 309–34.

Hindelang, Michael J.
1978 "Race and involvement in crimes." American Sociological Review 43:93–109.

Holmstrom, Lynda L., and Ann W. Burgess
1978 The Victim of Rape: Institutional Reactions. New York: Wiley-Interscience.

Johnson, Elmer
1957 "Selective factors in capital punishment." Social Forces 36:165–9.

Judson, C. J., L. J. Pandell, J. B. Owens, J. L. McIntosh, and D. L. Matschullat
1969 "A study of the California penalty jury in first degree murder cases." Stanford Law Review 21:1297–431.

Kalven, Harry, and Hans Zeisel
1966 The American Jury. Boston: Little, Brown.

Katz, Sedelle, and Mary Ann Mazur
1979 Understanding the Rape Victim: A Synthesis of Research Findings. New York: Wiley-Interscience.

LaFree, Gary D.
1979 "Determinants of police, prosecution and court decisions in forcible rape cases." Ph.D. dissertation, Department of Sociology, Indiana University.
1980 "Variables affecting guilty pleas and con-

victions in rape cases: toward a social theory of rape processing." Social Forces 58:833–50.

Law Enforcement Assistance Administration
1979 Rape Victimization in Twenty-Six American Cities. U.S. Government Printing Office.

Lemert, Edwin M., and Judy Roseberg
1948 "The administration of justice to minority groups in Los Angeles County." University of California Publications in Culture and Society 11:1–10.

Levi-Strauss, C.
1949 Les Structures 'Elementaires de la Parente. Paris: Presses Universitaires de France.

Mills, C. Wright
1948 The Marxists. New York: Harcourt, Brace.

Myrdal, Gunnar
1944 An American Dilemma. New York: Harper and Brothers.

National Institute of Law Enforcement and Criminal Justice
1977 Forcible Rape, A National Survey of the Response by Prosecutors. Prosecutor's Volume I. U.S. Department of Justice. Law Enforcement Assistance Administration. Washington, D.C.: U.S. Government Printing Office.

Nelson, Steve, and Menachem Amir
1975 "The hitchhike victim of rape: a research report." Pp. 47–64 in Israel Drapkin and Emilio Viano (eds.), Victimology: A New Focus, Vol. V., Lexington, Mass.: Lexington.

Partington, Donald H.
1965 "The incidence of the death penalty for rape in Virginia." Washington and Lee Law Review 22:43–75.

Piliavin, Irving, and Scott Briar
1964 "Police encounters with juveniles." American Journal of Sociology 70:206–14.

Quinney, R.
1970 The Social Reality of Crime. Boston: Little, Brown.

Rose, Vicki McNickle
1977 "Rape as a social problem: a byproduct of the feminist movement." Social Problems 25:75–89.

Rowan, Carl
1952 South of Freedom. New York: Knopf.

Savitz, Leonard D.
1973 "Black crime." Pp. 467–517 in K. S. Miller and R. M. Dreger (eds.), Comparative Studies of Blacks and Whites in the United States. New York: Seminar Press.

Sebba, Leslie, and Sorel Cahan
1975 "Sex offenses: the genuine and the doubted victim." Pp. 29–46 in I. Drapkin and E. Viano (eds.), Victimology: A New Focus, Vol. V, Lexington, Mass.: Lexington Books.

Sellin, Thorsten
1928 "The Negro criminal: a statistical note." The Annals of the American Academy of Political and Social Science 140:52.

Skolnick, Jerome H., and J. Richard Woodworth
1967 "Bureaucracy, information and social control: a study of a moral detail." In David J. Bordua (eds.), The Police: Six Sociological Essays. New York: Wiley.

Taylor, I., P. Walton, and J. Young
1973 The New Criminology: For A Social Theory of Deviance. New York: Harper and Row.

Turk, Austin
1969 Criminality and Legal Order. Chicago: Rand McNally.
1979 "Analyzing official deviance." Criminology 16:459–76.

Wolfgang, M.E., and B. Cohen
1970 Crime and Race: Conceptions and Misconceptions. New York: Institute of Human Relations Press.

Wolfgang, M. E., and M. Riedel
1973 "Race, judicial discretion, and the death penalty." The Annals of the American Academy of Political and Social Science 407:119.

Woodward, Comer Vann
1955 The Strange Career of Jim Crow. New York: Oxford University Press.

COMMENTS

THE NONINFLUENCE OF "FAMILY BACKGROUND" ON INTELLECTUAL ATTAINMENT*

(A CRITIQUE OF SCARR AND WEINBERG, *ASR* OCTOBER, 1978)

This critique places a recent article by Scarr and Weinberg (1978) into the eugenic or hereditarian school of studies which has examined the relative impacts of genetics and environment on IQ. Because of the tremendous policy implications of the resolution of the IQ debate, studies bearing on the question should be closely scrutinized for scientific rigor and objectivity. The Scarr and Weinberg article fails to meet rigorous scientific standards for several reasons. The theoretical concepts of genetics and environment are not explicitly and satisfactorily operationalized. The authors explain only a very small portion of the variance of the dependent variable, adolescent IQ, yet make conclusions based upon these insignificant results. Sampling bias undercuts environmental variation and jeopardizes external validity. The use of regression analysis is unclear, and correlation "heritability" analysis is arbitrarily and unfoundedly employed.

The perpetual debate in social science concerning the relative contributions of genetic inheritance and environment to individual development, in general—and to intellectual achievement, in particular—is not only intrinsically interesting but also has social policy implications. So great are the policy implications of this debate for the distributions of rewards within society that two clearly identifiable camps have emerged on the issue, despite a lack of rigorous application of scientific methodology in developing causal links and despite inconclusive data.

One school of authors of studies of IQ test scores and of factors causing score differentials between groups is the eugenic or hereditarian school and includes the works of Burt (1972; 1970), McDougall (1910; 1924), Davenport (1911; 1928; 1933; 1934), Eysenck (1971), Jensen (1969a; 1969b), and Reed et al. (1954). Into this camp also falls the recent study by Scarr and Weinberg (1978). This school argues that the impact of heredity upon IQ is great and exceeds the impact of environment upon IQ.

The other camp, called the environmental school, or the "naive environmental" school by its critics, remains skeptical that importance of heredity in IQ development exceeds the importance of environment. These authors cite the known deleterious effects (of malnutrition, disease, and deprivation due to poverty) upon IQ development as proof of the potency of environmental factors. Into this second camp fall on-going studies by the National Center for Health Statistics (Dupuy, 1977) examining links between family income and well-being and standardized tests and outcomes; Drillien's (1968; 1969) work examining social class, birth weight, and IQ scores in all-Nordic child populations; the explanation of group differences by Pasamanick (1951), Pasamanick and Lilienfeld (1955), and Pasamanick and Knobloch (1955); and the studies of Eichenwald and Fry (1969) of mental retardation prevention through the control of infectious diseases.

To the degree that the impact of genetic inheritance is greater than that of environmental factors, dominant groups in society may not only be expunged of guilt for disparities in intellectual attainment with minority groups but also alleviated of responsibility for compensatory education and other environmental mechanisms to close the gaps. Throughout history, the policy consequences of this intellectually unresolved question have already been great. Dominance of the eugenic or hereditarian school has helped to establish the racial immigration quotas in the U.S. Immigration Act of 1924. Between 1925 and 1941, the effect of this act was to reduce by over six million the immigration flow of groups labeled inferior by IQ test results, such as Jews, Italians, and Greeks. During the same time period in Germany and Europe, eugenicism (run rampant) contributed the horrific genocide of the Holocaust. In the United States, the Supreme Court doctrine of "separate but equal" education for blacks, in practice, was often separate, unequal, and yet justified by eugenic philosophy. More recently the environmental school has provided justification for compensatory education programs. Because of the obvious emotionalism of the debate and the magnitude of its outcome for public policy (both historically and in the future) academic research on the subject should strive for impeachable objectivity, avoiding conclusions which are unsupported by scientifically collected and analyzed data. Thus, each new piece of evidence bearing on the genetic inheritance-environment question should be scrutinized with great interest for scientific rigor and legitimacy. The Scarr-Weinberg (1978) article is no exception to this rule.

---

* Direct all correspondence to: Marcia Whicker Taylor; Department of Government and International Studies; University of South Carolina; Columbia, SC 29208.

Scarr and Weinberg (1978) employ data collected on two samples of families: "biological" families, or those who have children by the birth process and whose offspring are consequently genetically related, and "adoptive" families whose children are adopted shortly after birth and are consequently not genetically related to the adopted parents or siblings. On the basis of analysis of these data, Scarr and Weinberg (1978:686) conclude

> it seems evident to us that the study of adoptive and biological families provides extensive support for the idea that half or more of the long-term effects of "family background" on children's intellectual attainments depend on genetic, not environmental, transmission.

This conclusion is incredible because none of their data, nor their analysis of the data, supports this conclusion. Furthermore, their tortuous analyses often obfuscate rather than clarify the evidence bearing on the basic question of genetic inheritance vs. environmental impacts. The conclusions that seem so evident to Scarr and Weinberg may not appear so obvious to others, for these reasons:

(1) Lack of explicit operationalization of theoretical concepts employed. What is being measured and tested in the Scarr-Weinberg (1978) research is ambiguous and slippery. While the authors begin their presentation by discussing the potential saliency of "family background" on intellectual attainment, they quickly switch to a discussion of "behavior genetic" methods and argue (p. 675) that "theoretically, regressions of adopted child outcomes on adoptive family characteristics will provide genetically unbiased estimates of true environmental effects in the population." While the regressions of adopted-child outcomes on adoptive-family characteristics do provide estimates of the environmental effects of the particular family characteristics measured and employed in the analysis, those family characteristics employed by Scarr and Weinberg are by no means inclusive of all relevant environmental effects. Indeed, the Scarr-Weinberg family variables are not even inclusive of all relevant familial environmental effects, as any attempts to measure the nature and intensity of nurturing or parental interest in child learning and intellectual attainment have been precluded. It is inappropriate to imply, as do Scarr and Weinberg, that, because the biological and adoptive families have slightly different R^2's in the regressions, genes (and not environment) are the source of family-background transmissions. More precisely, the authors never clarify what specifically operationalizes the concepts, "family genetic transmission" and "family environment transmissions." Are all family characteristics of the biological sample measures of family genetic transmission? If all of these variables (for example, father's education, mother's education, father's occupation, family income, birth rank, number of children, father's IQ, and mother's IQ) are indeed indirect measures of family genetic transmission in the biological family sample, why are they not more highly correlated? No correlation of family background characteristics exceeds .61 (Table 2, Scarr-Weinberg, 1978:678), the correlation for father's occupation and father's education, and most correlations of family characteristics in the biological sample are considerably less. One could argue that the relationship of father's occupation to father's education, for example, is a greater reflection of labor market demand than of family genetic transmission to adolescent IQ.

Whether the family background characteristics of the biological sample measure genetic transmissions, at all, is doubtful at worst and conjecture at best. If one concedes that at least some portion of these background characteristics measures genetic inheritance, the thoughtful reader must conclude that the family background characteristics for the biological sample in the Scarr-Weinberg (1978) study are a mixture of genetic and environmental transmissions, to the degree that genetic inheritance is measured at all. How the authors (p. 688) can then conclude, as they do, that these family background characteristics actually understate, rather than overstate, measured genetic transmissions is most perplexing.

(2) Very small explained variance in the dependent variable. Even to the casual reader familiar only with the rudiments of regression analysis, the aspect of the Scarr-Weinberg (1978) regression equations totally ignored by the authors but, nonetheless, so obvious and compelling, is the paltry amount of variance in the dependent variable, even by social science standards, explained by the independent variables they employ. The bulk of the article is spent arguing about the relative contributions of various independent variables, as well as the larger meaning of those variables for the gene-environment debate, when the reported R^2's show that all of the independent variables, combined, explain almost none of the variance in adolescent IQ. For the biological sample, the

highest R^2 obtained is .31, while, for the adoptive sample, an even lower .16 is the best obtained. Eight independent variables are used to obtain the model explaining 31% of the biological sample dependent variable variance, while 11 independent variables are used to obtain the model explaining 16% of the adoptive sample dependent variable variance. The obvious conclusion from these R^2's is that family background characteristics explain almost nothing about adolescent IQ variation—an incredible finding, which should have led the authors to note, as does this comment, the noninfluence of family background, at least as measured by Scarr and Weinberg, on intellectual attainment. With so little of the total variation in the dependent variable explained, to dwell at length (as do the authors) on the relative contributions of independent variables is to make "much ado about nothing." Despite R^2's, which never explain more than .31 of the variance, and despite the fact that these low R^2's were generated by mixture of environmental and genetic transmission measures, the authors blithely conclude that genetic transmissions account for 40% to 70% of adolescent IQ variation, a finding totally unsupported by the data.

(3) Sampling bias undercuts environmental variation and jeopardizes external validity. Random sampling is not employed to extract an unbiased sample for the Scarr-Weinberg (1978) study. Consequently, external validity—that is, the extent to which conclusions from the data can be generalized to a broader population—is limited. An analysis of the socioeconomic characteristics of both samples reveals that the largely self-selected samples were atypical of the general population in several potentially salient ways (for example, family income, parental education levels, etc.). Furthermore, the sampling procedure compressed the variance for environment, if socioeconomic characteristics were meant to measure environment. In essence, environment is held fairly constant at enriched levels for both samples. The subsequent finding that observed variation in the dependent variable is based on inheritance transmission is not startling, given that environmental variance was compressed, while genetic variance, especially for the adoptive sample, was not.

(4) Unclear use of regression analysis. If the authors intended for birth rank and family size to measure environment and for the remainder of family background characteristics to measure genetic inheritance for the biological sample, the rationale underlying the order in which the regression variables were loaded when analyzing the two samples is unclear. Standard procedure when a researcher is attempting to demonstrate the greater significance of one variable or set of variables, as Scarr and Weinberg implicitly if not explicitly are trying to demonstrate the greater potency of genetic transmission measures, is, first, to load all other independent variables. The variable or variables in which one has the greatest interest are then loaded in a second stage and are regressed into the residuals resulting from the first stage regression with all other variables. Not only do Scarr and Weinberg not follow this procedure but they also intersperse the "environmental" variables of birth rank and family size with "genetic" variables.

Second, the relative contribution (of any independent variable in a regression equation), toward explaining dependent variable variation, depends in part on which other independent variables are in the equation. Hence, one can compare independent variable effects across regression equations only if the exact same variables are employed in each equation, a procedure which is violated when additional variables for natural mother characteristics are loaded into the adoptive sample, in addition to other family background characteristics. A more appropriate comparison would be to compare the natural parent characteristics for both samples. While IQ scores may not have been readily available for natural mothers of adopted children, surely additional data on the natural mothers in the biological sample equivalent to the data obtained for natural mothers of adopted children could have been secured. However, only education was used for the latter group, and data on age and occupation which was collected for natural mothers of adopted children was not obtained for the natural mothers of the biological sample—a puzzling omittance, despite the fact that some mothers did not "work" (housewives would counter that they *do* work).

Third, because the signs of some regression coefficients are opposite from the direction anticipated, the authors ignore them in the analysis and discuss total R^2's. This is compounded, however, by the invalidation of cross-regression equation comparisons since family background characteristics are not measuring, by the authors' assertion, the same kind of transmissions in the two samples. Had equivalent measures been loaded for the two samples simultaneously, two-sample significance tests testing for the cross-equation differences in equations might have been conducted.

(5) Arbitrariness in correlation "heritability" analysis. Having failed with regres-

sion analysis to show that family background characteristics are salient to adolescent IQ., the authors launch into bivariate correlation analysis. That correlations among related family members of the biological sample are generally higher than the equivalent correlations among unrelated family members of the adoptive sample is interesting, although given that no control variables are employed, the possibility that spurious relationships (which might account for the higher biological-sample correlation) cannot be discounted. Then, however, on the basis that related family members share half of their genes, the authors arbitrarily decide to multiply the difference in correlations between samples by 1.6 (2., adjusted for "assortative mating") in a calculation that makes no theoretical sense. If adolescents in the adoptive sample share *no* genes with adoptive parents, and if biological sample adolescents share *all* genes with their natural parents, half with the mother and half with the father, then the difference between sample correlations alone should reflect genetic transmissions. If one is concerned about assessing the combined effect of both parents, midparent-child correlation differences would be more appropriate. Unfortunately, for Scarr and Weinberg (1978) mid-parent correlation differences are less than those obtained by the arbitrary multiplications.

(6) Unexplained sample differences. Scarr and Weinberg (1978) use enlarged samples for their correlation analysis. Given the ability of most regression programs to handle missing data in an airwise fashion, the rationale for the smaller sample sizes for the regression analysis are never explained.

In conclusion, what began as an interesting and imaginative piece of research did not live up to its original promise, primarily due to the lack of conceptual clarity in specifying the operationalization of key concepts and the authors' haste to prove the potency of genes.

Marcia Whicker Taylor
University of South Carolina

REFERENCES

Burt, Cyril
1970 "The mental differences between individuals," Occupational Psychology 44:141–7.
1972 "Inheritance of general intelligence," American Psychologist 43:175–90.
Davenport, C. B.
1911 "Euthenics and eugenics," Popular Science Monthly 78:16–20.
1928 "Crime, heredity, and environment," Journal of Heredity 19:307–13.
1933 "An alleged case of inheritance of acquired characteristics," American Naturalist 67:549–59.
1934 "Heredity and environment in man," Science 79:185.
Drillien, Cecil M.
1968 "Studies in mental handicap: some obstetric factors of possible aetiological significance," Archives of Disease in Childhood 43:283–94.
1969 "School disposal and performance for children of different birthweight born 1953–1960," Archives of Disease in Childhood 44:562–70.
Dupuy, Harold J., and Gunnar Gruvaeus
1977 The Construction and Utility of Three Indexes of Intellectual Achievement. Hyattsville, MD.: National Center for Health Statistics.
Eichenwald, Heinz F., and Peggy C. Fry
1969 "Nutrition and learning," Science 1963:644–8.
Eysenck, H. J.
1971 The I.Q. Argument: Race, Intelligence, and Education. New York City: Library Press.
Jensen, Arthur R.
1969a "How much can we boost I.Q. and scholastic achievement?" Harvard Education Review 39:1–123.
1969b "Learning ability and socioeconomic status," Journal of Special Education 3:23–35.
1970 "I.Q.'s of identical twins reared apart," Behavior Genetics 1:133–44.
McDougall, W.
1910 "Instinct and intelligence," British Journal of Psychology 3:250–66.
1924 "Racial mental differences," Reports of the British Association for the Advancement of Science 439–40.
Pasamanick, Benjamin
1951 "The intelligence of American children of Mexican parentage: a discussion of uncontrolled variables," Journal of Abnormal Social Psychology 46:598–602.
Pasamanick, Benjamin, and A. Lilienfeld
1955 "Association of maternal and fetal factors with the development of mental deficiency. Journal of the American Medical Association 159:155–60.
Pasamanick, Benjamin, and Hilda Knobloch
1955 "Early language behavior in Negro children and the testing of intelligence." Journal of Abnormal Psychology 50:401–2.
Reed, Sheldon C., Elizabeth W. Reed, and J. D. Palmer
1954 "Fertility and intelligence among families of the mentally deficient," Eugenics Quarterly 1:44–52.
Scarr, Sandra, and Richard A. Weinberg
1978 "The influence of 'family background' on intellectual attainment." American Sociological Review 43:674–92.

CALLING ALL CAMPS! THE WAR IS OVER!*

(REPLY TO TAYLOR)

We appreciate the opportunity to address again the readership of *ASR* because the journal is central to the social sciences. Contrary to Taylor's (1980) muddled chauvinism, we propose that the Hundred Years War between nature and nurture is over. Her last-ditch skirmishes with genetic differences in behavior bear as little resemblance to current thinking in the social sciences as Shockley's sperm bank bears to contemporary thought in the biological sciences.

The problem we studied, the effects of "family background," is of concern to both social and biological scientists. A multidisciplinary perspective provides more complex models, which can lead to more complete explanations of the phenomena of parental influence on their children. Although there are advantages to viewing the same problem through several conceptual lenses, there are inherent difficulties in communication among people from several disciplines.

Taylor, like most social scientists, seems to be familiar with the concept of "family background," as it is used in the social sciences, to mean the total biasing effects of family membership on long-term outcomes of children. The focus of social science research is on *how much* bias, regardless of the sources of the bias. Most social scientists state or imply that family *environmental* differences are the source of "family background" effects.

Evolutionary biologists and behavior geneticists focus on *how* "family background" affects long-term outcomes of children. "Family background" is considered to include the effects of shared genes *and* shared environments. Taylor evidently does not understand that questions about sources of bias in "family background" for children who are genetically related to their parents must include a consideration of both genetic and environmental transmission from parent to child. In our study of adoptive and biologically related families, we attempted to answer both social and biological questions of "how much" and "how."

As psychologists, sympathetic to both social and biological perspectives, we are concerned with sources of individual differences and with the application of developmental psychology to the welfare of children and their families (e.g., Scarr, 1969, on birth weight and IQ; Scarr and Williams, 1973, on early intervention for low-birth-weight infants; Scarr, 1979, special issue of the *American Psychologist* on psychology and children's welfare; Scarr and Schwarz, 1979, on an early intervention program; Weinberg, 1979, on preschool intervention programs; Weinberg and Moore, 1975, on the evaluation of early intervention programs). We are committed to the *realistic* use of environmental interventions on behalf of children. An integral part of our conceptual framework is the understanding that human variation depends most often on both genetic and environmental differences.

Differences versus Development

A common error among social scientists is the failure to distinguish genetic and environmental sources of individual *differences* in behavior from the necessary roles of both genes and environments in behavioral *development*. One cannot assess the relative impact of heredity or environment on intelligence, per se, because everyone must have both a viable gene complement and an environment in which the genes can be expressed over development. No genes, no organism; no environment, no organism.

Behavioral *differences* among individuals, on the other hand, can arise in any population from genetic differences, from variation among their environments, or both. Imagine a population of genetically identical clones who are reared in family environments that vary from working to upper-middle class. Any behavioral differences among the clones would necessarily arise from developing within those different environments. Next, imagine a genetically diverse human population reared in laboratory cages. All members experience exactly the same environments. Naturally, all differences among these individuals are accounted for by their genetic variability. Notice that in the two fantasies, the organisms all have *both* genes *and* environments for *development*. In human populations, behavioral differences among people arise most often from both genetic and environmental diversity.

Theory and Operations

The theoretical bases of the research we (Scarr and Weinberg, 1977; 1978) reported are both population genetics and principles of socialization. Individual genetic differences in populations result from the evolutionary history of selection and variation and the transmission of genes from one generational pool to the next—from parents to their children. Concepts in population genetics were operationalized by studying families of different degrees of genetic relatedness—parents who transmit *half* of their genes to their children compared with parents who transmit *none* of their genes to their children. From the theoretical perspective of socialization, human parents provide the rearing environment for their children, regardless of their genetic relatedness. In adoption studies, however, the natural parents of adopted-away children transmit only their genes and not their social environments to their offspring. Thus, adoption provides a naturally occurring experiment for studying the effects of genetic variation and differences in socialization.

---

* Direct all communications to: Sandra Scarr; Department of Psychology; Yale University; New Haven, CT 06520.

The study of adoptive families provides an opportunity to estimate the effects of environmental differences on individual behavior. The fact that all adoptive families provide environments in the normal species range means that all adopted children's genotypes have the opportunity to be expressed (with genetically variable results). Adoptive parents transmit no genes to their children, although selective placement creates a very small ($\leq$.01) genetic correlation in adoptive families.

In our (Scarr and Weinberg, 1978) study, the biological families provided similar environmental opportunities for the development of their children. In biological families, however, each parent contributes a random half of his or her genotype to each child.

We (Scarr and Weinberg, 1978) operationalized the concept of genetic IQ differences in the population of upper-middle-class whites in the United States by comparing IQ resemblance among members of the biological and adoptive families drawn from this population. We compared pairs of family members who share about half of their genes with pairs who share virtually none of their genes. That is, by subtracting the IQ correlation of adoptive pairs from that of biological pairs, one subtracts the effects of environmental differences among families, measured by adoptive pair resemblance. The remaining surplus resemblance of biologically related pairs must be due to their greater genetic resemblance.

Because biologically related parents and children share about *half* of their genes, the remaining covariance is an estimate of about *half* of the genetic variance in the population from which the families were drawn. To estimate the *total* effects of genetic differences one must nearly double the difference between the correlations of biological versus adoptive relatives.

To calculate the genetic influence of *both* parents on each child (midparent-child correlation), one does not merely add up the genetic correlation of each parent with the child. Although the explanation of the midparent-child correlation is complex (see Falconer, 1970: 179–81; Munsinger, 1975: 624–5), the value is predicted by the term $\sqrt{2}\, r_{po}$, where r_{po} is the correlation of one parent with the child. The calculation is affected by the assortative mating of parents.

When the same parents have more than one child, the siblings share about half of their genes, because each child receives a random half from each parent. Some siblings will have by chance as few as 40% or as many as 60% shared genotypes.

When parents are not mated at random for a trait, as they are not for IQ, then the genetic correlations in biological families average more than 0.50, a fact that should be used to adjust estimates of genetic differences, as we did in our nonarbitrary multiplier of 1.6. The theoretical reasons for this multiplier are given in the lengthy footnote to Table 6 in the original (Scarr and Weinberg, 1978:685) article.

The midparent-child correlation is the best estimate of that portion of the genetic variance that is additive. Any effects of the parents' particular gene *combinations* are not included in the midparent-child or parent-child correlations. Thus, the differences between parent-child and midparent-child correlations in biological and adoptive families are measures of additive genetic variance or *narrow* heritability. Sibling correlations do include portions of the dominance and other nonadditive genetic effects. The estimate of *broad* heritability from the sibling correlation (0.61) was naturally larger than the estimate of *narrow* heritability from the midparent- or parent-child correlations (0.38–0.50). As we (Scarr and Weinberg, 1978) also indicated, sibs share a common rearing environment, not shared by parents and their children.

Representative Sampling

The population from which the adoptive and biological families were drawn was certainly not the entire United States white population. As we (Scarr and Weinberg, 1978) emphasized repeatedly (on pp. 675, 678, 686, 687, 688, and 689!), these families do not include lower class or economically deprived or abusive, neglectful environments. Nor are the families representative of racial minorities.

We agree with Taylor that the range of SES sampled by families in this (Scarr and Weinberg, 1978) study was necessarily restricted. Although the correlations among parental income, education, and occupational prestige were as high as those reported by studies with more representative samples (see p. 679), all of the children were reared in nondeprived, nonabusive environments. We agree that "the subsequent finding that observed variation in the dependent variable is based on inheritance transmission is not startling, given that environmental variance was compressed, while genetic variance, especially for the adoptive sample, was not" (Taylor, this issue, p. 857). The fact that the SES range of these families includes at least two-thirds of the United States white population in this age cohort makes the results of *some* interest, however.

Even within the range of working-class to upper-middle-class United States white families, the regression equations included only SES, parental IQ, sibling order, and family size measures of "family background," which "are far from perfect indices of children's experiences" (Scarr and Weinberg, 1978:689). But these are a more complete set than most social science studies of "family background" include. Furthermore, with the inclusion of a better set of such measures, "the amount of variance explained by *measured* rather than unmeasured environments might be increased in both kinds of families, but the genetic variance estimated would remain the same" (p. 689). Given the nature of the "family background" measures, "much of the variance is still unexplained, of course!" (p. 689).

Unmeasured Environments

The *unmeasured* effects of differences among family environments are, in fact, estimated as part of the total environmental variance by the adoptive family correlations. That is, the slight IQ resemblance between adoptive parents and adopted children and between adopted siblings necessarily means that 18 years of being reared in *different* family environments (measured and unmeasured) have only slight effects on the children's IQ differences. And it is from the comparison of correlations between biological relatives and correlations between adoptive family members (*not* from the regression equations) that we were able to estimate that 40% to 70% of the adolescents' IQ differences resulted from genetic differences among them (see Scarr and Weinberg, 1978: Tables 5 and 6). By contrast, the median correlation among adoptive parents, children, and siblings shows that only 9% of the IQ variance results from total differences among family environments in this SES range.

Regression Equations

Tables 3 and 4 presented three sets of comparable regression equations for biological and adopted adolescents' IQ scores, in columns 1–6. The first set, columns 1 and 2, gives the family SES variables usually considered in social science studies of "family background." For the adopted children, these measures index differences in only their family environments. For children biologically related to their rearing parents, the same SES variables represent both genetic and environmental transmission, as Taylor notes. "It is precisely the comparison of the equations for the adopted and the biological adolescents that permits an estimate of the relative importance of genetic and environmental differences in these indices of "family background."

In columns 3 and 4, sibling order and family size have been combined with the SES measures, but not in a stepwise fashion, as indicated by the changes in the coefficients on SES. The set of variables in these equations represents indices of both between- and within-family differences in "background."

In columns 5 and 6, the IQ scores of the rearing parents have been included with the previously mentioned variables. Parental IQ has rarely been available in social science studies of family effects on offspring, but is of great interest to behavior genetic studies of family effects. Again, it can be seen by changes in coefficients from the preceding columns that the variables entered the equation simultaneously to produce the best weighted combination of predictors.

The last two equations, columns 7 and 8, are unique to the adopted children, because their rearing parents are not their genetic parents. When some information about the adopted child's genetic background was introduced, the variance explained in the children's IQ scores was doubled over that explained by differences in adoptive rearing environments alone.

As Taylor notes, the measures of differences in rearing *environments* (both between- and within-family indices) accounted for only 7.5% of the adopted adolescents' IQ variance. By contrast, the *same* variables accounted for 31% of the IQ variance among biologically related adolescents, *because* parents in this SES range transmit their intellectual and social status achievements to their offspring by genetic as well as environmental means. Taylor's evaluation of the importance of 31% of the variance should be seen in the context of the expected R^2. Because parents and their children share neither all of their genes—nor all of their experiences—random, individual, genetic variance and experiences unique to individuals should account for at least one third of the IQ variance. We do not know of any studies of "family background" that accounted for more than 35% of the children's IQ variance (see Burks, 1928; Leahy, 1935; Sewell and Hauser, 1975; Duncan et al., 1972; Grilliches and Mason, 1972; Jencks, 1972; Jencks and Brown, 1978).

The "Same" Equations

Taylor (1980) requests comparable regression equations of child IQ on "family background" characteristics for adoptive and biological families, using pairwise deletion. We chose casewise deletion to avoid the suspicion that we had unfairly weighted partial information on employed mothers and natural fathers of the adopted children. Table 1 (herewith) shows comparable sets of equations for biological and adoptive families, including the educational, occupational, and age differences of rearing and genetic parents.

Three regression equations have been calculated, two that contain the same variables for the adopted and biological adolescents (rearing family only) and two that contain the same information—sort of. As Taylor (1980) requested, we show the regression of adolescent IQ on the educational, occupational, and age levels of both the rearing and genetic parents for both groups of adolescents. Unfortunately, as we reported in the (Scarr and Weinberg, 1978) article, there is insufficient information on the natural fathers and on the occupational prestige of employed, rearing mothers. Using pairwise deletion, the regression equations for child IQ on rearing family background are much the same, here, as they were in the original report. The inclusion of information about the natural mothers and fathers of the adopted children greatly increased the R^2 (from .037 to .483), but the latter is an absurd result because of the degrees of freedom. We (1978) were not hiding these results, but, rather, chose not to subject the readers to nonsense.

More informative are the zero-order correlations between adolescents' IQ scores and the background characteristics of the rearing and genetic parents. The adolescent IQ correlations with the educational levels of parents who are genetically related to their children reach statistical significance whether or not those parents had social contact with the children. In the biological families, where the parents are both the rearing and the genetic parents, mothers' and fathers' educational levels are related to their offsprings' IQ scores. Similarly the educational levels of the natural parents of the adopted children are correlated with the IQ scores of children whom they did not rear. In addition, natural mothers' occupational level has a small but statistically reliable coefficient with child IQ. No characteristic of the adoptive parents is significantly correlated with the IQ scores of the adopted adolescents.

Table 1. Standardized Regression Coefficients of Adolescent IQ on Demographic Characteristics of Rearing and Genetic Parents in Biologically Related and Adoptive Families (Pairwise Deletion)

	Adolescents' IQ Scores						
	Biological			Adopted			
			Zero-Order				Zero-Order
	(N)	beta	r	(N)	beta	beta	r
Rearing Parents							
Father's Education	262	.26	.25***	179	−.04	−.53	.07
Mother's Education	262	.17	.28***	188	.02	−.40	.09
Father's Occupation	269	−.12	.10	182	.09	.12	.11
Mother's Occupation	183	.03	.09	114	.11	.57	.12
Father's Age	269	.13	.11	182	.07	−1.25	.12
Mother's Age	269	−.06	.11	191	.05	.92	.13
Genetic Parents							
Father's Education				24		1.15	.43*
Mother's Education				184		.00	.28***
Father's Occupation				<10			
Mother's Occupation				154		.08	.17*
Father's Age				23		.00	.17
Mother's Age				184		.04	−.06
R^2		.11			.037	.483	
Adj. R^2		.08			−.021	−1.238	
F		3.63			0.638	0.281	
df		6, 173			6, 99	10, 3	

* $p<.05$, *** $p<.001$.

We hasten to reiterate that the average level of IQ in the adopted group is higher than would be predicted from the intellectual levels of the natural parents because, as a group, they have been reared by adoptive families above-average in both SES and interest in children. Differences among adoptive-family environments in the range of working- to upper-middle class had negligible effects on differences in the adolescents' IQ scores, however.

Unexpected Results

"This study was designed to assess the cumulative impact of family environments at the end of the child-rearing period" (Scarr and Weinberg, 1978: 676). We expected to find that the differences among family environments would prove to be of *greater* importance by late adolescence than they had been for several samples of young adopted children (Burks, 1928; Leahy, 1935; Scarr and Weinberg, 1977; Horn et al., 1979). Just the opposite was found. In retrospect, our prediction seems unreasonable, because by early to late adolescence, children are subjected to many extra-familial influences (schools, peers, TV, etc.), and they are far freer than younger children to select their own environments. Parental influences are diluted by the more varied mix of adolescent experience.

Our interpretation of the results now emphasizes the kind of "niche building" that is called *active genotype-environment correlation* (Plomin et al., 1977). Different people select different aspects of their environments which they find compatible and then live in them, enlarge them, and deepen them. Part of what they bring to the selection of niches are the genetic differences as to what they are good at, what they enjoy, and what makes them comfortable. Adopted children, who are not genetically related to their parents or to each other, build niches that are correlated with their own genotypes but not with those of their family members. Biologically related children also build niches that are correlated with their own genotypes, but their choices continue to be correlated with those of their genetically related family members. We have found continuing resemblance in interests among the biological (but not among adoptive) family members (Grotevant et al., 1977), personality (Scarr et al., 1980), attitudes (Scarr and Weinberg, 1980), and specific cognitive skills (Carter-Saltzman, 1977). Genetic resemblance and genotype-environment correlations continue to affect IQ resemblance into later adolescence, whereas the effects of common family rearing environments wane.

Calling a Truce between "Camps"

We detailed here our theoretical framework and operational methods, drawn from both social and biological science in the hope that *ASR* readers will return to the original (Scarr and Weinberg, 1978) article to reconsider the evidence. Although it may be distasteful to some social scientists to acknowledge genetic sources of individual and SES differences, it appears to us important to recognize the biosocial nature of human variation.

We strongly oppose a eugenic strategy that interferes with people's civil liberties; we propose that understanding the nature of human variation will facilitate the design and implementation of effective environmental interventions to improve people's lives.

Sandra Scarr
Yale University
Richard A. Weinberg
University of Minnesota

REFERENCES

Burks, B. S.
1928 "The relative influence of nature and nurture upon mental development, a comparative study of foster parent–foster child resemblance and true parent–true child resemblance." Yearbook of the National Society for the Study of Education 27:39–99.

Carter-Saltzman, L.
1976 "The structure and transmission of individual differences in patterns of cognitive ability." Unpublished Ph.D. dissertation, University of Minnesota.

Duncan, O. D., D. L. Featherman, and B. Duncan
1972 Socioeconomic Background and Achievement. New York: Seminar Press.

Falconer, D. S.
1970 Introduction to Quantitative Genetics. New York: Ronald Press.

Grilliches, Z., and W. M. Mason
1972 "Education, income and ability." Journal of Political Economy 80:S74–S103.

Grotevant, H. D., S. Scarr, and R. A. Weinberg
1977 "Patterns of interest similarity in adoptive and biological families." Journal of Personality and Social Psychology 35:667–76.

Horn, J. M., J. C. Loehlin, and L. Willerman
1979 "Intellectual resemblance among adoptive and biological relatives: the Texas adoption project." Behavior Genetics 177–207.

Jencks, C.
1972 Inequality: A Reassessment of the Effects of Family and Schooling in America. New York: Basic Books.

Jencks, C., and M. Brown
1978 "Genes and social stratification." Pp. 169–233 in P. Taubman (ed.), Kinometrics: The Determinants of Economic Success

Within and Between Families. New York: North Holland-Elsevier.

Leahy, A. M.
1935 "Nature-nurture and intelligence." Genetic Psychology Monographs 17:237–308.

Munsinger, H.
1975 "The adopted child's IQ: a critical review." Psychological Bulletin 82:623–59.

Plomin, R., J. C. DeFries, and J. C. Loehlin
1977 "Genotype-environment interaction and correlation in the analysis of human behavior." Psychological Bulletin 84:309–22.

Scarr, S.
1969 "The effects of birthweight on later intelligence." Social Biology 16:249–55.
1979 "Psychology and children: current research and practice. Introduction to the special issue." American Psychologist 34:809–11.

Scarr, S., and J. C. Schwarz
1979 "Report to the Bermuda government on the evaluation of the child development project, October 1978 to August 1979."

Scarr, S., P. L. Webber, R. A. Weinberg, and M. A. Wittig
1980 "Personality resemblance among adolescents and their parents in biologically-related and adoptive families." Unpublished paper. Department of Psychology, Yale University.

Scarr, Sandra, and Richard A. Weinberg
1978 "The influence of 'family background' on intellectual attainment." American Sociological Review 43:674–92.

Scarr, S., and R. A. Weinberg
1977 "Intellectual similarities within families of both adopted and biological children." Intelligence 1:170–91.
1980 "The transmission of authoritarianism in families: genetic resemblance in social-political attitudes?" In S. Scarr (ed.), IQ: Race, Social Class and Individual Differences. Hillsdale, N.J.: Erlbaum (In press).

Scarr, S., and M. L. Williams
1973 "The effects of early stimulation on low-birth-weight infants." Child Development 44:94–101.

Sewell, W. H., and R. M. Hauser and Associates
1975 Education, Occupation and Earnings: Achievement in the Early Career. New York: Academic Press.

Taylor, M.
1980 "The noninfluence of 'family background' on intellectual attainment: a critique of Scarr and Weinberg." American Sociological Review 45:855–8.

Weinberg, R. A.
1979 "Early childhood education and intervention: establishing an American tradition." American Psychologist 34:912–6.

Weinberg, R. A., and S. G. Moore
1975 Evaluation of Educational Programs for Young Children. Washington, D.C.: The Child Development Associate Consortium.

THE EFFECTS OF CROWDING FOUND IN THE TORONTO STUDY: SOME METHODOLOGICAL AND EMPIRICAL QUESTIONS*

(A COMMENT ON BOOTH AND EDWARDS, *ASR*, 1976)

Virtually all investigators agree that the evidence on the effects of overcrowding in the home has suffered from serious conceptual and methodological problems. Recently there have been two major studies, one in Chicago and one in Toronto, aimed specifically at overcoming these limitations. In the Chicago study we concluded that overcrowding had substantial effects (Gove et al., 1979), whereas the investigators of the Toronto study came to exactly the opposite conclusion. For example, in summarizing their results, Booth (1976:104) concludes that crowding has almost no detrimental effects and that "the minute effects of crowding revealed by our analysis indicate that we have a good deal of flexibility in housing people in dense environments . . . without fear that it will result in significant personal disorganization or in detriments in the health of the inhabitants." Given such diametrically opposed conclusions, it is desirable to determine if there is any basis for choosing one over the other. There are at least two important reasons for doing this. First, for purely scientific reasons, it is important to know whether crowding affects humans and, if so, under what condition and in what manner. Second, to the degree that social policy is based on social research, making a mistake on this issue can have undesirable consequences by legitimizing policies whose implementation would either waste a great deal of money (if crowding has no effects) or have a substantial negative impact on the quality of life (if crowding does have serious effects).

As the major paper for the Chicago study is readily available to readers of this journal (Gove et al., 1979), we will focus our comments on the Toronto study. Regarding the Chicago study, we will simply note the following points: (1) by and large, the characteristics of our sample correspond very closely to those of the households in Chicago both in terms of sociodemographic characteristics and in terms of levels of crowding; (2) our analysis is closely tied to the theoretical literature on the effects of crowding, paying particular attention to the subjective experience of crowding as indicated

---

* Direct all correspondence to: Walter R. Gove, Department of Sociology; Vanderbilt University; Nashville, TN 37235.

by an excess of social demands and a lack of privacy; (3) to a substantial extent our measures of the subjective experience of crowding tended to "interpret" the generally significant relationship between our objective measure of crowding, persons per room, and the dependent variables; and (4) the relationships found with crowding generally were linear which, when combined with the characteristics of our sample, suggests that our results can readily be generalized to other populations.

In attempting to determine the reasons for the inconsistencies between our findings and those of the Toronto study, we have found three problem areas in studies done by Booth and his associates: (1) problems with the Toronto sample, in terms of its usefulness in a study of crowding, and the fact Booth and his associates misrepresent the degree to which it is representative of urban populations in general; (2) problems of analysis, specifically the measurement of crowding, the statistical procedures used in the analysis, and the interpretation of the data; and (3) problems concerning inconsistencies between findings and conclusions drawn from the Toronto study and conclusions drawn from studies done by Booth and his associates with other data sets.

I. The Sample

Instead of attempting to get a representative range of levels of crowding, the sample in the Toronto study was specifically drawn to obtain very high levels of household crowding. The 13 census tracts sampled in the Toronto study were, in the words of Booth and Edwards (1976:310), "selected for their potential in yielding a large number of families residing in dwellings in which the number of persons exceeded the number of rooms." These census tracts were also selected because they contained high rates of intact families with dependent children and persons who understood English (Booth, 1976:17). From these census tracts "nearly 17,000 screening interviews [were held which] yielded 862 eligible households. In 560 of these we were able to obtain interviews with one or both parents for a 65 percent completion rate" (Booth and Edwards, 1976:311). The households from which interviews were obtained were, in terms of social class, very homogeneous and were at the lower end of the class continuum. As Booth and Edwards (1976:311) state, "The majority (80 percent) of the household heads were employed in blue-collar occupations, typically ones requiring only modest skills. Only 23 percent had completed high school; however, more than half had completed the eighth grade." Furthermore, the characteristics of the respondents were limited by the fact that they all came from intact white families, the wife was under 45 years of age, at least one child was present, and all the respondents were able to converse in English.

The Toronto sample thus is comprised largely of households with extreme levels of crowding (approximately 50% of the respondents lived in homes with more than one person per room) and is very homogeneous in socioeconomic status and a number of other characteristics. Because the Toronto sample is so unrepresentative, not just of Toronto but of any typical urban population, it is not clear what the general implications of these data are (also, see Baldassare, 1979b). The homogeneity of the sample almost guarantees that there will be little variance and thus most relationships will be very modest.

Booth acknowledges that the sampling procedures were designed so that the number of persons sampled from houses where the number of persons exceeded the number of rooms would be equal to the number of persons sampled from houses where the number of persons did not exceed the number of rooms (Booth, 1976:19). This was apparently done so that there would be an adequate number of crowded persons. No theoretical reason is given for this decision, and the procedures used resulted in a sample comprised almost exclusively of persons residing in relatively crowded households. They claim that the sample is not seriously flawed and that it can be taken to be representative of urban households (Booth, 1976:39). However, Booth himself acknowledges that the average number of persons per room in Toronto is .58 (Booth, 1976:39). Furthermore, data from the United States, a highly urban country, show that Booth's sample is hardly representative of it either. For example, Gove et al. (1979) showed that the census data indicated that the median number of persons per room in 1970 for households in the United States was .54, and for U.S. central cities .51. The percentage over 1.01 persons per room in the United States in 1970 was 8.2%, and for central cities was 8.5%. As Baldassare (1979b) has shown, these percentages had probably declined by the time the Toronto study was conducted. If crowding is to a substantial extent culturally defined (as suggested in Gove et al., 1979; Baldassare, 1979a; Hall, 1966; and Booth, 1976:27), then in the absence of any other criteria, being crowded may be defined as being more crowded than is typical for other persons in one's culture. The validity of this assumption is suggested by the fact that both the Chicago study (Gove et al., 1979) and Baldassare's (1979a) secondary analysis of two national samples found that the relationship with persons per room and various indicators of "pathological" states was linear and there was virtually no evidence of a tipping effect associated with high levels of crowding (i.e., all households with more than one person per room). Treating household crowding as a phenomenon relative to one's particular culture, the less crowded half of Booth's sample contains a substantial number of crowded persons. For example, 52% of Booth's sample are supposedly uncrowded (Booth, 1976:Table 3.1). However, if one assumes that a crowded home is one which contains more persons than the average for households in Toronto in 1971 (.58 persons per room), and then one interpolates using data in Booth's (1976) Table 3.1 (and assumes that households are evenly distributed across the range of .40 to .75 persons per room), one finds that only 10.28% of Booth's sample are actually uncrowded, and 89.72% are crowded. Using Baldassare's (1980) cutoff for overcrowding (.60 persons per room) and the same interpolation technique employed above reveals Booth's sample to be only 11.43% uncrowded, and 88.57% crowded. If one uses .51, the median persons per room for the United States central cities, then Booth's sample is 6.29% uncrowded and 93.71% crowded. By virtually any reasonable standard of crowding, Booth's sample is overwhelmingly crowded, and contains a very small percentage of persons at the low end of the crowding continuum. In short, there is virtually no variance of crowding in the normal range (i.e., most of the variance in crowding is among persons already crowded). If our interpretation is correct then virtually all of his sample reside in crowded households and it is not surprising that he finds crowding has little effect.

Furthermore, homogeneity of the sample with respect to SES—80% blue collar, typically with modest skills (Booth and Edwards, 1976:311)—nearly eliminates any need for controlling for SES in the analysis. As Booth (1976:36) indicates, such a study design avoids the problem of multicollinearity, but it also makes it impossible to look at the interaction of crowding with SES or to control for the effects of SES, since there are virtually no middle class and upper class persons in the sample. Because most of the sample is crowded, this design contains a problem perhaps even more serious than having high levels of multicollinearity among one's independent variables, the problem that one's key independent variable (crowding) may not contain enough variance to test properly for its effects even at the zero-order level. That homogeneity is a problem is suggested by the fact that when we compare the total explained variance in Booth and Edwards (1976:315, 316) with the total explained variance on analogous variables in the Chicago study, we find that the Chicago study is explaining approximately twice as much explained variance as the Toronto study. This is perhaps pointed out most clearly by the two variables which are most similar: Toronto study (1) "times struck children," $r^2 = .06$, (2) played with children, $r^2 = .04$; Chicago study, (1) frequency of physically striking children $r^2 = .12$, (2) being supportive of child $r^2 = .09$.

The sample is even more problematic than it might at first appear. First, no data are presented on the response rate to the screening interviews. Second, because the researchers were sampling both parents in each household, the base for calculating the response rate should be the number of households times 2. If this is done, their actual response rate—Booth (1976:10) calls it a completion rate—after the screening interview with selected respondents was 49.7%, not 65% as stated (Booth:1976:20). Our response rate at this point in the interview procedure was 89.1%. Third, their sample is made complex by the fact that for some households they have two interviews and for other households one. In households for which they have two interviews, the data from the two interviews obviously should not be treated as independent. However, although Booth and Edwards (1976) report their response rate in terms of households, their analysis is limited to interviews, with each interview being treated as a separate independent unit.[1]

---

[1] One particularly troublesome aspect of their data is that, as Booth and Welch (1978) have shown elsewhere, for those households where they were able to obtain interviews with both the husband and wife and analyzed the items they felt would show at least a fairly high degree of consensus, they found that the level of correlation, whether dealing with shared events or similar attitudes, was generally quite modest (see their Table 1). One of their most extreme cases of a weak relationship was on a variable crucial to one of Booth's analyses dealing with crowded and sexual behavior (Edwards and Booth, 1977), where Booth and Welch (1978:28) found that the agreement between husbands and wives was only $r = .15$ on the item "(Did) lack of privacy ever prevent intercourse?" Booth and Welch (1978:30) indicated they "labored hard to find some correlation or predictions of consensus among marital partners . . ." but that ". . . substantive interpretations of these mixed findings (are) not plausible." It is clear that such negative findings raise questions

As noted, the 13 census tracts from which the samples were drawn were purposely chosen because they had the highest level of household crowding of all the census tracts in Toronto. Booth also had other very stringent criteria for his sample. As a consequence only 5.1% of the households screened met his criteria and in only 3.3% of the households screened were interviews actually obtained. As noted, his sample is very different from the general population in that his respondents tend to live in very crowded households, they have little education, household heads almost invariably had menial occupations, all respondents were married and living with their spouses, the wives were all under 45 years of age, and all respondents were white, able to converse in English, and in relatively good physical health.[2] On three occasions, however, Booth asserts the characteristics of the sample "do not limit the generalizability of findings to urban adult family members" (Booth, 1977:647; see also Edwards and Booth, 1976:75; and Booth and Welch, 1978:25 for a similar claim). In contrast, we would view the sample in the Toronto study as so atypical that it is very difficult to draw any conclusions regarding the general effects of crowding or, for that matter, other variables.[3]

II. The Measures of Crowding and the Interpretation of Findings

Most studies have used persons per room as their objective indicator of crowding. Instead, Booth and Edwards developed "objective" and "subjective" scales of crowding. They had no theoretical criteria for choosing the items in their scales. What they did was to run a set of items against their *dependent* variables and then select those items that explained at least 1% of the variance. This method of deriving their scale items is quite unconventional and, as noted by Booth (1976:24), would seem to "stack the deck" in favor of finding effects of crowding. Substantively, many of the items appear unrelated and, although they report the scale items had "a high degree of inter-item reliability for three of the four scales" (p. 812), the average correlations (r) are modest (.24, .26, .31, .02). As Booth and Edwards are concerned with the interitem reliability of their scales, we would suggest they use alpha coefficients rather than their idiosyncratic use of "average inter-item correlations."

Booth and Edwards (1976) state, "The simple crowding variable (persons per room and households per block) explains less variance than the crowding scales, which suggests that as crowding does have significant effects the scales tap them better." This statement is very misleading. For example, a comparison of the standardized regression coefficients in Tables 3 and 4 (Booth and Edwards, 1976:315–16), of persons per room and households per block with the object scales, taking size and direction into account, shows that 15 of the comparisons favor the traditional measures, 9 favor the scales, with 4 being ties. Similarly, a comparison of the number of zero-order relationships for the same variables shows that there are more statistically significant relationships in the predicted direction for the traditional measures (4 out of 32) than for their "objective" scales (2 out of 32). Other analyses show the traditional measures to be more powerful predictors than "subjective" or "objective" scales. The fact that, in combination, the scales explain more variance than the simple crowding measures might be expected because the scales contained both "objective" and "subjective" measures of crowding. Booth and Edwards (1976:317) argue that these scales tap very different phenomena. Thus, one would anticipate that a combination of the scales would explain more variance than measures that deal only with objective crowding. Booth and Edwards used 4 scales and 2 single crowding variables. The scales contain a greater proportion of instances where the relationship is in an unpredicted direction and the variance explained by the "incorrect" relationships comprises part of Booth and Edwards's explained variance for the scales. Thus, if one considers the number of independent variables and the direction of the relationships there is no evidence for be-

---

about survey data in general (a point made by Booth and Welch), but it is even more clear that they raise very serious questions about the quality of data in their particular survey, especially as it was not done by a survey organization with an established record for quality work.

[2] Booth (1977:647) indicates that "of the 302 households in which interviews cannot be obtained, 72 individuals were too ill to be interviewed, 76 refused to be interviewed and 214 did not speak English well enough to complete the interview."

[3] Not only is this sample grossly unrepresentative of any typical urban population in North America, it is also thoroughly unrepresentative of the population in the census tracts from which it was drawn. As Booth (1976:17) notes, "The proportion of households in which people exceeded rooms ranged from 12 to 28 percent of the units in selected tracts," while 48% of the households selected contained more than one person per room. We might further note that if one scans the column in the *City County Data Book* (U.S. Census, 1972) showing the percentage of households in U. S. cities with more than 1.01 persons per room in 1970, such percentages *rarely* exceed 11%.

Table 1. The Comparison of the Proportion of Zero-Order Correlations That Are Statistically Significant (.05) for the Crowding and Social Structural Variables in the Toronto Study

	Social Relations		
	Social Structure		Crowding
	# Sig./Total #		# Sig./Total #
Education	1/16	Persons/room	2/16
Occupation	0/16	Household Objective	2/16
Age	3/16	Household Subjective	3/16
Total	4/48	Total	7/48

From Booth and Edwards (1976:314).

lieving that the scales individually explain more variance than the simple crowding variables. Apparently, the items analyzed to develop their scales were not well chosen. In short, not only are the scales atheoretical and improperly derived, but they appear to be worse predictors than the traditional measures of crowding.

Booth and Edwards (1976:317) state "we might hypothesize that the greatest predictor of the subjective crowding would be objective crowding itself." They find, however, that "the actual correlations between the objective and subjective crowding measure are quite modest." Puzzled by this lack of relationship they eventually conclude that the experience of crowding is largely unrelated to actual crowding. Given that we found persons per room strongly related to the subjective experience of crowding (Gove et al., 1979), we suspect that their general lack of relationship is a consequence of their methodology and the fact that their subjective scales do not focus on the theoretically important dimensions of subjective crowding, demands, and privacy.

As to a very concrete issue, what exactly are Booth's "modest" relationships between objective and subjective crowding? It is hard to tell. In Booth and Edwards (1976:317) they are presented as .12 for females and .19 for males, but in their Table 2 (p. 314) they are presented as .10 for females and .22 for males, and in Booth (1976:32) as .16 for females and .14 for males. This is not the only case of carelessness in the presentation of data. For example, Booth and Edwards (1976:312) state "the relatively high .51 zero-order correlation between the objective crowding scales and the people/room variable and a correlation of a similar magnitude (.49) between the neighborhood objective scale and the household/blocks also attest to the reliability of the study" (Booth, 1976). However, a simple look at Table 2 (p. 314) shows that these are the values for *women only* (for men, they are .36 and .57, respectively).

Fischer et al. (1975:412) have forcefully argued that there should be some theoretical rationale for linking a particular measure of crowding to a particular "pathology." Throughout the papers and the book describing the Toronto study, the effects of neighborhood crowding and household crowding on the pathologies are combined in the discussions. Booth and Edwards (1976:311) admit that their study of the effects of neighborhood crowding on many of the variables they are considering is very unusual and they present no rationale for why one would expect neighborhood crowding to be linked to their dependent variables. Such a link is not obvious. In fact Galle et al. (1972) showed neighborhood crowding was unrelated to "pathological" behavior, a finding that has been subsequently replicated in a number of studies. Why, for example, would one anticipate the item, "Are the neighborhood stores in which you shop too crowded?" to be linked to poor family relations or physical health (particularly after controlling for socioeconomic and ethnic variables)? The fact is that, in their study, neighborhood crowding is never related to the dependent variables, but the lack of a relationship (which is to be expected) is included as part of the finding that crowding has little effect. For example, Booth and Edwards (1976:317) state in their report on family relations,

> out of the 84 possible relationships (six crowding measures, the two sexes, the seven dependent variables) only 11 are significant at the .05 level. Ten of those relationships show crowding to have adverse effects and one shows it to have positive influences.

But 28 of the relationships deal with neighborhood crowding, none of these is statistically significant, and none should have been included in the analysis. Thus, the number of relationships considered should be 56 and not 84; then the effects of crowding would appear to be somewhat stronger than the discussion by Booth and Edwards implies.

Now let us turn to something that borders on being inexplicable and theoretically should be expected to mask the effects of crowding. In the table discussed previously, Booth and Edwards (1976:315–6) present the relationship between six crowding variables and seven dependent variables using Betas produced after controlling for the effects of age, socioeconomic status, education and ethnicity. These controls are, of course, entirely proper. However, in each of their analyses they also control for the effects of the five other crowding indexes. To the extent that one measure of crowding relates to another measure of crowding (i.e., they both measure crowding) Booth and Edwards have eliminated through controls the effects they are attempting to investigate. This tactic, which is virtually guaranteed to show crowding has little effect, is used throughout the analysis of the Toronto data. (Note: Because of the ineffective way crowding has been measured, as we have explained, this totally inappropriate analysis is not as damaging as one would expect on theoretical grounds.)

As is apparent, the Toronto study suffers from serious problems of sampling, measurement, and analysis, and the conclusion drawn by Booth and his associates, that crowding has very little effect, rests on an extremely tenuous base. Since we do not have access to the data, perhaps the best way to evaluate whether the Toronto data demonstrate that crowding is unimportant is to compare their strength with variables typically thought to be important predictors. With the Toronto data, this can be done by comparing the proportion of zero-order correlations that are statistically significant for the crowding and the social stratification variables. These comparisons, using social relations and health as dependent variables, are presented in Table 1. From these data it is obvious that, while few of the relationships are statistically significant, the crowding variables have a higher proportion of significant relationships.[4] Thus, for this very atypical sample, household crowding has at least as much, and possibly more, effect than some of the traditional demographic variables considered by sociologists. In short, a careful scrutiny of the Toronto study shows that the data, in spite of the conclusions drawn by Booth et al., do not conflict seriously with the Chicago study.

III. Other Findings by Booth Concerning Crowding

Anyone reading the work by Booth and Edwards (1976) and the other papers from the Toronto study (Booth, 1976; 1975; Booth and Cowell, 1976; Edwards and Booth, 1977; Johnson and Booth, 1976) would conclude that Booth and his colleagues believe that crowding has very little if any pathogenic effect on human behavior. There is, however, a "second" Booth who strongly argues for the effect of crowding. Booth and Welch (1978) looked at the relationship between area density and civil disorder using 65 cities as the units of analysis; they found "households of crowding and to a less extent, areal density, are linked to the emergence of civil strife" (Welch and Booth, 1974:153), and they emphasize that crowding has a much greater explanatory power than do measures of industrialization, urbanization and discrimination. Subsequently Booth, Welch and Johnson (1976) conducted a study of crowding and urban crime rates and found a small but significant relationship between density and a variety of personal and property crimes. In a later paper, Edwards and Booth (1977:792) referred to this study as evidence for believing that crowding had significant pathogenic effects. And in a more recent paper, Booth, Johnson and Choldin (1977) attempted to evaluate the validity of official crime statistics and victimization crime rates by seeing if a set of independent variables predicted the same pattern for the two sets of rates. Their set of independent variables excluded a variety of variables one might expect to be included (e.g., they have no measure of income) but included both population density and household crowding on the grounds that the evidence indicates there are substantial reasons for believing them to be causally related to crime (Booth et al., 1977:189).

This second Booth, who believes in the pathogenic nature of crowding, seems totally unrelated to the first, who claims that crowding has virtually no effects. References in the articles of the first Booth do not include citations to the second Booth and vice versa. Nowhere is there any indication that Booth sees any contradiction in his results or any need to attempt to reconcile them either theoretically or empirically. This is particularly puzzling in that both sets of works were being produced over the same period of years. Thus, if one is to look at Booth's works in totality (1) one cannot tell if he believes crowding has an effect or not and

---

[4] The direction of these relationships was not taken into account. The one relationship with education is inverse. We did not know what direction to expect with age. Six of the seven relationships with crowding on social relationships show it as having a detrimental effect. We would note, if ethnic status is included, the effects of the social structure would appear to be somewhat stronger. However, "ethnic status" does not measure one's ethnic group but, rather, it answers whether or not one has recently migrated to Canada—which would appear to involve a whole set of problems dealing with adapting to a foreign culture.

(2) his work, being almost totally contradictory, does little to settle this empirical question. It seems to us that, even if an investigator cannot reconcile contradictory findings, he or she is at least under a strong obligation not to obscure the fact that the findings are contradictory. We feel it would be very useful if Booth and his colleagues would either attempt to reconcile his contradictory findings and stated conclusions or provide the reader with some guidelines on how to interpret their contradictory results.

We would note the most recent publication by Booth (Duvall and Booth, 1978) does not fit into either of these patterns. That paper has to do with aspects of the housing environment, one of which has to do with "perceived space and privacy problems." All the items in the perceived space scale were drawn directly from Booth's subjective crowding scale, and lack of privacy has been consistently treated throughout the literature as a consequence of crowding. Duvall and Booth find these items are related to poor mental health. Thus, in this case the Toronto data appear to be related to crowding; however, Duvall and Booth (1978:fn. 3) deny this, because in their words: "This is not a study of the effects of crowded conditions."

In summary, whether one looks simply at Booth and Edwards or at the whole set of published works by Booth and his associates on crowding, no reason is evident for concluding that crowding does not have important effects.

Walter R. Gove
Vanderbilt University
Michael Hughes
Virginia Polytechnic Institute and State University

REFERENCES

Baldassare, Mark
1980 "The effects of household crowding on subgroups: a research note." Mimeographed. Department of Sociology, Columbia University.
1979a Residential Crowding in Urban America. Berkeley: University of California Press.
1979b "Review of urban crowding and its consequences." Contemporary Sociology 8:466–7.

Booth, Alan
1977 "Wife's employment and husband's stress: a replication and refutation." Journal of Marriage and the Family 39:645–50.
1976 Urban Crowding and Its Consequences. New York: Praeger.
1975 "Crowding and social participation." Mimeographed. Department of Sociology, University of Nebraska.

Booth, Alan and J. Cowell
1976 "The effects of crowding upon health." Journal of Health and Social Behavior 17:204–20.

Booth, Alan and J. N. Edwards
1976 "Crowding and family relations." American Sociological Review 42:308–21.

Booth, Alan, David Johnson and Harvey Choldin
1977 "Correlates of city crime rates: victimization official statistics." Social Problems 25:187–97.

Booth, Alan and Susan Welch
1978 "Spousal consensus and its correlates." Journal of Marriage and the Family 40:23–34.

Booth, Alan, S. Welch and D. Johnson
1976 "Crowding and urban crime rates." Urban Affairs Quarterly 2:291–307.

Duvall, Donna and Alan Booth
1978 "The housing environment and women's health." Journal of Health and Social Behavior 19:410–41.

Edwards, John and Alan Booth
1976 "Sexual behavior in and out of marriage: an assessment of correlates." Journal of Marriage and the Family 38:73–81.
1977 "Crowding and human sexual behavior." Social Forces 55:791–808.

Fischer, Claude, M. Baldassare and R. Ofshe
1975 "Crowding studies and urban life: a critical review." Journal of the American Institute of Planners 41, 6, November.

Galle, Omer, Walter Gove and J. Miller McPherson
1972 "Population density and pathology: What are the relationships for man?" Science 170:23–30.

Gove, Walter, Michael Hughes and Omer Galle
1979 "Overcrowding in the home: an empirical investigation of its possible pathological consequences." American Sociological Review 44:59–80.

Hall, E.
1966 The Hidden Dimension. New York: Doubleday.

Johnson, David and Alan Booth
1976 "Crowding and human reproduction." Milbank Memorial Fund Quarterly 54:321–37.

United States Census
1972 City County Data Book. U.S. Government Printing Office, Washington, D.C.

Welch, S. and A. Booth
1974 "Crowding as a factor in political aggression: theoretical aspects and an analysis of some cross sectional data." Social Science Information 13:151–62.

REPLY TO GOVE AND HUGHES*

Contrary to the Gove-Hughes allegation, there is only one Alan Booth, and that is the Alan Booth who, along with his colleagues, has pursued many avenues in an attempt to learn how crowding might influence pathology.

---

* Address communications to: Alan Booth; Dept. of Sociology; University of Nebraska; Lincoln, NE 68588.

Gove and Hughes fail to distinguish between those studies using aggregate data and those relying on individual-level information. Like many investigators (including Gove), we explored the effects of crowding using cities (and even nations) as the units of analysis before individual-level data became available. As we point out (Booth and Edwards, 1976:309), the use of aggregate data may be the source of some of the conflicting findings that characterized early crowding research. All of the later studies using individual-level data have consistently confirmed the findings of the Toronto study. And, as we point out elsewhere in this issue, so does the work of Gove and his colleagues (1979).

Gove and Hughes, nevertheless, raise several issues regarding the Toronto investigation. Specifically, they claim that the Toronto study has the following limitations: (1) there is little variation in the crowding variables; (2) the sample is homogenous with respect to a variety of background variables; (3) the interview completion rate is lower than reported; (4) the crowding scales are atheoretical and improperly derived; (5) conventional crowding measures are superior to the scales used; (6) the analysis is flawed by inconsistencies in the correlations between the same variables and the improper use of controls; and (7) the crowding measures have stronger effects than the background variables. Not only are their assessments incorrect, but the purported limitations have little relevance for the size of the crowding effects found.

The keystone of Gove and Hughes's criticism is their claim that the Toronto sample is so homogenous with respect to household crowding that the effects of this variable are greatly underestimated. This would be correct if either of two things were evident: first, that the effect of crowding on pathology conformed to a "tipping point" rather than a linear model (Figure 1A illustrates the linear model, while Figure 1B shows the "tipping point" model in which the effect occurs at a certain point on the crowding continuum); or second, that the disproportionate sample used in the Toronto study explains less variance in pathology than when it is weighted to return the sample to proportionality. Neither is true. Gove and Hughes assert, and we agree, that the effect of people-per-room on assessment of pathology is linear across the range of the crowding measure with no evidence of a "tipping point." While the Toronto study crowding measures fall at a higher point on the continuum (range b in Figure 1A), this would make a difference only if most of our sample was above the "tipping point." As neither the Chicago data, the Toronto study, nor any other analyses we know of support the "tipping point" model, we can discount this factor as a source of bias in the Toronto study. The only other way this could be an important consideration is if the variance caused by people-per-room was diminished by the sampling procedure. The disproportionate sampling procedure used in the Toronto study did indeed affect the distribution of the people-per-room measure, since the sample was drawn to ensure that half the sample came from households in which there was more than one person-per-room. Comparison with the variance in people-per-room computed with the sample weighted to proportionality, however, found that our sampling procedure increased—not decreased—the variance in people-per-room. We can perform a simple test of the consequences of the disproportionate sampling for the relationship between crowding and pathology by comparing the amount of variance crowding explains in a representative linear dependent measure computed for the disproportionate sample with the amount explained when weights are used to return the sample to proportionality. The explained variance with the disproportionate sample used was 3.4%. Weighted to proportionality it decreased to 2.8%. If we had sampled proportionately and thus moved our measures toward the national median, it would have decreased the variance in our crowding measure and reduced, not increased, the effects.

The Gove-Hughes argument that our estimates of the effect of crowding on pathology are reduced because our sample is homogenous with respect to the background variables is equally vacuous. If our interest was to show the relationship between, say, education or occupation and pathology, such a statement would have merit. However, our intent, as was the case with the Gove et al. (1979) study, was to find the effect of crowding on pathology net of the spurious effects introduced by the background measures. The relative heterogeneity of the background variables is irrelevant, as they are controlled in the analyses of both the Toronto and Chicago data. Gove and Hughes's "illustration" of the problem, where they compare similar measures in both studies, can be easily turned around to verify the validity of our procedures. They show the total variance explained by a combination of crowding and background variables for two measures that are similar in the studies and conclude that the Chicago study is explaining "twice as much variance as the Toronto study." While this is true for total explained variance, the amount uniquely contributed by the crowding measures, beyond that explained by the background variables, is similar. After removing the effects of the controls, the crowding measures account for around 1% of the variance in each of the variables in the Chicago study and they explain 0% and 3% in the Toronto investigation. Thus, the Chicago study does no better at finding an effect net of the background variables. The only consequence of the greater heterogeneity in the Chicago study is that the background variables do explain more variance in pathology. The impact of crowding is unaffected.

The interview completion rate charged to be inaccurate was calculated by dividing the number of households where at least one interview was obtained by the total number of eligible households. We did not obtain full interviews from both husband and wife in all cases; therefore, if one based a completion rate on the number of people, it would be somewhat lower. The household is, however, the more suitable unit upon which to base the completion rate calculation for two reasons: (1) the environment and circumstances shared by the husband and wife are common ones; and moreover (2) the analysis of children was based on information provided by either the husband or the wife. More importantly, the completion rate is not a factor that would bias the results because our analysis shows that the characteristics of households from which interviews were obtained did not differ from those where they were not obtained. In short, the Gove and Hughes claim has no bearing on the results of the study.

Likewise, contrary to their critique, the development of the crowding scales was neither atheoretical nor improper. While the data collected allowed us to test a number of theories, the predominant one guiding the study was a stress model hypothesizing that space limitations foster disturbances in family relations. If the creation of the scales were atheoretical, we would have been much less selective in our choice of measures. As it was, we chose only those most reflecting space limitations. We included a refinement of the people-per-room measure—the number of hours the respondent was at home and awake during which the number of people was equal to or exceeded the number of rooms. The procedure used in developing the scales was similar to a canonical scaling model (Van de Geer, 1971:83–92) in which weights for items are developed on the basis of their association with a set of dependent variables, standard procedure for selecting a set of independent items that maximize the relation with a set of dependent measures. The procedure used to derive the scales was, therefore, not improper.

Gove and Hughes attempt to show that our scales are no better than conventional measures of crowding. In comparisons of the standardized regression coefficients between crowding and pathology they find many of the traditional measures to have higher coefficients. However, the average number of points by which the coefficients differ is only .06, with the vast majority equal to or below this number. It is difficult to attach much meaning to differences so small. Regardless of which measures one might consider superior, this does not alter the size of the effect of crowding on pathology as assessed by either measure. The effects, like those found in the Chicago study, are small or simply nonexistent.

The allegation of inconsistencies in the correlations reported in our study is another in-

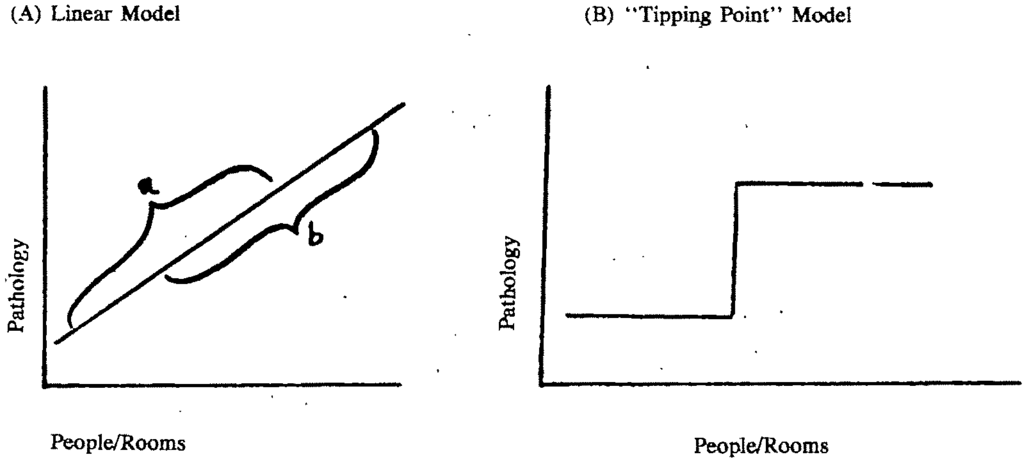


Figure 1. Linear and "Tipping Point" Models Relating Household Crowding and Pathology

stance where Gove and Hughes are "grasping for straws." The correlations showing the relation between objective and subjective crowding reported in Table 2 of our (1976) paper were, as stated, generated by a listwise procedure. The correlations presented in the footnote on page 312 of that paper were not. The listwise procedure accounts for the slight differences. Again, this claim has no bearing on the size of the effects observed or the conclusions one can draw from them. Neither does the fact that all of the crowding indices were included in the regression equations simultaneously have a bearing on these matters. The results are not affected because the scales are not highly correlated as the critics themselves note.

Finally, we are struck by the inconsistency of Gove and Hughes's arguments when they point to the fact that our crowding measures have stronger effects than the background measures. On the other hand, they use the lower variance of some of our background variables as a criticism of our study. On the other, they try to use the consequence of the lower variance (lower zero-order correlations of background variables with pathology measures) as an indication that we have stronger crowding effects than reported. Not only are these arguments inconsistent, they are unfathomable.

Gove and Hughes write: ". . . a careful scrutiny of the Toronto study shows that the data . . . do not conflict seriously with the Chicago study." We thoroughly agree. As we have just shown, nothing Gove and Hughes suggest in their comments on the Toronto study successfully challenges the size of the effects found. In instances where the crowding measures used in the Toronto and Chicago studies are similar, the effects are comparable. If the magnitude of the effects are similar, why do the investigators in these two studies reach such different conclusions? As we point out elsewhere in this issue, it is when Gove and his colleagues use a measure of crowding that is neither conceptually nor analytically distinct from their dependent variables that larger effects are detected. Such contamination, we surmise, led Gove and his colleagues to their misguided conclusions. We can only hope that their exaggerated views of the magnitude of crowding effects will not mislead public policy.

Alan Booth
University of Nebraska–Lincoln
David R. Johnson
University of Nebraska–Lincoln
John N. Edwards
Virginia Polytechnic Institute and State University

REFERENCES

Booth, A. and J. Edwards
1976 "Crowding and family relations." American Sociological Review 41:308–21.

Gove, W., M. Hughes and O. Galle
1979 "Overcrowding in the home: an empirical investigation of its possible pathological consequences." American Sociological Review 44:59–80.

Van de Geer, J. P.
1971 Introduction to Multivariate Analysis for the Social Sciences. San Francisco: W. H. Freeman and Company.

IN PURSUIT OF PATHOLOGY: THE EFFECTS OF HUMAN CROWDING*

(COMMENT ON GOVE, HUGHES, AND GALLE, *ASR*, FEBRUARY, 1979)

All too few field studies exist concerning the effect of household crowding in urban areas. Those using statistical techniques controlling for possible confounding factors, such as socioeconomic status, are even more rare. Therefore, the addition of the report by Gove, Hughes and Galle (1979) is worthy of note. Drawing on interview data from a large (2,035) sample of Chicago residents, the authors examine the effects of household crowding on a wide range of dependent variables, purportedly indicating its adverse effects. Among the pathological consequences catalogued by the authors are poor mental health, withdrawal, a lack of effective planning, deteriorated physical well-being, poor familial relations and child care. In sum, the authors conclude (p. 79), ". . . it is time to turn away from the question of whether it [household crowding] ever has effects to the study of the factors which magnify or minimize its effects." Obviously, such a conclusion has broad implications for housing and land-use policy.

A careful review of the Gove et al. (1979) report reveals, however, some severe problems, making their conclusion premature, if not misguided, and, in a more general sense, misleading.[1] Close examination of their data suggests a contrary conclusion, one quite similar to that of field studies conducted in Hong Kong (Mitchell, 1971) and Toronto (Booth, 1976; Booth and Cowell, 1976; Booth and Edwards, 1976; Booth and Johnson, 1976; Edwards and Booth, 1977; Johnson and Booth, 1975).

---

* Address all correspondence to: Alan Booth; Sociology Department; University of Nebraska; Lincoln, NE 68588.

[1] Part of the authors' justification for their research is that most prior studies are inconclusive. In particular, they suggest (Gove et al., 1979:60) that the sample used in the Toronto study (Booth, 1976) is ". . . so atypical and homogenous and the measurement and analysis so flawed that it is difficult to draw any conclusions about the effect of crowding. . . ." As these are undocumented assertions, they will not be addressed here but will be refuted in a suitable forum where the charges are developed and fully specified.

The difficulties in accepting their conclusion are several but begin most fundamentally with the conceptualization and measurement of crowding. The experiential crowding variables used (lack of privacy and felt demands) are, or should be, conceptually and analytically distinct from the measures of social pathology. Yet no evidence of such a distinction is reported by Gove et al. (1979). They claim that the beliefs that one does not have sufficient privacy and that others are too demanding are crowding measures. However, they may well be measures of poor mental health. How then are we to interpret the relationship of insufficient privacy and felt demands to irritation, alienation, a lack of positive effect, low self-esteem, and unhappiness—the dependent measures of mental health used by the investigators? A person who feels unloved, irritated, alienated, self-deprecatory and unhappy could well manifest these feelings in a positive response to the question: "At home, does it seem as if you can never be by yourself?" (one of the privacy measures); or the item: "Does it seem as if others are always making demands on you?" (one of the demand measures). In short, there is a strong possibility that the causal direction is the reverse of that proposed. Poor mental health or deteriorated social relations (the indicators that Gove and his colleagues (1979) use as dependent variables) could very well lead individuals to feel that people are altogether too demanding and not granting them sufficient privacy (the factors they use to assess crowding) or, just as likely, the feeling that people are too demanding may be another indication of poor mental health.

If the experiential measures of crowding are indicators of poor mental health, the basic regression model used by Gove et al. (1979) is misspecified and the estimates biased. A model we think better fitted to the Chicago study data along with the model Gove and his colleagues present in their paper is shown in Figure 1. While we cannot fully estimate our alternative model, sufficient information is available to show that our model is better fitted to the data than is theirs.

Using the basic theorem of path analysis, we can trace through the paths to estimate the expected values of the correlations between crowding and pathology.

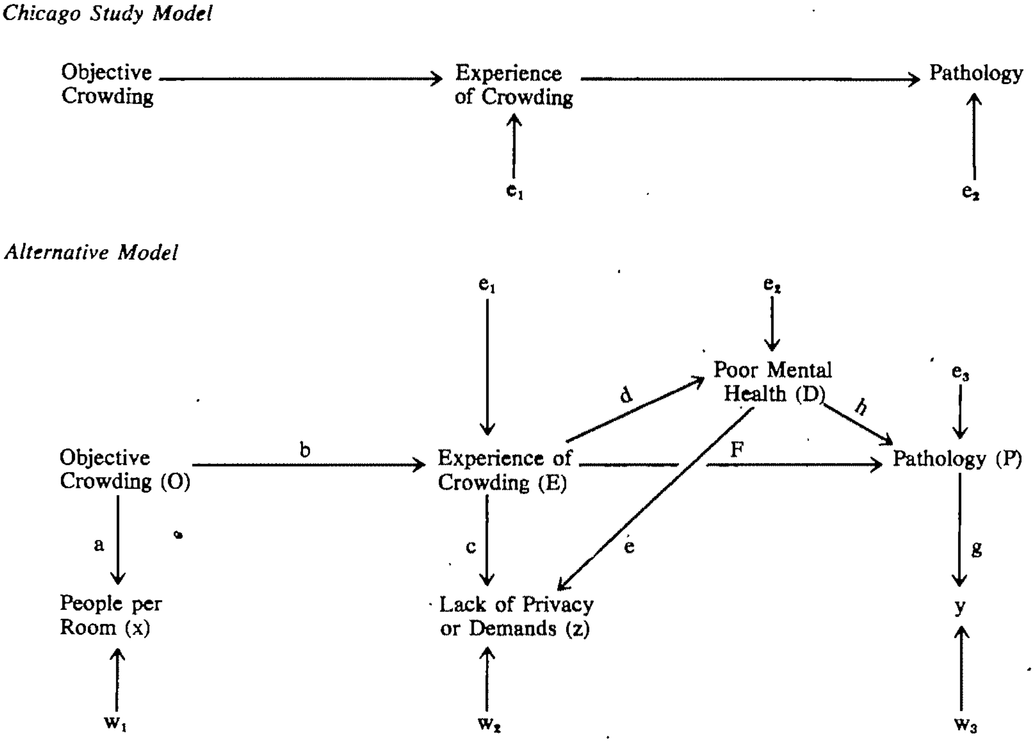


Figure 1. Two Alternative Models of Crowding Effects

$$r_{xy} = abfg + abdhg = abg(f + dh)$$
$$r_{zy} = cdhg + cfg + ehg = cg(f + dh) + ehg$$

The magnitude of path e indicates the degree of bias in the measure of crowding experience. If no bias were present (e = 0), then the term ehg in the equation for r_{zy} would also be equal to zero. It can be shown that, if no bias is present, then the ratio of the two correlations is

$$\frac{r_{xy}}{r_{zy}} = \frac{abg(f + dh)}{cg(f + dh)} = \frac{ab}{c}$$

If poor mental health does not bias the measure of crowding experience, then the ratio of the two correlations should always be the same (within sampling error), regardless of the values of f or dh, provided the model is correctly specified. However, if, as we hypothesize, e is nonzero and positive, then as the size of path h (poor mental health on the measure of pathology) increases, it will increase r_{zy} but have no effect on r_{xy}. Thus, the ratio ab/c decreases as the relationship between pathology and poor mental health increases. For each measure of crowding and pathology we computed ratios using the beta coefficients generated from the regressions which included the control variables but not the crowding measures.

Excluding three ratios with negative values (their interpretation is ambiguous), the smallest ratios from both the demands and privacy variables analyses were those for the following pathology measures: involved in arguments; not enough sleep; catch a bug from others; not cared for by others; psychiatric symptoms; nervous breakdown; and get along with others—all of them variables closely tied to the conceptualization of poor mental health. The average size of the ratios was .22. The largest ratios were those for the indicators of positive marital relations; child care; alienation; mental health balance; overall health; and physical violence—variables that cannot be readily viewed as indicators of poor mental health. The average size of these ratios was 1.63. The low ratios stemming from the way the mental health measures were analyzed indicate that the experiential indicators of crowding are also measuring poor health. Therefore, it appears that the experiential measures cannot be used to assess the effect of crowding on pathology when using the mode of analysis (multiple regression) selected by Gove and his colleagues (1979).

It could be argued, however, that the ratios are varying because of the presence of direct paths from people-per-room to the pathology measures. Such paths would explain some of the variation in the ratios. In eleven of the thirty analyses, people-per-room does have a significant direct path to pathology measures. However, seven of the paths (the largest ones) are for the "care of children" pathology indicators, many of which (as we show below) are indicators of crowding experience rather than pathology. When these are removed from consideration, the effect of people-per-room is either not significant or modest (Betas of .12 or less). Therefore, it is not the presence of a direct effect of people-per-room that is causing the ratios to vary. Rather, it is the magnitude of the effects of the experiential variables that affect the ratios, and this variation is created by the association of the experiential indices with poor mental health. Thus, bias in the experiential measures makes substantive interpretation of the effects of crowding on pathology meaningless when multiple regression is used.[2]

The measurement problems unfortunately do not stop here. While people-per-room is a commonly used measure of crowding, it has basic conceptual and methodological defects. Conceptually, it means that all members of the household require the same number of rooms. That is, four persons in a four-room household are as crowded as eight people in an eight-room dwelling, and four unrelated adults need the same number of rooms as a couple with two small children. This is untenable on the face of it. Small children do not need the same number of rooms as adults, and a married couple does not have need of the same number of rooms as two unrelated adults. Further, room needs do not increase proportionally to the people gained. As a case in point, a three-room household may meet the space needs of a married couple with no children. The addition of four children is not likely to increase the space needs to nine rooms (the number of rooms needed to keep people-per-room constant). Because household composition influences space demands, people-per-room is limited as a measure of space demands.

Ratio variables (such as people-per-room) create methodological pitfalls in interpreting regression analyses (Fuguitt and Lieberson, 1974; Schuessler, 1973). Gove and his collaborators indicate that people-per-room has independent effects on selected mental health measures above and beyond what is explained by the number of people (including number of children) and the number of rooms, suggesting that the three variables measure somewhat different things. Curiously, after demonstrating that people-per-room is highly correlated with number of people in the household (a variable for which it is reasonable to assume a correlation with many of the same dependent variables), the authors do not control for number of people (or number of children) in their analysis. Perhaps the findings would have been very different had they done so.

---

[2] We would have preferred to compare the two models directly, using LISREL (Jöreskog and Sörbom, 1978) rather than multiple regression, but the authors refused to make the correlations matrix available to us. The indirect comparisons are, however, sufficient to challenge the adequacy of the measures used to tap crowding experience.

Not incidentally, the number of people in the household has conceptual and analytical implications for the lack of privacy and demands, the two other independent variables. As the number of people increases arithmetically, the complexity of interaction increases as an exponential function. Hence, the degree to which number of people is related to the experiential variables is an important consideration. Unfortunately, the analysis is presented in a way that does not permit us to evaluate the relations between the experiential variables and the components of people-per-room. How much variation in the experiential variables does number-of-people explain and how much variance does people-per-room explain? The omission of these variables from the tables render conclusions about the effects of the independent variables questionable at best. It is a puzzle why the investigators did not use less problematic measures such as rooms deficit (Booth, 1976:24–32), where crowding is derived from the average space needs of different types of households. The authors clearly had the data to do so.

One final measurement problem to be noted is that several dependent variables, those having to do with the care of children, are not dependent variables at all. They simply tap another dimension of crowding experience. One item has to do with whether children have a place to study (clearly a function of space) and another with whether children have difficulty getting away from others (related to both space and people). It is not surprising that the independent variables show substantial relationships with these items. It is noteworthy that the crowding variables have a negligible effect on the child care items having no spatial elements (physically punish child and supportive of child). Coupled with the failure to control for number of children in this analysis, it is indeed questionable if crowding can be viewed as an important variable affecting the care and behavior of children.

This leads us to another major problem, the deficiencies in interpretation. The investigators find a general pattern of low positive relationships between crowding and pathology, concluding that the adverse results are important consequences of crowding. They contend that the amount of variance explained by crowding is comparable to the size of effects found in other social science research using individual level data. We disagree. The mean total variance explained by the crowding variables for the 30 dependent variables shown is .042%. Given the questionable nature of the experiential variables, it is important to estimate the variance explained by people-per-room alone. Though not given in the report, we judge the mean variance explained by people-per-room to be approximately one-third that explained by all of the crowding variables, or .014%. This may be charitable since the average correlation between people-per-room and the dependent variables is .128, while for felt demands it is .174, and for lack of privacy it is .151. Explained variance of slightly more than 1% is very comparable to the amount of variance explained by crowding in the more than 30 health, family, and social variables explored in the Toronto study (Booth, 1976) where the mean explained variance is .023% for men and .020% for women.

Because means are sometimes deceptive, we include a tabulation (Table 1) of the explained variance broken down by percent for the dependent variables in the two studies. It is evident that in the Chicago study the combined measures explain just slightly more variance (approximately 2%) than the crowding measures in the Toronto study. Given the dubious value of the experiential variables, a comparison of the effects of people-per-room in the Chicago study with the variance explained by the crowding scales used in the Toronto study is in order. In this instance, people-per-room explains slightly less variance than the scales in the Toronto study. Why, then, the completely different interpretation of the results?

The fundamental question is: How large does an effect have to be before it may be regarded as important? Both of the other field investigations mentioned earlier (Toronto and Hong Kong) revealed slightly more adverse consequences than would be expected by chance. The large proportion of statistically significant relations found by Gove et al. (1979) may be accounted for in part by the size of the sample, which is three times the size of the one in the Toronto study (Booth, 1976). The larger Chicago sample is clearly more sensitive to detecting small effects.

How important are a few points difference on a list of psychiatric symptoms or alienation scale, or a few percentage points difference on

Table 1. Percentage of Variance Explained in Dependent Variables by the Chicago and Toronto Studies

	1–2 Percent	3–4 Percent	5–7 Percent	8+ Percent	Number of Variables
Chicago Study					
Number of variables for experential and people-per-room crowding variables	9	11	8	2	30
Number of variables for people-per-room variable	28	—	2	—	30
Toronto Study					
Number of variables for combined crowding measures					
Males	19	8	4	—	31
Females	22	7	3	—	32

an item where the respondent judges his health or the closeness of his/her spouse? As Gove and his collaborators do not present us with the unstandardized regression coefficients, or even a matrix from which they may be reproduced, it is not possible to estimate the exact effect of a change of one point on a privacy scale or of a room on the people-per-room index. The diminutive variance explained would suggest that the effects are small and probably similar to those found in other field studies. In terms of the overall well-being of the individual, these are insignificant and far removed from the impact of such things as losing a job, dropping out of school, becoming disabled, or being the victim of a crime. The type of effects found in the Chicago study are small influences in a world in which every human is subject to numerous adverse and positive influences each day—stresses which most individuals are fully equipped to cope with as part of everyday life.

In our view, the Gove et al. (1979) results are not unlike those reported from comparable studies. However, the deficiencies in measurement and interpretation are compounded by lesser, though significant, problems in sampling and analysis. The Chicago sample is not a representative probability sample. Tracts were selected on the basis of their chance of yielding crowded households, blacks and certain socioeconomic levels. According to the investigators this was done to reduce the dependence of the crowding measures on the background variables and to maximize the range of variation in the crowding measure. Such manipulations of the sample may have little effect on the measures of central tendency, as is shown for their sample, but they do affect measures of dispersion and the correlations among the measures. To use beta coefficients and contributions to explained variance as population parameter estimates, when the sampling design tampers with the standard deviations and the correlations among the measures, is a flaw in the analysis. This is a particularly serious flaw when the authors proceed to conclude: (1) that the crowding measures have substantial effects independent of the control variables; and (2) that the crowding measures often explain more variance in the dependent measures than do the controls. Such conclusions may be the result of the sampling design, and any substantive interpretations drawn from them are likely to be distorted (Blalock, 1967).

The report is also marred by crucial omissions. Reliability figures for the two experiential scales are not given. The investigators present evidence to indicate that lack of privacy is significantly related to race and that felt demands is associated with the sex of respondent (Gove et al., 1979:65). The data suggest there may be important interaction effects. These are never explored. Typically, researchers include in the presentation of their findings a zero-order correlation matrix of the relations between key variables. This allows the reader to examine the data for alternative explanations and to check the investigators' conclusions. Means and standard deviations are usually presented so that the reader can examine the distributional properties of the variables and decide whether they are in sufficient conformity to the requirements of the statistical procedures used. None of this is available to the reader of the article by Gove and his associates.

On the basis of our review of the Chicago crowding study, we suggest that:

1. the lack of privacy and excessive demands from others may be signs of psychopathology as much as measures of crowding;
2. people-per-room as a measure is not uniformly suitable for different types of households and should be used only when there is some control for the number of people in the households;
3. some of the dependent variables which

were used contain elements of the independent variable;

4. the sampling frame may distort the standardized regression coefficients, thus leading to erroneous conclusions;
5. significant amounts of crucial analyses are omitted from the presentation; and
6. the findings are similar to those of other field studies showing crowding to have negligible effects on pathology.

The examination of the findings of Gove and his collaborators (1979), in the light of the problems in their measures, interpretation, sample and analysis, leads to the conclusion that household crowding has small effects on the health and well-being of families in some cases. Rather than showing large effects, rivaling the impact of more traditional demographic variables, our interpretation of the findings would suggest they are remarkably similar to those of previous research.[3] The findings support, not reject, the contention that household crowding is a variable of minimal importance in explaining pathology, at least in modern industrial society. On the basis of findings to date, pathology has little to do with crowding.

Alan Booth
University of Nebraska–Lincoln
David R. Johnson
University of Nebraska–Lincoln
John N. Edwards
Virginia Polytechnic Institute and State University

REFERENCES

Blalock, H.
1967 "Causal inference in natural experiments: some complications in matching designs." Sociometry 37:300–15.

Booth, A.
1976 Urban Crowding and Its Consequences. New York: Praeger.

Booth, A., and J. Cowell
1976 "The effects of crowding upon health." Journal of Health and Social Behavior 17:204–20.

Booth, A., and J. N. Edwards
1976 "Crowding and family relations." American Sociological Review 41:308–21.

Booth, A., and D. R. Johnson
1976 "Crowding and human reproduction." Health and Society (Summer):321–37.

Edwards, J. N., and A. Booth
1977 "Crowding and human sexual behavior." Social Forces 55:791–808.

Fuguitt, G., and S. Lieberson
1974 "Correlations of ratios or differences scores having common terms." Pp. 128–44 in H. Costner (ed.), Sociological Methodological Methodology. San Francisco: Jossey-Bass.

Gove, Walter R., Michael Hughes, and Omer R. Galle
1979 "Overcrowding in the home: an empirical investigation of its possible pathological consequences." American Sociological Review 44:59–80.

Johnson, D. R., and A. Booth
1975 "The effect of crowding on child health and development." American Behavioral Scientist 18:736–47.

Jöreskog, Karl G., and Dag Sörbom
1978 LISREL IV: Analysis of Linear Structural Relationships by the Method of Maximum Likelihood. Chicago: National Educational Resources, Inc.

Mitchell, R.
1971 "Some social implications of high density housing." American Sociological Review 36:18–29.

Schuessler, K.
1973 "Ratio variables and path models." Chapter 10 in A. Goldberger and O. Duncan (eds.), Structural Equation Models in Social Sciences. New York: Seminar Press.

IN PURSUIT OF PRECONCEPTIONS: A REPLY TO THE CLAIM OF BOOTH AND HIS COLLEAGUES THAT HOUSEHOLD CROWDING IS NOT AN IMPORTANT VARIABLE*

(REPLY TO BOOTH, JOHNSON, AND EDWARDS)

A casual reading of Booth et al.'s (1980) comments would seem to indicate that there are serious problems with the analysis and conclusions in the study by Gove et al. (1979) on the effects of household crowding. A careful reading suggests that on some points Booth et al. have made very serious mistakes, and on other points they have raised serious issues that need to be addressed. We are happy to have the opportunity to demonstrate that the issues they raise do not, in fact, impose serious problems with our analysis or conclusions.

(1) Booth et al. (1980) state that lack of pri-

---

[3] We had similar expectations concerning the pathological effects of crowding at the initiation of the Toronto project. Contrary to our beliefs, we found that crowding effects simply were not there in the magnitude we anticipated. In a like vein, many of the reviewers of the almost two dozen reports on the Toronto project responded, "I see the results, but I still don't believe them." A lesson in the sociology of knowledge obviously could be, and needs to be, drawn here.

* Address all correspondence to: Walter R. Gove; Department of Sociology; Vanderbilt University; Nashville, TN 37235.

vacy and overstimulation by social demands "should be conceptually and analytically distinct from the measures of social pathology. Yet no evidence of such a distinction is reported by Gove et al." It is true that we made no attempt to justify the conceptual and analytic distinction between overstimulation by social demands and lack of privacy and our various measures of mental health, poor social relations, poor physical health and poor child care because we assumed that at the conceptual level the distinction between these concepts is obvious. However, it is clear from Booth et al.'s discussion that what they are really concerned with is that our operational definitions of lack of privacy and overload of social demands overlap with some of our dependent measures, particularly mental health, that this seriously muddles our analysis and may have led us to an erroneous conclusion. The latter issue we see as a serious one, and we will devote the latter part of our rejoinder to demonstrating that a seemingly plausible problem is, in fact, not problematic.

(2) Booth et al. (1980) present what they take to be our model, state that it is misspecified, and then propose a path model which they believe is the correct model. The claim that the model presented in their Figure 1 is our model is a misrepresentation of our position. We very clearly state (Gove et al., 1979:62) that this model is oversimplified and is only a very general model of the way crowding affects pathologies. Furthermore, we strongly imply that the pattern of relationships among the three indicators of crowding will vary across pathologies. Perhaps more to the point, we did not present and/nor attempt to test a path model of crowding at all. Instead, our paper is an attempt to answer two very simple, related questions: First, does crowding explain a significant amount of variance in indicators of social pathology over and above that explained by traditional demographic indicators? Second, do the crowding variables generally explain as much variance in indicators of pathology as do the demographic variables, which are generally taken as important predictors in sociology? These questions are not amenable to the kind of causal analysis which Booth et al. suggest.

If we were interested in testing a causal model, we would not proceed in the way Booth et al. appear ready to do. In their path model, they hypothesized that poor mental health leads to lack of privacy and an excess of demands. We assume that they are not concerned with objective levels of privacy and demands, but with the subjective experience. At the moment we will deal with the usefulness of their proposed path model. As Duncan (1975:168) notes, "causal models in sociology are likely to be seriously misspecified," and, as his book demonstrates, the utility of path models is limited to a very narrow range of phenomena (also see Leik 1979:602).[1] One could devote an entire paper to the problems with Booth et al.'s model and their attempt to draw conclusions from it but simply pointing out their basic flaws should be sufficient.

First, to be properly specified, their model must include the control variables of sex, race, marital status, education, age, and family income which were used in the analysis. The exclusion of these variables means they have a misspecified model.

Second, the various pathological domains discussed in our paper—mental health, social relations in the home, physical health and child care—are all related domains. Mental health affects social relationships, physical health and child care. Similarly, social relations, physical health and disruptive children all affect mental health. Comparable relationships occur with the other domains. At the same time, the various components of the domains are analytically distinct and empirically distinguishable (e.g., for our mental health measures see below; for social relationships, see Brocki 1979). In short, for a properly specified model, the need, at least initially, is to include a large number of measures of mental health, social relationships, physical health and child care. In constructing the initial path model within each domain, each dependent variable should be presented as involved in a complex network of causal relations with each of the others. Furthermore, in the initial analysis, each of the variables within a domain should be treated as though it were related to all of the other variables in all of the other domains. Obviously, as a practical matter, this is not a problem amenable to path analysis, and any attempt to analyze this data by path analysis would be meaningless.

---

[1] Leik (1979:602) states:

> If there is only one approach to theory, in the sense of the kind and shape of acceptable relationships (asymmetric, linear), then we do not even ask many potentially useful questions. All rates of change are constant, all variables are capable of exceeding any specifiable bounds, all effects are assumed to be from variable to variable, rather than to rates of change, to thresholds or ceilings, or to other parameters of more basic equations. In short, the only questions asked are whether more variables should be added and in what order they should appear. There is not much creativity in such an enterprise.

However, even if we go along with Booth et al. (1980), their analyses do not support the conclusion that our subjective scales are biased due to an underlying relationship with the mental health of the respondent.[2] Booth et al. present a mathematical analysis which suggests that if no bias were present in our measures of subjective crowding, then the ratio of the regression coefficient between objective crowding and pathology (controlling only demographic variables) to the regression coefficient between subjective crowding and pathology (controlling only demographic variables) should be constant across pathologies. According to Booth and his colleagues, if the value of this ratio changes across our pathologies, and if the ratio declines in value for indicators which are highly related to mental health and increases for variables which have no conceivable relationship to mental health, then there is evidence of bias.

Which variables should we look to as measures of mental health of the respondents? Booth et al. (1980), in the third paragraph of their comment, mention several, all among the variables we specifically indicate in our article as measures of mental health: ". . . irritation, alienation, a lack of positive affect, low self-esteem, and unhappiness. . . ." However, when they summarize their analysis of the ratios, their list of variables having the lowest ratios (those variables which, if Booth et al. are correct, should be indicators of mental health), contains none of the variables they suggested as measures of mental health possibly affected by bias. The variables with the lowest ratios are "involved in arguments; not enough sleep; catch a bug from others; not cared for by others; psychiatric symptoms; nervous breakdown; and, get along with others—all of them, variables closely tied to the conceptualization of poor mental health." Only two of these variables were among the group of mental health variables in our article; the others involve health and social relations in the home. As we indicated above, these other variables may have some relation to mental health, but it is hard to support the claim that they are "closely tied to the *conceptualization* of poor mental health" (emphasis added). Two of these variables seem very far from any definitional relationship with mental health: "not cared for by others," (The wording of the actual question was: "When you are *really* sick, is there someone to help take care of you?") and "catch a bug from others" (The actual wording of the question was: "If someone else at home is sick with the bug, do you often seem to catch it?"). How these items are closely tied to the "conceptualization of poor mental health" escapes us, and the idea is not defended or explained by Booth et al., simply asserted.

If one looked only at the lowest ratios presented by Booth and his colleagues, then one might think that there was some substance to the claim that our subjective scales are biased. However, Booth et al. also present the variables with the highest ratios. These are "positive marital relations; child care [i.e., all the child care items]; alienation; mental health balance; overall health; and physical violence—*variables that cannot be readily viewed as indicators of poor mental health*" (emphasis added). Booth et al. claim without additional comment that indicators in this list, which contains two of our mental health scales (mental health balance and alienation), and one of the scales which they suggested in the third paragraph of their comment to be an indicator of mental health (alienation), "cannot be readily viewed as indicators of poor mental health."

Given their claims about variables with low ratios, their claims about variables with high ratios seem very questionable. It is difficult to understand how Booth et al. can claim that "getting along with others" is an indicator of mental health, yet positive marital relations is inconceivable as a measure of mental health, as though positive marital relations does not involve getting along with anyone. If having arguments is so closely tied to the "conceptualization of poor mental health" (Booth et al., 1980), then why is becoming involved in physical violence (an escalated argument) so obviously unrelated to mental health? Tessler and Mechanic (1978) demonstrate that physical health and mental health are very highly intercorrelated, yet Booth et al. suggest, with no evidence and no rationale, that physical health is among the variables least related to mental health.

There does not appear to be any pattern in these variables which indicates that our subjective scales are biased because of mental health. There seems to be quite an adequate number of indicators of mental health among variables with high ratios, and among those with low ratios to prevent us from discovering the trend suggested by Booth et al. as indicative of bias. This is particularly evident when one calculates the suggested ratios for all the variables in our article, and arranges them from high to low as in Table 1.

[2, 3] We would emphasize that in another factor analysis, where we looked only at items measuring experiences in direct reaction to crowding, the experience of an excess of social demands and lack of privacy emerged as two very distinct and strong factors.

Table 1. Ratios Suggested by Booth et al. (1980) as Demonstrating Bias in the Subjective Scales of Crowding, Arranged in Descending Order

	Ratios*	Mental Health Variables $\bar{X}$ = .582	Social Relations in the Home $\bar{X}$ =.577	Physical Health $\bar{X}$ = .448	Care of Children $\bar{X}$ = 1.79	Items Suggested by Booth et al. $\bar{X}$ = .651
Supportive of child	3.55				C	
Positive affect	(−)2.90**	M**				B**
Know parents of playmates	2.68				C	
Know child's playmates	2.34				C	
Child can get away from others	1.84				C	
Child has place to study	1.32				C	
Physically punish child	1.27				C	
Alienation	1.03	M				B
Involved in physical violence	.966		S			
Positive marital relations	.920		S			
Overall health	.855			H		
Mental health balance	.766	M				
Relief when children out of home	.700				C	
Marital relations balance	.630		S			
Children are a hassle	.626				C	
Self esteem	.609	M				B
Must do chores (when sick)	.545			H		
Happiness	.529	M				B
Cannot get good rest	.506			H		
Feel close to spouse	.467		S			
Negative marital relations	.445		S			
Manifest irritation	.434	M				B
Nervous breakdown	.406	M				
Psychiatric symptoms	.301	M				
Get along with others	.296		S			
Not cared for by others (when sick)	.296			H		
Catch 'bug' from others	.278			H		
Not enough sleep	.208			H		
Get along with children	−.135**		S**			
Involved in arguments	.018		S			

* It is unclear how Booth et al. (1980) calculated their "average ratios." The ratios in this table were calculated by obtaining the ratios associated with the privacy scale and the demands scale, and then calculating the mean of these two ratios.

** These items were not included by Booth et al. (1980) because of negative sign. These ratios are not included in the calculation of the means at the top of the Table. Booth et al. (1980) mention 3 ratios with negative signs, while we appear to have only 2. This is because for one item there are two negative signs while for the other there is one.

The items which we would suggest are the best indicators of mental health are those which we included in our original Table 3, entitled "Overcrowding and Mental Health" (Gove et al., 1979:71). Ratios for these variables are widely scattered up and down the range of the ratio. The mean value of the ratio for all the mental health items is .582, clearly not the lowest for all the four groups of variables. Child care is highest, with a mean of 1.79, followed by mental health at .582, social relations in the home at .577, and physical health at .448. The mental health items suggested by Booth et al. as items possibly confounded with our subjective scale have a mean ratio of 1.10 if one includes positive affect (a scale which Booth et al. (1980) eliminate from consideration because the negative sign for its ratio makes it "ambiguous") and .651 if positive affect is excluded. This ratio of .651 is about three times higher than the low group of variables (mean .22) claimed by Booth et al. to be "closely tied to the conceptualization of poor mental health." We are mystified why Booth et al. ignore these more relevant comparisons and try to make us believe that the variables with low ratios are measures of mental health and those with high ratios are not.

There does appear to be variation in the ratio across our indicators of pathology, but this ratio does not vary in the way Booth et al.

claim is indicative of mental health bias. What Booth et al. do not make clear is the fact that this ratio will vary if the pattern of relationships between objective crowding and pathology and subjective crowding and pathology changes from one indicator to the other. This is precisely what would happen if the effect of the subjective scale relative to the effect of objective crowding changed from one variable to the next, and/or the impact of controlling for the demographic variables on the relationship between subjective crowding and pathology relative to the impact of controlling for demographics on the relationship between objective crowding and pathology changed from one indicator of pathology to the next. This is what one would expect unless each of the indicators of pathology was related to the subjective and objective measures in exactly the same way—a situation which we specifically suggested in our original (Gove et al., 1979:62) article was likely *not* to be the case.

(3) Booth et al. (1980, fn. 2) indicate that they asked us to make some of our data available to them so that they could check on the accuracy of our analysis and conclusions, and that we refused to provide them with the data. We would like to indicate why we chose not to give them the data they requested. First, we are presently involved in more thorough analyses of our crowding data (Gove et al., forthcoming, 1981) and did not want to make our data publicly available until the more complete analysis was published. Second, and more serious, we had questions about the way Booth intended to use our data. As is indicated in Gove and Hughes (1980) the work of Booth and his collaborators is replete with numerous conceptual and methodological flaws, and we were concerned that they would unknowingly misuse our data and produce an analysis that was substantially flawed. For example, in a number of articles Booth and his colleagues indicate that the characteristics of their sample "do not limit the generalizability of findings to urban adult family members" (Booth 1977:647; see also Edwards and Booth 1976:75; and Booth and Welch 1978:25, for a virtually identical claim). Given the very atypical nature of their sample, this seems like a very strong statement. In each case, it was justified, however, by presenting the demographic characteristics of Toronto and comparing them to the demographic characteristics of a sample of 4,889 respondents randomly drawn from the 17,128 households screened, and *not* to the characteristics of the 862 households *interviewed* and actually included in the sample (see Booth, 1976:38–9). The characteristics of the households screened, however, were very different from the subset selected for interviewing. To give only one example, in the screened sample, 69% of the households had intact families, and in the interviewed households, all the families were intact. It is, thus, impossible to empirically support the claim that the characteristics of the sample "do not limit the general applicability of the findings to urban adult families." This misrepresentation of their data, combined with the fact that they wanted to use our data to test their inappropriate path model, and the fact that they present contradictory evidence in regard to ratios discussed above as supporting their position would seem to justify our caution.

(4) Citing Fuguitt and Lieberson (1974) and Schuessler (1973), Booth et al. indicated that the use of the ratio variable, persons-per-room "create[s] methodological pitfalls." However, as is shown by Long (1980) and Kasarda and Nolan (1979), the use of a ratio variable is nonproblematic when there is little measurement error. The number of persons in a household and the number of rooms in the household (particularly when there are detailed instructions on how to count rooms) should contain little measurement error, particularly when compared to most variables used by sociologists. In short, the use of this particular ratio variable is appropriate.

(5) Although we are studying crowding, and Booth et al. (1980) acknowledge that we demonstrated that our objective measure of crowding, persons-per-room, does measure crowding and not something else, they indicate we should have added to our control variables the number of persons. They give no theoretical reasons for adding the number of persons to our analysis, and the number of persons is not a measure of crowding. Furthermore, when Booth and Edwards (1976) used persons-per-room, they themselves did not control on the number of persons. As the number of persons comprises the numerator of our measure of crowding, the empirical effect on purely mathematical grounds has to be to decrease substantially the apparent effect of crowding, and is, in fact, highly collinear with crowding (r – .801)—a situation which would cause severe methodological problems. In the absence of any theoretical reason for including the number of persons in the analysis, we are totally mystified by their suggestion.

(6) Booth et al. (1980) suggest that we use the rooms-deficient measure developed by Booth (1976). However, that measure is seriously flawed as a measure of crowding. What the measure establishes is the typical number of rooms for households containing different combinations of types and numbers of house-

hold members. If the number of rooms is fewer than is typical for a particular combination of types of household members, then the household is "crowded," and if it has more rooms than the average for that "household type" then the household is "uncrowded." What Booth never considers is that for some combinations of household members (e.g., poor, multigenerational households), the household with the average number of rooms may be very crowded while for other household combinations (e.g., husband and wife only) the average number of rooms may, in most cases, reflect low levels of crowding. To take the extreme case, a person who lives alone in two rooms might very well, by their definition, be socially crowded. Finally, we would ask why, if their measure is so good, they did not use it as an independent variable in their study?

(7) The final measurement problem Booth et al. (1980) note is that we have several dependent measures that are not dependent variables at all. Booth et al. mention two: "no place for a child to study" and "no place at home where the child can get away from others." Since those are the only questions of this nature, we obviously have only two questions, not several. Furthermore, as they note, these questions "tap [a] dimension of [the] crowding experience." As we are studying the crowding experience, we are puzzled that Booth et al. would find these questions inappropriate. We would note that Booth and Johnson (1975) found children in crowded households to be one year behind in school, and we would suggest that one of the major reasons for this finding, which we view as important, is tapped by the two questions asked.

(8) Booth et al. discuss at considerable length the amount of variance explained by our crowding variables and they indicated that the amount borders on the trivial. First, we would note that we explain twice as much variance as that explained by Booth et al. Second, the amount of variance uniquely explained by our three crowding variables is 4.2%, and the amount of variance uniquely explained by the combined effects of our six control variables (sex, race, marital status, education, age, and family income) is 4.1%. Thus, the percent variance explained may seem small to Booth et al. but, compared to the traditional variables used by sociologists, it is substantial. Furthermore, the total variance explained by the crowding variables (including collinear variance) is 5.7%, not 4.2% as Booth and his colleagues assert.

In total, for our 30 variables we explain an average of 9.8% of the variance. We go to considerable length to demonstrate that, for survey data of this type, this is about the amount of variance one would expect to explain. We made the point that this fact is not always recognized. To demonstrate our point once again, let us note three very diverse studies. Chiricos and Waldo (1975) investigated the determinants of criminal sentencing and looked at the effects of SES, age, race, urbanization, confinement in a juvenile institution, prior arrests and prior convictions; they found that out of 11 analyses, these variables, on the average, expalined 8.9% of the variance in sentence length. Wolinsky (1978) studied 24 indicators of health facility utilization and used 29 predicting variables, including restricted days, bed disability days, lost days, subjective health limited mobility, limited activity, total health conditions total chronic conditions and total chronic conditions limiting activity or mobility. On the average he explained 10.4% of the variance in health utilization. Warheit et al. (1976) looked at marital status, age, SES, sex, race, marital history plus some interaction terms and explained 10.7% of the respondents' mental health scores. In summary, with individual data one should not anticipate explaining much variance.

(9) Booth et al. (1980) assert that we selected census tracks on their "chance of yielding crowded households, blacks and certain socioeconomic levels." This statement misrepresented what we did. We selected tracks to get a representative range of crowding, SES and the proportion of blacks residing in Chicago, while at the same time attempting to minimize the collinearity between crowding and SES. (In doing our selection there was a slight tendency to overselect tracks with both high and low SES.) Booth et al. state that our sampling design affected our measures of dispersions and the correlation among the measures. They assert that we have committed a "particularly serious flaw when [we] proceed to conclude . . . that our crowding measures have substantial [independent] effects. . . ." As is clearly indicated in Table 1 of our article (Gove et al., 1979:63), the variation in our control variables appears to be similar to the variation on the same variables as Chicago. These data will be presented in more detail in Gove et al. (1981). If there is any deviation, it is that we have more variation in the sample on our measures of SES than occurs in Chicago, and thus our sample is slightly weighted toward finding strong effects of SES as compared to crowding. Thus the issue raised by Booth et al. is not a serious one. Furthermore, it is certainly appropriate for us to draw conclusions regarding the census tracks from which we draw representative samples, and (as noted) these tracks are selected such as to be fairly

characteristic of Chicago (except that there should be less collinearity between SES and crowding in our sample than in Chicago).

Booth (1976) draws the same type of conclusions we drew, differing only in that he concluded crowding had very little effect. As Booth (1976) notes, and Gove and Hughes (1980) emphasize, he drew his sample from very atypical and very crowded census tracks. Furthermore, Booth (1976) did not randomly sample households residing in those census tracks (as we did) but screened households, obtaining interviews in only 3.3% of the households screened. Thus, Booth (1976) is not only not in a position to draw any conclusions regarding crowding in Toronto, but he is also in no position to draw conclusions regarding the census tracks he sampled. If Booth et al. (1980) think our sampling design makes it difficult to draw conclusions, they should apply the criteria they raise regarding our study to the Toronto study. If they do, they will find by their own criteria the sample design in the Toronto study is so flawed that they have an insurmountable problem and have no basis from which to draw any general conclusion regarding the effect of crowding.

(10) Booth et al. also criticize us for omitting data and not considering certain interactions in more detail. We would note two things: First, the paper originally submitted was twice as long as the paper published and thus some omissions were not by our choice. Second, much more detailed analyses will be presented in Gove et al. (1981).

(11) To us the most serious issue raised by Booth et al. (1980) is whether our operational indicators of a lack of privacy and overload of social demands are empirically as well as analytically distinct from our measures of mental health. In their path model Booth et al. treat mental health as a single variable which clearly suggests that they are willing to treat mental health as an unidimensional concept. However, even before Bradburn's (1969) excellent book on psychological well-being, it had become clear that mental health involves a variety of analytically and empirically distinct dimensions. As this is the case, and as we used a variety of indicators of mental health, the question really is whether our measures of the experience of crowding (lack of privacy and social demands) are measuring the same thing that our various indicators of mental health are measuring. In the section below we present a factor analysis which indicates that psychiatric symptoms, positive affect, self-esteem, manifest irritation and the subjective experience of crowding, as we have measured them, are empirically distinct. We then briefly discuss our measures of alienation, happiness and the mental health balance scale.

The factor analysis is presented in Table 2. It clearly demonstrates that our measures of psychiatric symptoms, the experience of crowding,[3] self-esteem, manifest irritation, and positive affect (1) are distinct factors and (2) the items in each scale tend to load only on one factor. This is particularly important with regard to the measures of the experience of crowding, for it clearly demonstrates that the items measuring mental health and those measuring experience of crowding are empirically as well as analytically distinct. In short, the problem which Booth et al. suggested underlay our analysis, namely, that our measures of the experience of crowding and mental health were empirically indistinguishable, is simply not a problem. We would note that these factors all scale at least reasonably well (the alpha reliability coefficient with psychiatric symptoms is .85, with experience of crowding it is .73, with self-esteem it is .72, with manifest irritation it is .68, and with positive affect it is .66).

Regarding our other measures of mental health, our measure of alienation is actually a composite of three subscales (political powerlessness, normlessness and belonginglessness), and its overall alpha reliability coefficient, which is .58, is actually lower than the alpha reliability of some of its subscales. As a consequence, the alienation items do not emerge clearly as a factor. Nevertheless, when it is entered into the factor analysis, it emerges as the sixth factor with an eigen-value of 1.16. Furthermore, the alienation items are unrelated to the experience of crowding (only one item achieves a factor loading of over .1 on the experience of crowding factor, and three of the items are negatively related). When nervous breakdown is entered into the analysis as a single item, it obviously will not emerge as a separate factor; instead, it has a moderate loading on the psychiatric symptoms factor (.37), and is unrelated to the other factors. However, a nervous breakdown is obviously a powerful experience and warrants being included in an investigation of mental health. As Campbell et al.'s (1976) discussion of happiness demonstrates, happiness is clearly an important and analytically distinct concept. It has a very modest negative loading on the positive affect factor, but by and large it appears to be largely unrelated to any of the factors presented in Table 2. The mental health balance scale is a combination of the psychiatric symptom and positive affect scales and is thus inappropriate for inclusion in our factor analysis. The mental health balance scale is

Table 2. Varimax Factor Rotation of the Individual Items in the Four Major Mental Health Scales and the Subjective Experience of Crowding Scales (Lack of Privacy and Social Demands)

		Factor 1	Factor 2	Factor 3	Factor 4	Factor 5
Psychiatric Symptoms Scale	PS1	0.290	0.092	−0.168	0.145	0.066
	PS2	0.414	0.039	−0.172	0.067	−0.010
	PS3	0.259	0.002	−0.091	0.055	0.015
	PS4	0.490	0.065	−0.182	−0.004	−0.055
	PS5	0.641	0.071	−0.016	0.042	−0.042
	PS6	0.626	0.148	−0.113	0.118	0.012
	PS7	0.569	0.058	−0.110	0.102	−0.084
	PS8	0.515	0.075	−0.132	0.097	0.055
	PS9	0.561	0.087	−0.099	0.078	0.012
	PS10	0.560	0.096	−0.160	0.119	−0.051
	PS11	0.699	0.087	−0.167	0.050	0.002
	PS12	0.630	0.118	−0.100	−0.028	−0.059
	PS13	0.550	0.071	−0.236	−0.037	−0.062
Subjective Experience of Crowding Scale	Dem1	0.075	0.470	−0.028	0.094	0.057
	Dem2	0.130	0.639	−0.067	0.088	0.013
	Dem3	0.082	0.553	−0.047	0.132	0.040
	Dem4	0.257	0.619	−0.001	0.185	0.041
	Priv1	0.084	0.376	−0.080	0.069	−0.011
	Priv2	0.103	0.700	−0.036	0.062	−0.024
	Priv3	−0.008	0.278	−0.076	0.083	−0.021
Self-Esteem Scale	SE1	−0.057	−0.011	0.304	0.057	0.072
	SE2	−0.172	−0.046	0.548	−0.092	0.007
	SE3	−0.016	−0.085	0.516	−0.076	−0.030
	SE4	−0.097	−0.065	0.429	−0.039	0.100
	SE5	−0.129	−0.045	0.407	0.110	0.052
	SE6	−0.141	−0.056	0.457	−0.035	0.095
	SE7	−0.157	−0.065	0.504	0.016	0.120
Manifest Irritation Scale	MI1	0.043	0.091	−0.006	0.590	0.034
	MI2	0.109	0.041	−0.054	0.291	0.027
	MI3	0.114	0.092	−0.098	0.105	−0.037
	MI4	−0.039	0.083	−0.022	0.380	0.065
	MI5	0.106	0.158	−0.037	0.437	0.018
	MI6	0.291	0.176	−0.129	0.224	−0.063
	MI7	0.035	0.187	0.011	0.574	0.001
	MI8	0.027	0.025	−0.020	0.110	0.035
	MI9	0.103	0.154	−0.031	0.518	0.012
	MI10	0.289	0.206	−0.160	0.451	0.023
Positive Affect Scale	PA1	0.022	0.019	0.085	0.118	0.457
	PA2	0.096	0.031	0.148	−0.005	0.571
	PA3	0.023	0.018	0.097	0.004	0.642
	PA4	−0.135	0.013	0.095	0.033	0.508
	PA5	−0.213	0.017	0.031	−0.037	0.520
Eigen-values		6.22	2.44	1.55	1.89	1.08

useful not only because it probably is the best summary measure of mental health (e.g., see Bradburn, 1969; Cherlin and Reeder, 1975). We would emphasize, however, that Bradburn and, particularly, Cherlin and Reeder see the individual components of mental health as worth more attention than any summary measure.

In conclusion, the items involved in the factor analysis presented in Table 1, as well as the items discussed only in writing, provide a very clear demonstration that the measures of the experience of crowding are not empirically confounded with our measures of mental health. This is not to say that the experience of crowding is not related to one's mental health; in fact, our whole analysis is based on the assumption (and demonstration) that such a relationship exists. What our analysis does indicate is that not only are the concepts analytically distinct, but that they have been measured in such a manner that the operational definitions are also empirically distinct.

To summarize, we have carefully considered all of the issues raised by Booth et al. and conclude that none of the issues raised by Booth et al. regarding the validity of our study withstands careful scrutiny. We reassert that our study strongly suggests that crowding does often have substantial effects, and it is time to turn away from the question, "Does crowding ever have effects?" and turn toward questioning the conditions which maximize or minimize the effects of crowding.

Walter R. Gove
Vanderbilt University
Michael Hughes
Virginia Polytechnic Institute and State University

REFERENCES

Booth, Alan
1976 Urban Crowding and its Consequences, New York: Praeger.

Booth, Alan
1977 "Wife's employment and husband's stress: a replication and refutation." Journal of Marriage and the Family 39:645–50.

Booth, Alan, and John N. Edwards
1976 "Crowding and family relations." American Sociological Review 41:308–21.

Booth, Alan and David Richard Johnson
1975 "The effect of crowding on child health and development." American Behavioral Scientist 18:736–49.

Booth, Alan, David R. Johnson, and John N. Edwards
1980 "In pursuit of pathology: the effects of human crowding" (Comment on Gove, Hughes, and Galle, ASR, February, 1979) American Sociological Review 45:873–8.

Booth, Alan, and Susan Welch
1978 "Spousal consensus and its correlates." Journal of Marriage and the Family 40:23–34.

Bradburn, Norman
1969 The Structure of Psychological Well-Being. Chicago: Aldine.

Brocki, Severine
1979 "Gender, marital status and mental health." Unpublished Ph.D. dissertation, Department of Sociology, Vanderbilt University.

Campbell, Angus, Philip Converse, and Willard Rogers
1976 The Quality of American Life: Perceptions, Evaluations and Satisfaction. New York: Russell Sage.

Cherlin, A., and L. G. Reeder
1975 "The dimensions of psychological well-being: a critical review." Sociological Methods and Research 4:189–214.

Chiricos, Theodore, and Gordon Waldo
1975 "Socioeconomic status and criminal sentencing: an empirical assessment of a conflict proposition." American Sociological Review 40:753–72.

Duncan, Otis Dudley
1975 Introduction to Structural Equation Models. New York: Academic.

Edwards, John and Alan Booth
1976 "Sexual behavior in and out of marriage: an assessment of correlates." Journal of Marriage and the Family 38:73–81.

Fuguitt, G. and S. Lieberson
1974 "Correlations of ratios or differences scores having common terms." Pp. 128–44 in H. Costner (ed.), Sociological Methodology, San Francisco: Jossey-Bass.

Gove, Walter, and Michael Hughes
1980 "The effects of crowding found in the Toronto study: some methodological and empirical questions." (A comment on Booth and Edwards, ASR, 1976) American Sociological Review 45:864–70.

Gove, Walter R., Michael Hughes, and Omer R. Galle
1979 "Overcrowding in the home: an empirical investigation of its possible pathological consequences." American Sociological Review 44:59–80.

Gove, Walter, Michael Hughes, and Omer Galle
1981 Household Crowding: An Empirical Analysis of the Impact of a Physical Environment on Social Environment and its Consequences. New York: Academic (forthcoming).

Kasarda, John, and Patrick Nolan
1979 "Ratio measurement and theoretical inference in social research." Social Forces 58:228–48.

Leik, Robert
1979 "Comment: where is the cutting edge of sociology?" The Sociological Quarterly 29:591–603.

Long, Susan
1980 "The continuing debate over the use of ratio variables: facts and fiction." Pp. 37–67 in Karl Schuessler (ed.), Sociological Methodology. San Francisco: Jossey-Bass.

Schuessler, K.
1973 "Ratio variables and path models." Chap. 10 in A. Goldberger and O. Duncan (eds.), Structural Equation Models in Social Sciences.

Tessler, Richard, and David Mechanic
1978 "Psychological distress and perceived health status." Journal of Health and Social Behavior 19:254–62.

Warheit, George, Charles Holzer, Richard Faust, and Eric Single
1976 "Sex, marital status and mental health." Social Forces 55:459–70.

Wolinsky, Fredric
1978 "Assessing the effects of predisposing, enabling and illness-morbidity characteristics on health service utilization." Journal of Health and Social Behavior 19:384–96.

ITEMS

■ CHRISTOPHER JENCKS (Heredity, Environment, and Public Policy Reconsidered) is Professor, Department of Sociology and Center for Urban Affairs, Northwestern University. His current research centers on the social indicators of unselfishness as well as on trade-offs between earnings and other job characteristics. His latest book (from Basic Books, 1979, and written with 11 coauthors) is *Who Gets Ahead?*

■ JAMES N. BARON (Bringing the Firms Back In) is a Ph.D. candidate in the Department of Sociology at the University of California–Santa Barbara. WILLIAM T. BIELBY is Assistant Professor of Sociology, also at the University of California–Santa Barbara. The two are also affiliated with the Social Process Research Institute at UCSB. They are collaborating on a three-year study of economic segmentation, technology, and work organization being funded by the National Science Foundation.

■ RICHARD A. BERK (Crime and Poverty), Professor, Department of Sociology, University of California–Santa Barbara, is busy on three research projects: (1) evaluation of California Senate Bill 224; (2) a monograph reporting evaluation of impact of water-conservation programs in California; and (3) how to improve ways of monitoring the performance of the criminal justice system. His book (with Lenihan and Rossi, and from which this article is drawn), *Money, Work, and Crime*, is forthcoming from Academic Press. KENNETH J. LENIHAN is Associate Professor at John Jay College, the CUNY, and has worked for a decade in the area of criminal justice research. His most recent monograph, "Opening the Second Door," was published by the U.S. Department of Labor Employment and Training Administration. PETER H. ROSSI is Professor, Department of Sociology; and Director, Social and Demographic Research Institute—both at the University of Massachusetts–Amherst. Crime seriousness, social experimentation, and estimating household damage from natural disasters are among his fields of research interest. His book, *Hand of Survey Research* (written with Wright and Anderson), will be published by Academic Press in 1981.

■ DAVID A. SNOW (Social Networks and Social Movements) is Assistant Professor, Department of Sociology, University of Texas, Austin. He is engaged in research dealing with movement recruitment and conversion, interactive processes in crowds, the systematization of qualified research techniques, and the social construction and implications of "accidents." LOUIS A. ZURCHER, JR., also at the University of Texas, Austin, is Professor, Graduate School of Social Work and Department of Sociology. His present research is in the areas of: self-concept and social change, the dramaturgical nature of emotion, and symbolic interactionism of scapegoating. His *Human Responses to Social Problems* (with Louis Schneider, Cookie White Stephan, and Sheldon Olson) is forthcoming from Irwin-Dorsey. SHELDON EKLAND-OLSON is Associate Professor, also in the Department of Sociology at the University of Texas, Austin. He is doing (1) a study of "coming home from prison" and (2) conceptual development of the importance of social networks for studies of deviance and social control.

■ JOHN HAGAN (Differential Sentencing of White-Collar Offenders) is Professor, Department of Sociology, University of Wisconsin–Madison. He has just completed a monograph, "The Organizational Domination of Criminal Law: A Study of Victim Involvement in the Criminal Justice System"; and is involved in a study of the sanctioning of stocks and securities violators in Toronto. ILENE H. NAGEL (BERNSTEIN) is Associate Professor, School of Law, Indiana University. During the Fall, 1980, she is a Visiting Fellow at the Institute of Criminology at Cambridge University and at the Netherlands Institute for Advanced Study in the Humanities and Social Sciences. She and John Hagan have a forthcoming book on prosecutorial and judicial discretion with emphasis on the differential impact caused by a variety of elements. CELESTA ALBONETTI is a graduate student at Indiana University. She is working on an analysis of the impact of structural effects on the frequency and type of organizational contacts among treatment programs for adjudicated juvenile delinquents. In addition, she wants to do further research, applying organizational concepts to the analysis of the criminalization process.

■ RONALD S. BURT (Testing a Structural Theory of Corporate Cooptation) is Assistant Professor, Department of Sociology and Survey Research Center, University of California–Berkeley. His chief interest is the general development of structural sociology. KENNETH J. CHRISTMAN is a Ph.D. candidate in the Department of Sociology and is also at UC–Berkeley. In addition to studying the sociology of organizations, he is pursuing research in social psychology and is completing a monograph on the social psychology of Emile Durkheim. HAROLD C. KILBURN, JR., is a Ph.D. candidate also, in the Department of Sociology, State University of New York–Albany. He is currently engaged in a comparative analysis of interorganizational networks.

■ GARY D. LA FREE is Assistant Professor, Department of Sociology, University of New Mexico. With Barbara Reskin, he is studying jury verdicts in rape cases. He is also researching official decision-making processes in felony cases.

■ MARCIA WHICKER TAYLOR (Comment on Scarr and Weinberg) is Assistant Professor, Department of Government and International Studies, University of South Carolina. She is developing several computer simulation models which examine the impact of government structure and policies upon the distribution of rewards and upon citizen preference maximization.

■ WALTER R. GOVE (Comment on Booth, Johnson, and Edwards) is Professor, Department of Sociology, Anthropology, Vanderbilt University. MICHAEL HUGHES, on leave from his Assistant Professorship at Virginia Polytechnic Institute and State University, is also at Vanderbilt through the Fall semester, 1980. He and Gove, under an NICHD grant, are looking even further at the effects of both "crowding" and "living alone" upon mental health and general well-being.

■ ALAN BOOTH (Comment on Gove and Hughes) is Professor, Department of Sociology, University of Nebraska–Lincoln. DAVID R. JOHNSON is also at Lincoln, University of Nebraska, as an Associate Professor of Sociology. JOHN N. EDWARDS is Professor, Department of Sociology, Virginia Polytechnic Institute and State University.

OXFORD

GALAXY PAPERBACKS

Sociobiology Examined
Edited by ASHLEY MONTAGU
1980 368 pp. Galaxy Book 602 $5.95

Abortion in America
The Origins and Evolution of National Policy, 1800-1900
JAMES C. MOHR, University of Maryland
1979 352 pp. Galaxy Book 584 $4.95

Women and Equality
Growing Patterns in American Culture
WILLIAM H. CHAFE, Duke University
1978 224 pp. Galaxy Book 531 $3.95

Growing Old in America
Expanded Edition
DAVID HACKETT FISCHER, Brandeis University
1978 304 pp. Galaxy Book 532 $4.95

Slave Religion
The "Invisible Institution" in the American South
ALBERT J. RABOTEAU, University of California, Berkeley
1979 400 pp.; 14 illus. Galaxy Book 594 $5.95

Learning Non-Aggression
The Experience of Non-Literate Societies
Edited by **ASHLEY MONTAGU**
1978 224 pp. Galaxy Book 525 $3.95

Twilight of Authority
ROBERT NISBET, American Enterprise Institute
1977 304 pp. Galaxy Book 485 $4.95

America by Design
Science, Technology, and the Rise of Corporate Capitalism
DAVID F. NOBLE, Massachusetts Institute of Technology
1979 416 pp. Galaxy Book 588 $5.95

The Puritan Way of Death
A Study in Religion, Culture, and Social Change
DAVID E. STANNARD, Yale University
1979 256 pp. Galaxy Book 573 $3.95

Masters of Social Psychology
Freud, Mead, Lewin, and Skinner
JAMES A. SCHELLENBERG, Indiana State University, Terre Haute
1979 160 pp. Galaxy Book 590 $2.95

The Survivor
An Anatomy of Life in the Death Camps
TERRENCE DES PRES, Colgate University
1980 230 pp. Galaxy Book 596 $3.95

Days of Sorrow and Pain
Leo Baeck and the Berlin Jews
LEONARD BAKER
1980 448 pp.; 51 illus. Galaxy Book 611 $7.95

Kolyma
The Arctic Death Camps
ROBERT CONQUEST
1979 254 pp. Galaxy Book 591 $4.95

Sociology as an Art Form
ROBERT NISBET
1976 160 pp. Galaxy Book 472 $3.95

Prices and publication dates are subject to change.

Many of these fine books are also available in cloth.

OXFORD UNIVERSITY PRESS
200 Madison Avenue • New York, N.Y. 10016

OXFORD

Introductory Sociology

Order and Change in Society
Third Edition

GERALD R. LESLIE, University of Florida; **RICHARD F. LARSON**, Clemson University; and **BENJAMIN L. GORMAN**, University of Florida. Designed for students in the general survey course, *Introductory Sociology* stresses the interrelations of social life, social processes, and social institutions. The text covers all the major substantive areas of contemporary sociology, and thoroughly integrates major theoretical models and research methods. Outlines, summaries, lists of important terms, annotated readings, questions, and projects are included. Clearly written, lavishly illustrated and handsomely designed, *Introductory Sociology* presents sound sociological principles in a manner both comprehensible and relevant.
1980 608 pp.; 200 photos, 50 figs. $17.95

Complimentary Instructor's Manual

Socguide

An Aid to Learning Sociology through Projects, Puzzles and Problems to Accompany Leslie, Larson and Gorman: *Introductory Sociology*, Third Edition
MARGARET-MARY FRANZ, California State University, Hayward
1980 160 pp.; 22 figs. paper $4.95

A Primer on Ethnomethodology

KENNETH LEITER, Southern Methodist University
1980 264 pp. cloth $14.95 paper $4.95

The Environment

From Surplus to Scarcity
ALLAN SCHNAIBERG, Northwestern University
1980 480 pp. cloth $14.95 paper $9.95

Suburban Youth in Cultural Crisis

RALPH W. LARKIN, Rutgers University
1979 272 pp. cloth $14.95 paper $4.95

Theoretical Criminology

Second Edition
GEORGE B. VOLD. Revised by **THOMAS J. BERNARD**
1979 448 pp.; figs. $14.95

Crime Control Strategies

An Introduction to the Study of Crime
HAROLD E. PEPINSKY, Indiana University
1980 370 pp. . paper $9.95

Police Behavior

A Sociological Perspective
Edited by **RICHARD J. LUNDMAN**, Ohio State University
1980 352 pp. paper $5.95

Women, Crime, and Justice

Edited by **SUSAN K. DATESMAN**, Arizona State University, and **FRANK R. SCARPITTI**, University of Delaware
1980 384 pp.; 19 tables paper $5.95

Prices and publication dates are subject to change.

The Social Condition of Humanity

An Introduction to Sociology

IRVING M. ZEITLIN, University of Toronto. This unique introduction to the sociological perspective acquaints the student with the classical foundations of sociological analysis. Informed throughout by sociological theory, the book employs classical conceptual tools to address basic questions concerning the human condition in all the major areas of sociological concern.

January 1981 450 pp. paper $8.95

The Study of Social Problems

Five Perspectives

Third Edition

EARL RUBINGTON, Northeastern University, and **MARTIN S. WEINBERG,** Indiana University. Designed for the social problems course, this text-reader offers a structured survey, examples, and criticisms of five major perspectives on social problems: Social Pathology, Social Disorganization, Value Conflict, Deviant Behavior, and Labeling. More than one third of the selections are new to this edition. Each group of readings is prefaced by an introductory essay and followed by discussion questions and an annotated bibliography.

February 1981 284 pp. paper $7.95

The Solution of Social Problems

Five Perspectives

Second Edition

MARTIN S. WEINBERG, EARL RUBINGTON, and **SUE KIEFER HAMMERSMITH,** Indiana University-Purdue University at Indianapolis. This reader traces the implications of the five perspectives presented in *The Study of Social Problems* and demonstrates the kinds of solutions each perspective prescribes. The editors' text has been completely rewritten for this edition, while the readings, which explore a wide range of social problems, including racism, sexism, poverty, and drug addiction, have been updated.

January 1981 220 pp. paper $7.95

Sentencing

Edited by **HYMAN GROSS,** Cambridge University, and **ANDREW VON HIRSCH,** Rutgers University. This collection of readings in the current literature addresses the issue of disparity in sentencing in the context both of the historical and philosophical background, and of the contemporary debates over the aims of sentencing, and the modalities and severity of punishment.

January 1981 426 pp. cloth $19.95 paper $9.95

City Lights

An Introduction to Urban Studies

E. BARBARA PHILLIPS and **RICHARD T. LeGATES,** both of San Francisco State University. This introduction to urban studies presents social science theory and research, artistic insights, and current policy issues in terms of competing perspectives, encouraging the reader to choose more intelligently among them. Highly readable, comprehensive, up to date in its scholarship, and attractively illustrated, the text features student-tested learning aids to stimulate questioning and theory-building.

February 1981 592 pp.; 153 illus. paper $14.95

SINCE 1478

OXFORD UNIVERSITY PRESS

200 Madison Avenue • New York, N.Y. 10016

Handbook of Organizational Design

Volume I: Adapting Organizations to their Environments

Volume II: Remodelling Organizations and their Environments

Edited by P.C. NYSTROM and W.H. STARBUCK

This important new reference work offers a comprehensive survey that summarizes and reinterprets current research and also indicates new directions in organizational theory. In drawing together contributions by prominent scholars from a broad range of fields, the editors emphasize the interdisciplinary nature of a field that encompasses research in different nations, industries, organizational forms, and a wide range of social science disciplines. The *Handbook of Organizational Design* will become an invaluable reference for those professionals and advanced students in business management and the social sciences, not only as a basis for planning but also for its contributions to future directions and development.

Volume I characterizes the effects of environments on organizations. Chapters in this volume analyze the adaptive capabilities of organizations such as planning, forecasting and innovation. Attention is also devoted to the effects of change, particularly changes in technology and in the legal and political systems.

Volume II focuses on the process of change within organizations, including control systems, interdepartmental relations and job design. Contributors discuss the effects, both planned and accidental, that organizations have upon their environments and analyze the relationship between organizations, governments, and society.

P.C. NYSTROM is an Associate Professor of Business Administration at the University of Wisconsin. W.H. STARBUCK is Helfaer Professor in the School of Business Administration at the University of Wisconsin.

Available March 1981

Volume I	588 pp.; 32 figs.	$29.95	$55.00 (set)
Volume II	540 pp.; 29 figs.	$29.95	

For a full list of contents and contributors, please write to the Academic Department:

OXFORD UNIVERSITY PRESS
SINCE 1478
Publishers of Fine Books for Five Centuries

200 Madison Avenue, New York, New York 10016

Price and publication date are subject to change.

Singular in the Social Sciences
because Dorsey is:

Dedicated to publishing carefully selected, top-quality books in Anthropology, History, Political Science, Public Administration, Psychology, Social Welfare, and Sociology.

Open to opportunities to publish and promote books in the Social Sciences that break new ground. Recognizing that creative thinking and writing are not exclusive to any one school of thought, Dorsey has been receptive to many different approaches and techniques.

Respected in the publishing industry for providing the works of many widely acclaimed authors to educational institutions in the United States, Canada, and abroad.

Specialized in the Social Science field, The Dorsey Press employs a solid editorial staff with extensive education and field training in their particular areas of expertise.

Experienced in textbook publishing, The Dorsey Press executes unusually effective methods of editing, designing, manufacturing, selling, and promoting college texts.

Yearly produces many new and revised texts in the same quality manner that has been used for over 20 years.

The Dorsey Press
HOMEWOOD, ILLINOIS 60430

Community Development in America

edited by
James A. Christenson &
Jerry W. Robinson, Jr.

THIS is the first major assessment of the theory, research, and practice of community development (CD) in America.

The 25 contributors—all members of the Community Development Society—are experts from government, private enterprise, universities, extension service, and institutions.

They represent sociology, agricultural economics, planning, urban affairs, psychology, education, and social work.

The volume is designed to provide a state of the art of CD and information about CD. Key points include how it began, what it is today, problems CD faces, and future trends.

The three major themes of technical assistance, self-help and the conflict approach receive special emphasis.

What do community development professionals do? What are their individual traits, the functions they perform, the institutional setting in which they work, and their clientele?

Responding from a variety of perspectives, a cross section of CD workers share practical insights into their work.

Some major research is reviewed. A grounded theory framework demonstrates the integration of theory and practice in CD.

1980, 245 pp., ill., $8.50
(Mail orders: Add $1/book for mailing.)

Iowa State University Press
Dept. CD2, Ames, IA 50010

Community Power Succession

Atlanta's Policy-Makers Revisited

by Floyd Hunter

Who really runs Atlanta? Hunter combines scholarly analysis, personal reminiscences, observation, and social prescription in his study of the power structure of one of America's most dynamic cities. "A fascinating and worthy successor to the now-classic *Community Power Structure*. This book shows how right Floyd Hunter was about Atlanta and how fruitful his research methods can be in talented hands." —G. William Domhoff, University of California, Santa Cruz

approx. 220 pp., $15.00 cloth, $9.00 paper
September 1980

Research in Service to Society

The First Fifty Years of the Institute for Research in Social Science at the University of North Carolina

by Guy Benton Johnson and Guion Griffis Johnson

This clearly written and informative social history of a pioneering academic organization surveys the work of its distinguished founder, Howard W. Odum, and traces its research programs over a period of fifty years. The authors were both in the first group of research assistants who received Institute appointments in 1924.

approx. 440 pp., $20.00
September 1980

The University of
North Carolina Press
Post Office Box 2288
Chapel Hill, North Carolina 27514

A Social Gathering of Penguins and Vikings

BREAKTIME: Living Without Work in a Nine-to-Five World
Bernard Lefkowitz
Breaktime examines the changing role of work in American society through the experiences of men and women of all ages, income levels, and life styles who have found alternatives to the regimen of the traditional workday. "Combines sensitivity to the social trends transforming our culture with a loving feeling for the individuals caught in these trends"—DANIEL YANKELOVICH.
0-14-005547-9 448 pp. $ 3.50

THE VIEW IN WINTER: Reflections on Old Age
Ronald Blythe
An unflinching, yet sensitive, inquiry into the experience of aging by the author of the now classic *Akenfield.* "A wise and beautiful book, splendid in its conception, lyrical in its prose . . . a classic in the literature of old age" —*The New York Times Book Review.*
0-14-005663-7 288 pp. $ 4.95

ENTROPY: A New World View
Jeremy Rifkin with Ted Howard
Derived from the Second Law of Thermodynamics, the Entropy Law explodes the myth of progress, stating that all energy inevitably flows from the orderly to the disorderly. This impeccably credentialed work shows how entropy is inextricably connected to the past, present, and future development of society. A Viking Book.
0-670-29717-8 324 pp. $10.95

ONE NATION DIVISIBLE: Class, Race, and Ethnicity in the United States Since 1938
Richard Polenberg (Cornell University)
Combining social, cultural, economic, and political perspectives, this lively study focuses on patterns of class, race, and ethnicity which have been of central importance in the lives of the American people.
0-14-021246-9 368 pp. $ 4.95

Announcing the February 1981 publication of . . .

THE WHISPERINGS WITHIN
David Barash
This highly accessible study draws upon the latest developments in sociobiology to explore the relationship between genetics and human behavior. "A delightful book, forcefully, effectively argued, with exceptionally good examples" —EDWARD O. WILSON, author of *On Human Nature.*
0-14-005699-8 288 pp. $ 4.95

College Department, 625 Madison Avenue, New York, N.Y. 10022

VIKING PENGUIN VIKING PENGUIN

Sociological Perspectives from Wiley...

Forthcoming titles...

URBAN SOCIETY

An Ecological Approach, 2nd Ed.

Amos H. Hawley, *Universtiy of North Carolina*

Proceeding from the position that the form and content of man's collective life is a function of the efficiency of his means of transportation and communication, this text presents—in systematic form—the concepts, theories, and research findings on urban phenomena. The book explores the interrelations among the many kinds of urban knowledge, organizing them into an intelligible unity and revealing the relevance of this synthesis for an understanding of society as a whole. This revised edition brings all data up-to-date, with sections rewritten to incorporate new research findings.
(0 471 05753-3)   Jan. 1981   approx. 350 pp. paper

SOCIOLOGICAL THEORY

Classical Founders and Contemporary Perspectives

D. Paul Johnson, *University of South Florida*

Designed as a text in sociological theory, Johnson explains the key distinctions between the major underlying assumptions of the various theorists and schools of theory as he demonstrates their empirical and practical importance. Covers five classical pioneers of the field (Comte, Marx, Durkheim, Weber, and Simmel) and five contemporary schools of theory (symbolic interactionism, exchange theory, functionalism, conflict theory, and open systems theory). (0 471 02915-7)   1981   approx. 600 pp.

SOCIAL PSYCHOLOGY THROUGH SYMBOLIC INTERACTION, 2nd Ed.

Gregory P. Stone, *University of Minnesota,* & Harvey A. Farberman, *SUNY at Stony Brook*

A reader from the symbolic interactionist perspective for the social psychology course in sociology. Written by renowned authors who have made theoretical and substantive contributions to the perspective. The revised edition is designed for undergraduates, incorporating a pedagogical format, introductions and selections geared for that group. The new edition contains 41 reading selections—many of which are original.
(0 471 03029-5)   Jan. 1981   approx. 450 pp. paper

THE FAMILY AND SOCIAL CHANGE

Comparative Perspectives

Mark Hutter, *Glassboro State College*

An introduction to the study of the American Family from a crosscultural and historical perspective.
(0 471 08394-1)   Feb. 1981   approx. 500 pp.

UNDERSTANDING MARRIAGE AND THE FAMILY

James M. Henslin, *Southern Illinois University,* & James R. Hayes, *Southwest State University, Minnesota*

Look for this exciting cross-cultural reader for your courses in Marriage and the Family and Sociology of the Family.
(0 471 08606-1)   Feb. 1981   approx. 300 pp. paper

Recently published titles...

SOCIAL PROBLEMS

Divergent Perspectives

Thomas Sullivan, Kenrick Thompson, Richard Wright, George Gross, and Dale Spady, *all of Northern Michigan University*

A readable, yet comprehensive analysis of the major social problems confronting contemporary American society.
(0 471 02932-7)   1980   728 pp.

MARRIAGE IN A CHANGING WORLD, 2nd Ed.

Gerald R. Leslie, *University of Florida,* & Elizabeth M. Leslie

A comprehensive book that focuses on preparation for successful marriage and family living, covering the entire life-cycle from birth to death.
(0 471 05593-X)   1980   516 pp.

FREEDOM AND GROWTH IN MARRIAGE, 2nd Ed.

James Leslie McCary, *formerly with the University of Houston*

Here is an introduction to marriage and the family that emphasizes sexual adjustment and marital growth and development for the individual partners. Takes an unbiased view toward alternatives to marriage as well as differing types of marriage.
(0 471 05341-4)   1980   498 pp. paper

SOCIOLOGY

A Critical Approach to Power, Conflict and Change, 2nd Ed.

J. Victor Baldridge, *Higher Education Research Institute*

An introduction to sociology that takes a strong reformist approach, with special attention paid to macro-sociology. Emphasizes large-scale social institutions and their role in contemporary society. Action Manual and Instructor's Manual available.
(0 471 04708-2)   1980   547 pp.

To be considered for complimentary copies, write to R. Leyh, Dept. 7077. Please include course name, enrollment, and title of present text.

JOHN WILEY & SONS, Inc.
605 Third Avenue, New York, N.Y. 10158
In Canada: 22 Worcester Road, Rexdale, Ontario
Prices subject to change without notice.   1-7077

Help your students

grow in knowledge –

CONTEMPORARY SOCIAL PROBLEMS
Robert R. Bell, Temple University

cultivate new ideas –

HUMAN RESPONSES TO SOCIAL PROBLEMS
Louis Schneider, late of the University of Texas, Austin,
Cookie White Stephan, New Mexico State University,
Louis A. Zurcher, and Sheldon Eckland-Olson, both of
the University of Texas

put down roots –

THE EMERGENCE OF SOCIAL THEORY
Jonathan Turner, University of California, Riverside, and
Leonard Beeghley, University of Florida

with these new texts for 1981

Recently Published

INTERVIEWING:
Strategy, Techniques, and Tactics, Third Edition
Raymond L. Gorden, Antioch College

EXPERIENCING SOCIAL RESEARCH, Revised Edition
John F. Runcie, Public Systems Evaluation, Inc.

THE URBAN ENVIRONMENT
Samuel E. Wallace, University of Tennessee, Knoxville

Examination copies for adoption consideration available on request; please indicate course title and text presently used.

THE DORSEY PRESS **HOMEWOOD, ILLINOIS 60430**

Great Scott!

Sociology: Human Society

Third Edition

DeFleur/D'Antonio/DeFleur

January 1981, 640 pp, illus, hard

Solid sociology for your introductory course. Totally rewritten text includes new chapters on research in sociology, group life, age and sex roles, health and social welfare, plus extensive coverage of personal and career applications of sociology. Outstanding new illustration program, including 4-color photo essays. Multiple learning aids. **Instructor's Manual; Study Guide; Test Items; and Reader (see below).**

Sociology: Readings on Human Society

John E. Owen

January 1981, 416 pp, soft

Collection of 47 readings, with selections relevant to such current issues as women's liberation, marital tensions, war, and nationalism. Wide range of themes adaptable to any sociology text.

Social Psychological Theories

A Comparative Handbook

Carl J. Slawski

November 1980, 256 pp, soft

Supplementary text-with-readings shows students how to apply theory to their own personal life and experience.

Basic Sociological Research Design

Grant Bogue

December 1980, 288 pp, soft

Detailed discussion of the principles of sociological research design for beginning students in research methods, applied sociology, and related courses.

The Student Sociologist's Handbook

Third Edition

Bart/Frankel

December 1980, 228 pp, soft

Invaluable supplement provides overview of the field, introduces latest research methods and sources, and guides students, step-by-step, in writing research papers.

For further information write
Jennifer Toms, Department SA
1900 East Lake Avenue
Glenview, Illinois 60025

Scott, Foresman and Company

North American Social Report

A Comparative Study of the Quality of Life in Canada and the U.S.A. from 1964 to 1974

ALEX C. MICHALOS
Social Indicators Research Programme, University of Guelph, Canada

The *Report* will be published in five volumes.

I. Foundations, Population, and Health
1980, x + 278 pp. Paper Dfl. 45,– / US $ 23.50 ISBN 90-277-1058-9
II. Crime, Justice, and Politics
1980, xvi + 252 pp. Paper Dfl. 45,– / US $ 23.50 ISBN 90-277-1085-6
III. Science, Education, and Recreation – January 1981
IV. Environment, Transportation, and Economics – Summer 1981
V. Housing, Religion, and Morality – Spring 1982

D. Reidel Publishing Company
P.O. Box 17, 3300 AA Dordrecht, Holland
190 Old Derby St., Hingham, MA 02043, U.S.A.

"The first comprehensive presentation of the vast structuralist panorama."*

THE AGE OF STRUCTURALISM

Lévi-Strauss to Foucault

Edith Kurzweil

"Provides succinct summaries of the ideas of Claude Lévi-Strauss, Roland Barthes, Jacques Lacan, Michel Foucault, and Paul Ricoeur, among others....Unlike most commentators on structuralism, who are mainly concerned with its conceptual coherence or lack of it, Edith Kurzweil makes the welcome attempt to link the varieties of structuralism and the reactions against them to the particular circumstances of contemporary France."
—Eugene Goodheart, *Boston University*

"This valuable and wide-ranging survey...stands out for being critical as well as sympathetic, lucid as well as thoughtful."
—Morris Dickstein, *Queens College*

"An important contribution to contemporary intellectual and social history."—*Kurt H. Wolff, *Brandeis University*

$5.95 paper; $20.00 cloth

Available at better bookstores or direct from:
COLUMBIA UNIVERSITY PRESS
136 South Broadway, Irvington, New York 10533

WORKERS AND ORGANIZATIONS

New Books from MIT

GETTING STARTED:
The Youth Labor Market
by Paul Osterman
Over a million and a half youth leave high school every year and do not go on to college. Most enter the labor market. How these young men and women get started—find, choose, are selected for or sorted into jobs—is the central topic of this study. "*Getting Started* is the best book on the youth labor market that I have seen."—Leonard J. Hausman, Associate Professor of Economics, Brandeis University.
$20.00

BLACK WOMEN IN THE LABOR FORCE
by Phyllis A. Wallace
with the assistance of Linda Datcher and Julianne Malveaux
Phyllis Wallace was a key figure in the A T & T case, the largest civil rights settlement ever established in the United States. In this new book, she analyzes the recent economic literature on black women workers and makes specific recommendations for improving their status in the labor market. The study's approach also provides insights on how to better utilize *all* women in the labor force.
$15.00

LIVING WITH TECHNOLOGY:
Issues at Mid-Career
by Lotte Bailyn
in collaboration with Edgar H. Schein
An extensive foray into the diverse careers and midlife attitudes, aspirations, and prospects of more than 1,350 people who graduated from MIT in the 1950s. "This is the best analysis I have seen of a large-scale survey of people with technical educations."—Gene W. Dalton, Professor of Organizational Behavior, Brigham Young University.
$15.00

CONTROL AND IDEOLOGY IN ORGANIZATIONS
edited by Graeme Salaman and Kenneth Thompson
A general overview of theory and research on organizations, from the classic work of Weber, Durkheim, and Marx, to the most recent theoretical and empirical advances. "The book is very up-to-date; in fact, it is the best synthetic treatment of organizations that I know of that incorporates the range of recent thinking and research. The writing is uniformly quite good, and in a few places rises to a wonderful vividness."—Randall Collins, Professor of Sociology, University of Virginia.
$12.50 paperback

The MIT Press

28 Carleton Street
Massachusetts Institute of Technology
Cambridge, Massachusetts 02142

CONTEMPORARY SOCIOLOGY
A Journal of Reviews

Norval D. Glenn, Editor
(Bi-monthly ISSN 0094-3061)

A journal of reviews, criticism, and debate designed to give new thrust and style to reviewing and intellectual exchange within the field.

Contemporary Sociology publishes reviews and critical discussions of recent works in sociology and in related disciplines which merit the attention of sociologists. Since all sociological publications cannot be reviewed, a selection is made to reflect important trends and issues in the field.

Recent issues have featured articles by:

• David B. Bills • Kenneth C. Boulding • Theodore E. Long • Alex Inkeles • Amos H. Hawley • Edward A. Tiryakian • Lewis A. Coser • William Julius Wilson • Alfred Kuhn • James Grier Miller • Anthony Giddens • Everett K. Wilson

Members, $10 Non-members, $15
Non-member students, $10 Institutions, $20
Single issues, $4

American Sociological Association
1722 N Street, N.W.
Washington, D.C. 20036

Now in paperback...

Sociology and the Twilight of Man
Homocentrism and Discourse in Sociological Theory

By CHARLES C. LEMERT. "Lemert's analysis is fascinatingly original.... His challenge will be worth the attention of the professional sociologist." – *Library Journal*. "Lemert writes exceptionally well and clearly." – *Choice*.
0-8093-0975-0/$5.95 paper
0-8093-0851-7/$17.50 cloth

Ransom Kidnapping in America, 1874-1974
The Creation of a Capital Crime

By ERNEST KAHLAR ALIX. "An important work.... Little material of a scholarly nature has been written about kidnapping, and Alix's contribution will likely stand as the major reference on this subject for some time to come."
–*Contemporary Sociology.*
0-8093-0976-9/$5.95 paper
0-8093-0849-5/$15.00 cloth

SOUTHERN ILLINOIS UNIVERSITY PRESS
P.O. Box 3697, Carbondale, Illinois 62901

THE AMERICAN SOCIOLOGIST

(James L. McCartney, *Editor*)

(Quarterly—ISSN 0003-1232)

The American Sociologist contains major articles analyzing sociology as a profession and as a discipline. Included are papers on such professional concerns as the use of sociological knowledge and skills in academic and non-academic settings; the conditions of work and maintenance of professional standards; the ethical, practical, and intellectual issues related to research; practical problems affecting sociologists as professionals (e.g., taxes, publication, copyright); ideological issues related to the development of new perspectives.

Recent issues have included articles on:

- *Field Work and the Protection of Human Subjects*
  DUSTER, MATZA & WELLMAN
- *Filthy Pictures or the Case of the Fraudulent Social Scientist: Unmasking the Phony Expert,* ST. GEORGE & McNAMARA
- *Representations of Merton's Theory of Anomie*
  HILBERT & WRIGHT
- *Universality of Science and the Question of National Sociologies,*
  HILLER

ASA members, $8 **Non-members, $12** **Institutions, $16**
Single issues, $4

Order from:* *American Sociological Association
1722 N Street, N.W.
Washington, D.C. 20036

SOCIAL AND BIOLOGICAL PREDICTORS OF NUTRITIONAL STATUS, PHYSICAL GROWTH, AND NEUROLOGICAL DEVELOPMENT

Edited by LAWRENCE S. GREENE and FRANCIS E. JOHNSTON

CONTENTS: *F. E. Johnston*, Introduction: The Causes of Malnutrition. THE ANTHROPOMETRIC ASSESSMENT OF NUTRITIONAL STATUS. *J. H. Himes*, Subcutaneous Fat Thickness as an Indicator of Nutritional Status. *G. G. Harrison and M. White*, Overweight in Arizona Infants: Relation to Birthweight and Ethnic Group. *A. R. Frisancho*, Role of Calorie and Protein Reserves on Human Growth During Childhood and Adolescence in a Mestizo Peruvian Population. BREAST-FEEDING, BOTTLE-FEEDING, AND DISEASE EXPERIENCE. *W. A. Stini et al.*, Lean Tissue Growth and Disease Susceptibility in Bottle-Fed Versus Breast-Fed Infants. *R. Martorell*, Interelationships Between Diet, Infectious Disease and Nutritional Status. BENIGN NEGLECT. *C. M. Cassidy*, Benign Neglect and Toddler Malnutrition. EMPIRICAL STUDIES. *R. M. Malina*, Biosocial Correlates of Motor Development During Infancy and Early Childhood. *E. Pollitt and R. Leibel*, Biological and Social Correlates of Failure to Thrive. *T. J. Marchione and F. W. Prior*, The Dynamics of Malnutrition in Jamaica. *L. S. Greene*, Social and Biological Predictors of Physical Growth and Neurological Development in An Area Where Iodine and Protein—Energy Malnutrition are Endemic. *J. D. Haas*, Maternal Adaptation and Fetal Growth at High Altitude in Bolivia. F. E. Johnston et al., A Factor Analysis of Correlates of Nutritional Status in Mexican Children, Birth to 3 Years. CONCLUSIONS. *L. S. Greene*, Factors Affecting Nutritional Status, Physical Growth, and Neurological Development: Final Comments. Subject Index.

References appear at the end of each chapter.

1980, 368 pp., $25.00 ISBN: 0-12-299750-6

THE POLITICS OF WORKERS' PARTICIPATION

The Peruvian Approach in Comparative Perspective

By EVELYNE HUBER STEPHENS

A Volume in the STUDIES IN SOCIAL DISCONTINUITY Series

The Politics of Workers' Participation argues that workers' participation is a vital aspect in the distribution of power among social classes. Consequently, the development of workers' participation must be understood in the context of the struggle between sociopolitical forces to consolidate or transform a given social order.

The book illustrates four politico-economic systems: liberal-pluralist, authoritarian-corporatist, bureaucratic-centralist and democratic-socialist. Four stages of workers' participation—origins, purpose and design, development and effects, and supportive policies—are distinguished, and dynamics in these stages are linked to the functioning of sociopolitical forces at both the national-political and enterprise levels.

The book examines five specific cases: France, the Federal Republic of Germany, and Sweden to illustrate developed capitalist democracies; Yugoslavia as an example of a developing socialist authoritarian system; and Peru as an example of a developing capitalist authoritarian system.

1980, 304 pp., $29.50 ISBN: 0-12-666250-9

THE PROFESSIONAL STRANGER

An Informal Introduction to Ethnography

By MICHAEL H. AGAR

The Professional Stranger introduces an ethnographic perspective on the study of human life. Ethnography's goal is defined as the acquisition of an ability to interpret events as do members of some group. The perspective is further explored by contrasting ethnography with more traditional hypothesis testing approaches in the social sciences. Numerous examples are used, drawn for the most part from the author's field experiences in South India and with urban U.S. heroin addicts. While not intended as a methodological cookbook, several problems in ethnographic interviewing and observation are discussed. Throughout, there is a concern with the difficult task of providing systematic documentation of key ethnographic conclusions.

1980, 240 pp., $12.50 (Paperback)
*ISBN: 0-12-043850-X(t)**

*To obtain examination copies please write to the Sales Department, Academic Press, Inc. Requests must include your position, course of instruction, and enrollment.

Continuation Orders authorize us to ship and bill each volume in a series, or "Advances" type publication automatically, immediately upon publication. This order will remain in effect until cancelled. Specify the volume number or title with which your order is to begin.

Send payment with order and save postage and handling charge.
Prices are subject to change without notice.

ACADEMIC PRESS, INC.
A Subsidiary of Harcourt Brace Jovanovich, Publishers
111 FIFTH AVENUE, NEW YORK, N.Y. 10003/24-28 OVAL ROAD, LONDON NW1 7DX

Museums of Madness
The Social Organization of Insanity in 19th Century England

Andrew Scull, *University of California, San Diego*

"*Museums of Madness* fills an important gap in the history of the insane. It is the first adequate account of the rise of the asylum in England, the country which led the world in adopting its use....This is an excellent contribution to the growing literature on an important subject."
—Peter McCandless in *Albion*

"*Museums of Madness* is sociological history at its stimulating best." —W. F. Bynum in *Psychological Medicine*

1979 ISBN 0-312-55355-2 275 pp., cloth $18.95

The View from Goffman
edited by Jason Ditton, *University of Glasgow*

Although the brilliance and originality of Erving Goffman's contributions to sociology and anthropology are widely recognized, his major contribution to the development of social theory is often unappreciated. The contributors to this book seek to correct that misimpression, to show that what Goffman has produced is not merely a series of unconnected snapshots of modern urban life. Their papers—all specially commissioned for this volume—constitute the first extended analysis of Goffman's sociology as a coherent body of theory.

1980 ISBN 0-312-84598-7 289 pp., cloth $25.00

Sociology and Theology
Alliance and Conflict

edited by David Martin, John Orme Mills and W.S.F. Pickering

This pioneering study brings together two disciplines not usually considered in intellectual conjunction in order to examine the new perspectives that each can offer the other. The essays made available in this volume, all published here for the first time, explore such questions as: how aware are theologians of the social determinants of theology? how useful are sociological enquiries based on what may appear to be theologically naive descriptions of related ideologies?

1980 ISBN 0-312-74007-7 204 pp., cloth $22.50

Identity and Authority
Explorations in the Theory of Society

edited by Roland Robertson, *University of Pittsburgh*, and Burkhart Holzner, *University of Pittsburgh*

In seven extensive, specially commissioned essays, this book explores the relationship between structures of authority in societies and various modes of personal identity. The contributions include analytical, theoretical and comparative discussions on the theme of identity and authority, as well as essays that focus particularly on recent shifts in American society in the relationship between societal macro-structures and individual modes of existence.

1980 ISBN 0-312-40448-4 318 pp., cloth $27.50

The Sociological Enterprise
C. C. Harris, *University of Swansea*

Over the past decade, C. C. Harris argues, the proliferation of sociological investigations of widely varying problems has obscured the nature of sociology as a systematic enterprise. His purpose in this book is to devise a way of organizing the concepts underlying the varieties of sociological practice in order to constitute a set of concepts fundamental to the aims of sociology as an academic discipline.

1980 ISBN 0-312-73968-0 about 240 pp., cloth $22.50

Corporations, Classes and Capitalism
John Scott, *University of Leicester*

In this study, John Scott examines the transformation of industrial property over the last hundred years, exploring the significance of this transformation for the actual structure of control over business decision-making. He investigates the roles of stockholders, bankers and financial institutions in an effort to identify the main patterns of power and influence in the economy; analyzes the relationships among indusrial property, the class structure and the state; and examines the role of the multinational corporation in the world economy.

1980 ISBN 0-312-17011-4 about 219 pp., cloth $22.50

To order these books for your library at 20% discount,
please write:

St. Martin's Press
Post Office Box 5352 New York, NY 10163

CHARLES C THOMAS • PUBLISHER

New!

POLICE-COMMUNITY RELATIONS

Crisis in Our Time

Third Edition

By **Howard H. Earle,** *California State Univ., Los Angeles. Foreword by* **William M. Conroy.** Divided into four sections, the newly revised edition of this monograph opens with a review of the general problems of police-community relations, including a look at past events influencing police-community relations and an analysis of misunderstandings and grievances between the public and the police. The author then describes the kinds of communication that help to create positive police-community relations. Guidelines for developing police displays, press releases, news media contact, agency tours, and other activities are presented. Sociological factors that thwart police objectives, such as the isolation of the police officer from the community, the behavior and dynamics of small groups, folkways, and prejudice, are then explored. A concluding section examines current efforts and future plans to establish improved relations between police and community. *'80, 304 pp., 5 il., 1 table, $16.75*

New! **MENTAL RETARDATION: A Phenomenological Approach** *edited by* **Jerry Jacobs.** Mental retardation and the problems it entails are discussed from the perspective of the mentally retarded individual and his/her family. *'80, cloth-$24.50, paper-$16.75*

New! **IMPROVING EFFECTIVENESS AND REDUCING COSTS IN MENTAL HEALTH** *by* **Brian T. Yates.** Procedures for assessing, analyzing, and improving effectiveness, while reducing costs, for a wide variety of mental health services are presented. *'80, $19.75*

New! **COMPETITIVE BEHAVIORS OF OLYMPIC GYMNASTS** *by* **John H. Salmela.** The physical, mental, emotional, and social behaviors of competing Olympic gymnasts are examined. Male/female and cross-cultural comparisons are discussed. *'80, cloth-$19.75, paper-$14.50*

OCCUPATIONAL HEALTH AS HUMAN ECOLOGY *by* **Stewart Wolf, John G. Bruhn** *and* **Helen Goodell.** The relationship of personal fulfillment and job satisfaction to a worker's health is analyzed. *'78, $14.00*

THE NATURE OF MAN: A Social Psychological Perspective *edited by* **Richard L. Gorsuch** *and* **H. Newton Malony.** Data relating social psychology to ethics, moral decision making, and the state of man's existence is herein presented. *'76, $16.25*

New! **BECOMING A WOMAN: The Socialization of Gender** *by* **Bernice Lott.** The female experience from conception through old age and those aspects of the socialization process common to American women are scrutinized. *'80, cloth-$24.75, paper-$18.50*

New! **POLICE SYSTEMS OF EUROPE: A Survey of Selected Police Organizations (2nd Ed.)** *by* **Harold K. Becker.** This text's cross-cultural perspective of European police organization, management, and operation will offer readers new insight on ways to solve police problems in the U.S. *'80, $13.75, paper*

New! **THE EFFECT OF AIR IONIZATION, ELECTRIC FIELDS, ATMOSPHERICS AND OTHER ELECTRIC PHENOMENA ON MAN AND ANIMAL** *by* **Felix Gad Sulman.** The effects of air electricity on crime rate, urbanization patterns, and health are among the topics covered. *'80, cloth-$24.75, paper-$18.75*

THE CHANGING STATUS OF THE BLIND: From Separation to Integration *by* **Berthold Lowenfeld.** The education, rehabilitation, and socioeconomic status of the blind as well as public policy and attitudes toward blinded individuals are examined. *'75, $30.00*

LAW, LANGUAGE AND COMMUNICATION *by* **Walter Probert.** The author presents an analysis of the subtle relationships between law and the dynamics of verbal communication. *'72, $17.25*

Orders with remittance sent, on approval, postpaid

301-327 East Lawrence Avenue • Springfield • Illinois • 62717

> "Rarely does a book combine such good theory, comprehensive scholarship, meaningful empirical data, and practical suggestions for social policy. Robin Williams has produced one of the best monographs in several years."
>
> (*American Journal of Sociology*, 1974)

The late Arnold Rose's evaluation of *Strangers Next Door* by Robin Williams, Jr., is equally applicable to Robin's contributions to the profession, and to his very active role in encouraging and stimulating others to do their best work.

Just 18 years ago, Robin Williams joined The Dorsey Press as its Advisory Editor in Sociology and Anthropology. His efforts and expertise have guided Dorsey in building an impressive list of more than 40 textbooks and monographs.

As he retires from his position with The Dorsey Press, we want to express our deepest appreciation and best wishes for a long and productive relationship which will continue to influence The Dorsey Press and American sociology.

Charles M. Bonjean, Hogg Professor of Sociology at the University of Texas and Vice President of the Hogg Foundation for Mental Health will succeed Robin as Dorsey's Consulting Editor.

Dr. Bonjean's qualifications are impressive, including the co-editing and co-authoring of seven books as well as numerous chapters and articles. Chuck is currently editor of the *Social Science Quarterly*.

During the last school year, Chuck and Robin have worked together to achieve a smooth transition at Dorsey. Chuck plans no major changes in the tradition established by Robin. The year has been a good one and we have every reason to believe that our contributions to sociology during the next two decades will match those of the past.

The Dorsey Press **Homewood, Illinois 60430**

From Prejudice to Destruction

Anti-Semitism, 1700–1933

Jacob Katz

This major reinterpretation of modern anti-Semitism blends the history of ideas with social analysis. Katz describes how a set of negative ideas about the Jews gradually became transformed and then, around 1870, gained so much social force as to result in the premeditated and systematic destruction of the Jewish people of Europe. $20.00

Constancy and Change in Human Development

Orville G. Brim, Jr., and Jerome Kagan, Editors

Orville Brim, Jerome Kagan, and an outstanding group of specialists in human growth assess questions of change and continuity in physical, mental, and emotional development throughout the life span. $27.50

Changing Youth in a Changing Society

Patterns of Adolescent Development and Disorder

Michael Rutter

"A thoughtful and thought-provoking assessment of the current state of knowledge on adolescent development and disorder...There is no better source for investigator and practitioner."—*Glen H. Elder, Jr.,* Cornell University $14.00

Now available in Paperback—

Poverty and Progress

Social Mobility in a Nineteenth Century City

Stephan Thernstrom

Joint Center for Urban Studies

$4.95 paper; $16.00 cloth

Mind in Society

The Development of Higher Psychological Processes

L. S. Vygotsky

$4.95 paper; $12.50 cloth

Chicanos in a Changing Society

From Mexican Pueblos to American Barrios in Santa Barbara and Southern California, 1848–1930

Albert Camarillo

$7.95 paper; $17.50 cloth

Women in the Muslim World

Lois Beck and Nikki Keddie, Editors

$8.95 paper; $35.00 cloth

Case Studies in Medical Ethics

Robert M. Veatch

$7.95 paper; $16.50 cloth

The Rebellious Century

1830–1930

Charles Tilly, Louise Tilly, and Richard Tilly

$7.95 paper; $18.50 cloth

The Political Economy of Inflation

Fred Hirsch and John H. Goldthorpe, Editors

$5.95 paper; $14.00 cloth

Harvard University Press

Cambridge, Massachusetts 02138

Coming this winter:

Family Impact Seminar Series

The influence of government policies on families and family life is examined in three separate volumes on foster care, teenage pregnancy, and work schedules.

Balancing Jobs and Family Life:
Do Flexible Work Schedules Help?
Halcyone H. Bohen and Anamaria Viveros-Long

This study compares 700 workers in two federal agencies in Washington, D.C., to evaluate the effects of flexible work schedules on family stress, personal schedules, and the sharing of chores and childrearing between parents. Through surveys and in-depth interviews, the book looks at how different kinds of households (single men and women, single-earner families, and especially dual-earner families) use flexitime. Approx. 325 pp. 3 photos January ISBN 199-5 $19.50

Beyond Alcoholism
Alcohol and Public Health Policy
Dan E. Beauchamp

Challenging the traditional concept of alcoholism, which blames the victim and ignores the social and cultural factors that influence alcohol use, this book goes beyond the study of drinking behavior to the larger issue of defining and understanding social problems in a public health context. Approx. 250 pp. November ISBN 189-8 $17.50

From Suburb to Shtetl
The Jews of Boro Park
Egon Mayer

An analysis of the broad sociological factors that shaped the transformation of a conventional residential neighborhood in Brooklyn, New York, into one of the most important orthodox Jewish communities. 208 pp. 7 photos 1979 ISBN 161-8 $15.00

Now available in paperback

Homeboys
Gangs, Drugs, and Prison in the Barrios of Los Angeles
Joan W. Moore

1979 Spivack Award winner, Joan W. Moore, in collaboration with Chicano pintos (ex-convicts) and community workers, examines the world of the Los Angeles barrios. 240 pp. 3 maps 1978 cloth ISBN 121-9 $17.50 paper ISBN 114-6 $7.95

Black Testimony
Voices of Britain's West Indians
Thomas J. Cottle

In their own words, more than twenty black men, women, and children living in London discuss their homes, families, education, work and unemployment, immigration to England, and their feelings about their white fellow-citizens. 184 pp. 1980 ISBN 186-3 $12.50

Temple University Press
Broad & Oxford Sts. Philadelphia 19122

The long-awaited translation of
MAURICE HALBWACHS'
THE COLLECTIVE MEMORY
Introduction by Mary Douglas
translated from the French by Francis J. Ditter, Jr. and
Vida Yazdi Ditter

A leading French sociologist of the 1920s and 30s and Emile Durkheim's foremost student, Halbwachs provided a major development in the theory of the sociology of knowledge. He separated the elements of social life that contribute to the collective memory, an achievement that has been largely responsible for contemporary sophistication on the subject. Here, in this work, is the essence of his thinking on collective memory. "No one before Halbwachs had dared to take on the question of considering how it is that the institutional framework of society affects memory . . . . he fashions new questions that no others in that tradition have examined." —Robert K. Merton

A Paperback Original
256 pages
CN 800, $4.95

Harper & Row
10 E. 53d St., New York 10022

909

Afro-Americans
Albanians
Aleuts
Alsatians
American Indians
Amish
Appalachians
Arabs
Armenians
Assyrians
Australians & New Zealanders
Austrians
Azerbaijanis
Bangladeshi
Basques
Belgians
Belorussians
Bosnian Muslims
Bulgarians
Burmese
Canadians, British
Cape Verdeans
Carpatho-Rusyns
Central & South Americans
Chinese
Copts
Cornish
Cossacks
Czechs
Danes
Dominicans
Dutch
East Indians
English
Eskimos
Estonians
Filipinos
Finns
French
French Canadians
Frisians
Georgians
Germans
Germans from Russia
Greeks
Gypsies
Haitians
Hawaiians
Hungarians
Hutterites
Icelanders
Indochinese
Indonesians
Iranians
Irish
Italians
Japanese
Jews
Kalmyks
Koreans
Manx
Mexicans
Mormons
North Caucasians
Norwegians
Pacific Islanders
Pakistanis
Pennsylvania Germans
Poles
Portuguese
Puerto Ricans
Romanians
Russians
Scotch-Irish
Scots
Serbs
Slovaks
Slovenes
South Africans
Southerners
Spaniards
Spanish
Swedes
Swiss
Tatars
Thai
Turkestanis
Turks
Ukrainians
Welsh
Wends
Afro-Americans
Albanians
Aleuts
Alsatians
American Indians
Amish
Appalachians
Arabs
Armenians
Assyrians
Australians & New Zealanders
Austrians
Azerbaijanis
Bangladeshi
Basques
Belgians
Belorussians
Bosnian Muslims
Bulgarians
Burmese
Canadians, British
Cape Verdeans
Carpatho-Rusyns
Central & South Americans
Chinese
Copts
Cornish
Cossacks
East Indians
English
Eskimos
Estonians
Filipinos
Finns
French
French Canadians
Frisians
Georgians
Germans
Germans from Russia
Greeks
Gypsies
Haitians
Hawaiians
Hungarians
Hutterites
Icelanders
Indochinese
Indonesians
Iranians
Irish
Italians
Japanese
Jews
Kalmyks
Koreans
Kurds
Latvians
Lithuanians
Luxembourgers
Norwegians
Pacific Islanders
Pakistanis
Pennsylvania Germans
Poles
Portuguese
Puerto Ricans
Romanians
Russians
Scotch-Irish
Scots
Serbs
Slovaks
Slovenes
South Africans
Southerners
Spaniards
Spanish
Swedes
Swiss
Tatars
Thai
Turkestanis
Turks
Ukrainians
Welsh
Wends
West Indians
Yankees
Zoroastrians
Acadians
Afghans
Africans

WE THE PEOPLES

Introducing

Harvard Encyclopedia of American Ethnic Groups

Stephan Thernstrom, Editor/Ann Orlov, Managing Editor/
Oscar Handlin, Consulting Editor

An indispensable reference work for home, library, and office, the Harvard Encyclopedia gives every ethnic group in the United States an honored place. And its authoritative accounts of origins, histories, culture, distinctive characteristics, and current status make lively reading indeed.

"There is literally no other work which approaches it in comprehensiveness. Even more impressive, however, is the quality of the articles. The editors should be congratulated."
—Seymour Martin Lipset

Harvard Encyclopedia of American Ethnic Groups. 87 maps 1076 pp.
$49.50 until December 31, 1980; $60.00 thereafter.

The Belknap Press of
Harvard University Press
Cambridge, Massachusetts 02138

ROUTLEDGE & KEGAN PAUL

9 Park Street, Boston, Mass. 02108

Marx Against Marxism

JULIUS LOEWENSTEIN
Leo Baek Prize Winner
Translated by Harry Drost

This book traces the origins, contradictions and consequences of Marx's teachings on his followers. Loewenstein uses Marx's own words to speak against the rigid dogmatism inherent in much of Marxism, and concentrates on the interpretations of Marx's work by Max Weber.
October $20.00

Sociology, Work and Industry

TONY J. WATSON
Lecturer in Industrial Sociology,
Trent Polytechnic, Nottingham

Designed as a text for sociology and social science students specializing in the fields of work, industry and organization, the author seeks to integrate these often disparate study topics.
$30.00, paper $15.00

White Collar Proletariat

The Industrial Behaviour of British Civil Servants
MICHAEL P. KELLY
Lecturer in Sociology, Dundee College of Technology
February Original paperback $21.75

Approaching Sociology

A Critical Introduction
Revised Edition
MARGARET COULSON and
CAROL RIDDELL

This edition of an already successful textbook broadens its scope to incorporate recent sociological research.
$12.95, paper $5.00

London Patidars

A Case Study in Urban Ethnicity
HARALD TAMBS-LYCHE
Lecturer in Social Anthropology,
University of Bergen
Foreword by Professor John Rex

The Patidars, a landowning caste from India, have adopted a merchant ideology which, among those living in London, has become inseparable from their ethnic self-image. This book is an examination of how this form of ethnic boundary relates to the individual within it, and why it is maintained.
October $35.00
International Library of Sociology

The National Front

NIGEL G. FIELDING
Lecturer in Sociology, University of Surrey

This examination of one of Britain's most controversial political parties is aimed particularly at students of the sociology of deviance, political sociology and political science. It describes the ethnography, history, electoral performance and some demographic characteristics of its membership, and investigates key aspects of the party ideology — race, nationalism and conspiracy theory.
November about $25.00
International Library of Sociology

New in paperback

Hermeneutic Philosophy and the Sociology of Art

An Approach to Some of the Epistemological Problems of the Sociology of Art and Literature
JANET WOLFF
University of Leeds
October paper about $12.50
International Library of Sociology

New from Columbia

Exploring the City

Inquiries Toward an Urban Anthropology

ULF HANNERZ. In this bold first attempt to provide a coherent and unified theoretical basis for the study of urbanism, Ulf Hannerz draws together central ideas about the city and urbanism from anthropological, historical, sociological, and geographical sources. This wide-ranging analysis considers the contributions to urbanism from a variety of individuals, groups, and schools, including Weber, Simmel, Marx via the Chicago School, and the Manchester School. 384 pages, $15.00

Social Science Research and Decision-Making

CAROL H. WEISS with MICHAEL J. BUCUVALAS. This book examines the relationship between social science research and the choices and decisions of government officials and agency managers. It identifies the kinds of social science studies that officials pay attention to, explains when and how they find research studies useful, and shows how they apply the lessons of research to their work. 352 pages, $22.50

The Coming Crisis in British Penology

A. E. BOTTOMS, EDITOR. In essays ranging from "The Justice Model—Analysis and Development" to "The Future of Social Policy in Regard to Crime," ten British sociologists and social workers look at the future of justice and imprisonment in Britain. *Distributed in the U.S. and Canada for Scottish Academic Press.* 224 pages, $12.50 paper

To order send check or money order to Dept. JN at the address below, including $1.30 per order for postage and handling.

COLUMBIA UNIVERSITY PRESS
Address for orders:
136 South Broadway, Irvington, New York 10533

SOCIOLOGY OF EDUCATION

A Journal of Research in Socialization & Social Structure

(Alan C. Kerckhoff, *Editor*)

(Quarterly—ISSN 0038-0407)

Sociology of Education publishes papers on educational processes, and on human development. The research may focus on the individual, institutions, and structural arrangements among institutions bearing on education and human development.

Recent issues have included studies on:

Interactive Influences in Spady and Tinto's Conceptual Models of College Attrition—PASCARELLA & TERENZINI

Loose Structure, Retrenchment and Adaptability in the University—RUBIN

School SES Influences—Composition or Context?—ALEXANDER, FENNESSEY, McDILL & D'AMICO

Academic Sponsorship and Scientists' Careers—RESKIN

Three Perspectives on IQ and Social Differentiation—GUTERMAN, GORDON & RUDERT, ECKLAND

ASA members, $8 **Non-members, $12** **Institutions, $16**
Single issues, $4

Order from: **American Sociological Association**
1722 N Street, N.W.
Washington, D.C. 20036

St. Martin's Press

Coming in '81

Research Methods in the Social Sciences
Second Edition
DAVID NACHMIAS and CHAVA NACHMIAS, both of the
University of Wisconsin, Milwaukee

A basic text for courses in social science research methods, this widely praised book has been updated and expanded, taking into account the advice of both instructors and students who have used the First Edition. New chapters in the Second Edition cover the conceptual foundations of research, research design of pre- and quasi-experiments, questionnaire construction, and scaling and index construction.

Study Guide and *Instructor's Manual* available with the new edition.
February 1981 hardcover

Population
Demography and Policy
ROBERT H. WELLER, *Florida State University,* and
LEON F. BOUVIER, *Population Reference Bureau*

Taking a sociological perspective, Weller and Bouvier examine world and national patterns of population change, growth, and distribution. They look at the social and economic consequences of these trends as well as the individual and societal determinants of population behavior—fertility, mortality, and migration.

Instructor's Manual available. November 1980 hardcover

Society and Freedom
An Introduction to Humanist Sociology
JOSEPH A. SCIMECCA, *George Mason University*

This provocative new text introduces the basic elements and issues of the humanist perspective in sociology. It integrates fundamental sociological concepts with the themes of freedom, legitimacy, inequality, and political change to stimulate thoughtful class discussion.
February 1981 paperbound

Criminal Behavior
Readings in Criminology
edited by DELOS H. KELLY, *California State University, Los Angeles*

Criminal Behavior introduces the student to criminology through an appealing mix of theoretical essays and descriptive accounts of criminal careers. The articles illustrate how various types of crimes and criminal careers evolve, are maintained, and may be transformed.

August 1980 paperbound

Single Life
Unmarried Adults in Social Context
edited by PETER J. STEIN, *William Paterson College*

Single Life is the first comprehensive collection of essays dealing with the diverse population of single adults—a large, growing, and often misunderstood segment of today's society. The selections examine the life styles and major life issues of the divorced, separated, and widowed, as well as the never married.

February 1981 paperbound

The Sociology of Health and Illness
Critical Perspectives
edited by PETER CONRAD, *Brandeis University,* and ROCHELLE KERN, *New York University*

Organized around critical issues in the sociology of health and illness, these readings represent a variety of perspectives. Together, the selections explore the social roots and manifestations of the health care crisis in American society.

February 1981 paperbound

It's A Living
Work in Modern Society
GALE MILLER, *Marquette University*

It's A Living looks at the ways in which people make their livings and give meaning to their work. The text examines a wide range of activities, including professional and factory work, peasant labor, housework, and work in alternative communities.

February 1981 paperbound

To request a complimentary examination copy, please write
(specifying course, present text, and approximate enrollment) to:

St. Martin's Press • P.O. Box 5352 • New York, N.Y. 10163

New Books in Sociology . . .

Now Available

Understanding Abusive Families
James Garbarino and **Gwen Gilliam,**
Boys Town Center for the Study of Youth Development
288 pp. ISBN 0-669-03621-8 $19.95

The Maltreatment of the School-Aged Child
edited by **Richard Volpe, Margot Breton**, and **Judith Mitton**, University of Toronto
Foreword by **David G. Gil**
244 pp. ISBN 0-669-03463-0 $18.95

Discretion and Lawlessness: Compliance in the Juvenile Court
James T. Sprowls, The University of Wisconsin-Milwaukee
Foreword by **Daniel Katkin**
144 pp. ISBN 0-669-03540-8 $14.95

Jurors and Rape
A Study in Psychology and Law
Hubert S. Feild, Auburn University, and
Leigh Buchanan Bienen, Department of the Public Advocate, State of New Jersey
ISBN 0-669-01148-7 Now Available

Public Policy and the Family
Wives and Mothers in the Labor Force
Z. I. Giraldo, Duke University
240 pp. ISBN 0-669-03762-1 $22.95

Minimum-Drinking-Age Laws
An Evaluation
edited by **Henry Wechsler**, The Medical Foundation, Inc.
208 pp. ISBN 0-669-03380-4 $18.95

Economics and the Family
edited by **Stephen J. Bahr,**
Brigham Young University
208 pp. ISBN 0-669-03623-4 $19.95

Social Policies and Programs on Aging
Louis Lowy, Boston University
ISBN 0-669-02342-6 Now Available

Forthcoming

The Law and Politics of Abortion
edited by **Carl E. Schneider** and **Maris A. Vinovskis,**
Michigan Law Review Association
ISBN 0-669-03386-3 November 1980

Network Television and the Public Interest
A Preliminary Inquiry
edited by **Michael Botein** and **David Rice,**
New York Law School
Foreword by **Charles D. Ferris**
ISBN 0-669-02927-0 November 1980

Deviant Street Networks
Prostitution in New York City
Bernard Cohen, City University of New York-Queens College
Foreword by **Marvin E. Wolfgang**
ISBN 0-669-03949-7 November 1980

Adolescent Sexuality
A Study of Attitudes and Behavior
Helen F. Antonovsky,
Ben'Gurion University of the Negev
ISBN 0-669-04030-4 December 1980

HEATH

LexingtonBooks, D. C. Heath and Company
125 Spring Street, Lexington, MA 02173
(617) 862-6650 (212) 924-6460
Call our toll-free number, 800 428-8071

Lexington Books

The Whole World Is Watching

Mass Media in the Making and Unmaking of the New Left

Todd Gitlin

"No phenomenon in American life cries for examination more than the impact of the news media on persons, movements and events. One need not accept all of Gitlin's provocative conclusions to praise the exacting scholarship that has gone into this study of what happens to an anti-establishment movement performing on an establishment stage." —*Daniel Schorr*

"Refreshingly supple and realistic." —*The Nation*
$12.95

Growing Old in Silence

Gaylene Becker

In this first book-length study of deaf people in old age, Becker describes how they adapt to their disability and analyzes its particular implications for becoming old. She adds an important new dimension to our knowledge of both aging and disability.
$10.00

Deafness and Child Development

Kathryn P. Meadow

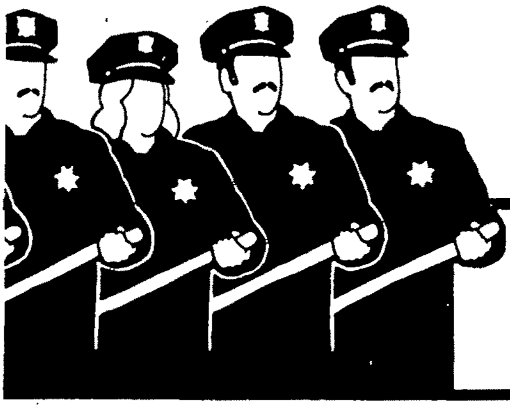
Meadow surveys the central work done on the social, linguistic, cognitive, and psychological effects of profound deafness in children and offers practical discussions with abundant concrete examples. The result is a book that provides a context for understanding research in childhood deafness and ways to apply its findings.
$12.95

Papal Power

A Study of Vatican Control over Lay Catholic Elites

Jean-Guy Vaillancourt

A thoughtful analysis of the papacy's manifold controls over the laity. Vaillancourt answers the fundamental question: "To accomplish its mission, does the Catholic Church need at its head a religious leader who also rules a temporal state and controls enormous wealth, or should it not divest itself of the trappings of power and wealth and function in a more egalitarian manner?"
$16.95



Breaking and Entering

Policewomen on Patrol

Susan Ehrlich Martin

Writing from a feminist as well as a sociological perspective Martin examines the problems female officers face "breaking and entering" into police patrol work, a job traditionally dominated by men.
$14.95

At bookstores

University of California Press
Berkeley 94720

's coming—
he new Broom & Selznick

SOCIOLOGY

A Text with Adapted Readings
Seventh Edition

by LEONARD BROOM, PHILIP SELZNICK,
& DOROTHY BROOM DARROCH

The book that defined the discipline is now extensively redesigned to be the most readable text in the field. Richly documented and systematically organized, the text reflects significant changes in context and organization. It retains its scientific orientation without sacrificing its academic integrity to become a "pop" sociology text.

Broom, Selznick, and new coauthor Dorothy Darroch have clearly modernized all subject areas and terminology in the text. All chapters have been significantly revised. This edition incorporates current and extensive research into new chapters on the life cycle, the sexes, deviance and diversity, and sociological methods. Points made in this visually striking full-color edition are reinforced with adapted readings—30 percent of which are new—and brief text inserts. To further reinforce comprehension, a glossary is included in this edition.

Also available: An instructor's manual with test bank and a study guide.

608 pages/tentative. ISBN 0-06-040991-6. February 1981.

To request examination copies, write to Suite 3D, Harper & Row, 10 East 53d Street, New York, N.Y. 10022. Please include course title, enrollment, and present text.

Harper & Row

17 FEB 1981

American Sociological Review

CALCUTTA UNIVERSITY
CENTRAL LIBRARY

The Presidential Address 889
Peter H. Rossi

Social Process and Hierarchy Formation in Small Groups 905
Ivan D. Chase

Weber's Last Theory of Capitalism: A Systematization 925
Randall Collins

Immigration and the Expansion of Schooling, 1890–1970 943
John H. Ralph and Richard Rubinson

The Meek Shall Not Inherit the Earth 955
L. Richard Della Fave

Research Notes

Suburban Status Instability 972
Harvey M. Choldin, Claudine Hanson, and Robert Bohrer

Generating Newsworthiness 984
Marilyn Lester

Comments 995
Robert Schlaifer, James M. Lutz

Volume 45 December 1980 Number 6

Notice to Contributors

A processing fee of $10 is required for each paper submitted, except for comments; such fees will be waived for student members of ASA. A check or money order, made payable to the American Sociological Association, should accompany each submission. The fee must be paid in order for the review process to begin.

The length of an *ASR* paper is generally ten (10) printed pages or thirty (30) typed pages. However, the *ASR* is happy to accept manuscripts up to fifty (50) typed pages in length for review. We also are interested in receiving manuscripts for a Research and Notes Section.

To permit anonymity in the review of manuscripts, keep identifying material out of the manuscript. Attach a cover page giving authorship, institutional affiliation and acknowledgments, and provide only the title as identification on the manuscript and abstract.

ASA Multiple Submissions Policy: Submission of a manuscript to a professional journal clearly implies commitment to publish in that journal. The competition for journal space requires a great deal of time and effort on the part of editorial readers whose only compensation for this service is the opportunity to read papers prior to publication and the gratification associated with discharge of professional obligation. For these reasons, *the American Sociological Association regards submission of a manuscript to a professional journal while that paper is under review by another journal as unacceptable.*

SUBMISSION AND PREPARATION OF MANUSCRIPTS

1. Submit four (4) copies and retain the original for your files. Copies may be Xerox, mimeograph or multilith, but not carbons.
2. Enclose a stamped self-addressed postcard for acknowledgment of manuscript receipt. *Manuscripts will not be returned.* Please do not send your original copy.
3. All copy must be typed, doublespaced (including indented material, footnotes and references) on 8½ by 11 inch white opaque paper. Lines must not exceed six (6) inches. Margins must be a minimum of one inch.
4. Include four (4) copies of an abstract of no more than 150 words.
5. Type each table on a separate page. Insert a location note at the appropriate place in the text, e.g., "Table 2 about here." Send copies, retain originals.
6. Figures must be drawn in India ink on white paper. Send copies, retain originals.
7. Clarify all symbols with notes in the margins of the manuscript. Circle these and all other explanatory notes not intended for printing.
8. Footnotes should not be placed in the body of the text. Type them (doublespaced) and attach them as a separate appendix to the text. Number them consecutively throughout the text. Footnotes are to be kept to a minimum and used only for substantive observations. Source citations are made within the text rather than in footnotes.
9. Acknowledgments, credits and grant numbers are placed on the title page with an asterisk.

REFERENCE FORMAT

A. *In the text:* All source references are to be identified at the appropriate point in the text by the last name of the author, year of publication and pagination where needed. Identify subsequent citations of the same source in the same way as the first, not using *"ibid.," "op. cit.," or "loc. cit."* Examples follow:

1. If author's name is in the text, follow it with year in parentheses. [". . . Duncan (1959) . . ."]
2. If author's name is not in the text, insert, in parentheses, the last name and year, separated by a comma. [". . . (cf. Gouldner, 1963) . . ."]
3. Pagination (without "p." or "pp.") follows year of publication after a colon. [". . . Kuhn (1970:71)."]
4. Give both last names for dual authors; for more than two use "et al." in the text. When two authors have the same last name, use identifying initials in the text. For institutional authorship, supply minimum identification from the beginning of the complete citation. [". . . (U.S. Bureau of the Census, 1963:117) . . ."]
5. Separate a series of references with semicolons and enclose them within a single pair of parentheses. [". . . (Burgess, 1968; Marwell et al., 1971; Cohen, 1962) . . ."]

B. *In the appendix:* List all source citations by author, and within author by year of publication, in an appendix titled "References." The reference appendix must be complete and include all references in the text. The use of "et al." is not acceptable in the appendix; list the names of all authors. (See A. 4. for text format.)
If there is more than one reference to the same author and year, distinguish them by the letters, a, b, etc. added to the year. [". . . Levy (1965a:331) . . ."] Give the publisher's name in as brief a form as is fully intelligible. For example, John A. Wiley and Sons should be "Wiley." If the cited material is unpublished, use "forthcoming" with name of journal or publisher; otherwise use "unpublished."
Use no underlining, italics or abbreviations.
Examples follow:

1. *Books:* Jud, Gerald J., Edgar W. Mills, Jr. and Genevieve Walters Burch
   1970 Ex-Pastors. Philadelphia: Pilgrim Press.
   U.S. Bureau of the Census
   1960 Characteristics of Population. Volume 1. Washington, D.C.: U.S. Government Printing Office.
   Bernard, Claude
   [1865] 1957 An Introduction to the Study of Experimental Medicine. Tr. Henry Copley Greene, New York: Dover.
2. *Periodicals:* Conger, Rand
   Forthcoming "The effects of positive feedback on direction and amount of verbalization in a social setting." Pacific Sociological Review.
   Merton, Robert K.
   1963a "The ambivalence of scientists." Bulletin of The Johns Hopkins Hospital 112:77–97.
   1963b "Resistance to the systematic study of multiple discoveries in science." European Journal of Sociology 4:237–82.
3. *Collections:* Davie, M.
   1938 "The pattern of urban growth." Pp. 133–61 in G. Murdock (ed.), Studies in the Science of Society. New Haven: Yale University Press.

See recent issues for further examples. **Revised 1980 ASR**

AMERICAN SOCIOLOGICAL REVIEW

Official Journal of the American Sociological Association

DECEMBER, 1980 VOLUME 45, NUMBER 6

ARTICLES

The Presidential Address: The Challenge and Opportunities of Applied Social Research .... Peter H. Rossi 889
Social Process and Hierarchy Formation in Small Groups: A Comparative Perspective .... Ivan D. Chase 905
Weber's Last Theory of Capitalism: A Systematization .... Randall Collins 925
Immigration and the Expansion of Schooling in the United States, 1890–1970 .... John H. Ralph and Richard Rubinson 943
The Meek Shall Not Inherit the Earth: Self-Evaluation and the Legitimacy of Stratification .... L. Richard Della Fave 955

RESEARCH NOTES

Suburban Status Instability .... Harvey M. Choldin, Claudine Hanson, and Robert Bohrer 972
Generating Newsworthiness: The Interpretive Construction of Public Events .... Marilyn Lester 984

COMMENTS AND REPLIES

The Relay Assembly Test Room: An Alternative Statistical Interpretation (Comment on Franke and Kaul, ASR October 1978) .... Robert Schlaifer 995
Worker Productivity at Hawthorne (Reply to Schlaifer) .... Richard Herbert Franke 1006
Welsh Political Mobilization: A Comment and a Note (Comment on Ragin, ASR August 1979) .... James M. Lutz 1028
Aggregation Gain and Loss in Electoral Research (Reply to Lutz) .... Charles C. Ragin 1031

OTHER

The Editor's Items 1037
1980 Editorial Consultants 1039
1980 Index, Volume 45, Numbers 1–6 1042

Editor: RITA J. SIMON

NEW EDITOR

Effective with the February 1981 issue, William H. Form will become the editor of the American Sociological Review. Beginning July 1, 1980, send all manuscripts to Prof. William H. Form, Department of Sociology, 326 Lincoln Hall, University of Illinois, Urbana, IL 61801.

Deputy Editors: CLARK McPHAIL AND JOE L. SPAETH

Associate Editors: PAUL BURSTEIN, ROBERT EVAN COLE, THOMAS J. FARARO, BERNARD FARBER, DANIEL GLASER, MICHAEL J. HINDELANG, GISELA HINKLE, ROSABETH KANTER, JAMES R. KLUEGEL, JOHN LOFLAND, MURRAY MELBIN, MICHAEL MOCH, ANTHONY OBERSCHALL, VALERIE K. OPPENHEIMER, JEFFREY PFEFFER, ALEJANDRO PORTES, SAMUEL PRESTON, BARBARA RESKIN, RICHARD RUBINSON, CHARLES W. TUCKER, NANCY BRANDON TUMA, JOHN WILSON, MAYER N. ZALD, LYNNE G. ZUCKER.

Copy Editor: ELIZABETH ROBINSON NEUMANN

Managing Editor: SHARON GUNASON

Executive Officer: RUSSELL R. DYNES

SCOPE AND MISSION OF AMERICAN SOCIOLOGICAL REVIEW: The *American Sociological Review* publishes work of interest to the discipline in general, new theoretical developments, results of research that advance our understanding of the most fundamental social processes, and important methodological innovations. Like other publications of the Association, emphasis is given to exceptional quality. Unlike the more specialized journals of the Association, the *American Sociological Review's* primary focus is on whatever has the most general bearing on the knowledge of society.

Concerning manuscripts, address: Editor, *American Sociological Review*, Department of Sociology, University of Illinois, Urbana, Illinois 61801. (217) 333-6515.

The *American Sociological Review* (ISSN 0003-1224) is published at 49 Sheridan Avenue Albany, New York, bimonthly in February, April, June, August, October, and December.

Applications for permission to quote from this journal should be addressed to the Executive Office

Copyright © 1980 American Sociological Association.

Annual membership dues of the American Sociological Association, $20–$120 ($10 of this amount may cover a subscription to the *American Sociological Review*); Associate, $25; International Associate, $25; Student Associate, $12.

Application for membership and payment of dues should be made to the Executive Office. Subscription rate for members, $10; nonmembers, $15; institutions and libraries, $30; nonmember students, $10. Single issue, $4.

New subscriptions and renewals will be entered on a calendar year basis only.

Change of address: Six weeks advance notice to the Executive Office and old address as well as new are necessary for change of subscriber's address.

Concerning advertising, changes of address, and subscriptions, address: Executive Office, American Sociological Association, 1722 N Street, N.W., Washington, D.C. 20036.

Claims for undelivered copies must be made within the month following the regular month of publication. The publishers will supply the missing copies when losses have been sustained in transit and when the reserve stock will permit.

Second class postage paid at Washington, D.C. and additional mailing offices.

MEMBERS OF THE COUNCIL, 1981

Officers of the Association

WILLIAM FOOTE WHYTE, President
Cornell University
RENÉE C. FOX, Vice President
University of Pennsylvania
ERVING GOFFMAN, President-Elect
University of Pennsylvania
JOAN HUBER, Vice President-Elect
University of Illinois–Urbana-Champaign
HERBERT L. COSTNER, Secretary
University of Washington
PETER H. ROSSI, Past President
University of Massachusetts, Amherst
RUSSELL R. DYNES, Executive Officer
American Sociological Association

Elected-at-Large

PAULINE B. BART
University of Illinois, Chicago Circle
NORMAN BIRNBAUM
Amherst College
ERNEST Q. CAMPBELL
Vanderbilt University
ARLENE K. DANIELS
Northwestern University
IRWIN DEUTSCHER
University of Akron
WILLIAM GAMSON
University of Michigan
HELENA Z. LOPATA
Loyola University of Chicago
THOMAS F. PETTIGREW
Harvard University
MORRIS ROSENBERG
University of Maryland
IMMANUEL WALLERSTEIN
SUNY, Binghamton
CHARLES V. WILLIE
Harvard University
MAURICE ZEITLIN
University of Cal., Los Angeles

(Articles in the REVIEW are indexed in the Social Sciences Index, Psychological Abstracts, Sociological Abstracts, Ayer's Guide, University Microfilms, Abstracts for Social Workers, International Political Science Abstracts, and United States Political Science Documents.)

AMERICAN SOCIOLOGICAL REVIEW

THE PRESIDENTIAL ADDRESS: THE CHALLENGE AND OPPORTUNITIES OF APPLIED SOCIAL RESEARCH*

PETER H. ROSSI
Social and Demographic Research Institute
University of Massachusetts–Amherst

American Sociological Review 1980, Vol. 45 (December):889–904

Introduction

The stance of our profession toward applied work of all sorts in sociology has been one of considerable ambivalence. On the one hand, applied work is decidedly below basic or theoretical work in the hierarchy of preference, prestige, and esteem. Correspondingly, academic employment is very strongly preferred over nonacademic positions, even though salary levels for the latter are often considerably higher. When we scan the credentials of job applicants or bibliographies appended to grant applications, articles published in journals that feature applied work are assumed to be of lesser quality than ones published in mainstream professional journals. Most sociologists are embarrassed when we are confused with social work or with family therapists. Indeed, some of our colleagues gloss over their applied work as if it were a vice best kept from public view.

On the other hand; our roots in applied concerns are old and very much alive. Our ranks always have been full of ministers and ex-ministers, radicals and ex-radicals, even a few conservatives and ex-conservatives, all of whom were attracted to sociology because our discipline appeared to have some relevance to social reform or its prevention. And when it comes to facing outward toward the public—and especially toward our benefactors—we are very quick to point to the many complicated social problems that justify our existence and need for support. Our public stance is often enough that a properly supported sociology will point the way to a better society with a lowered level of social troubles.

Thus, our ambivalence consists in believing—at one and the same time—that applied work is not worthy of our best efforts and our best minds and that an important reason for our existence is that sociology will lead to important practical applications.

Some of our critics apparently understand this ambivalence sufficiently to be able to exploit it and, occasionally, will criticize us on both counts. For example, earlier this year, Congressman Ashbrook (Ohio) spoke out in support of his proposal to slash the NSF appropriation for the basic research program in the social and behavioral sciences—a sterile title that hides sociology, political science, and economics in a plain brown envelope. He accused us (on the one hand) of being off in the cloudy heights of theory and speculation and, hence, not attending to the very real needs of our society, yet (on the other hand) of seriously endangering the moral order of our society by implicitly endorsing, through research on such matters, the dreadful patterns of deviance that afflict our society.

It should be pointed out that sociology

---

* Direct all correspondence to: Peter H. Rossi; Social and Demographic Research Institute; University of Massachusetts; Amherst, MA 01003.

Presented as the Presidential Address to the American Sociological Association at its annual meeting in New York City, August, 1980.

Grateful acknowledgements are due to James F. Short, Alice S. Rossi, Stanley Lieberson, Clark C. Abt, James D. Wright, Howard Freeman, and Richard Berk whose comments on an early draft helped to improve this paper.

is not the only discipline that holds an ambivalent view of applied work: this is a viewpoint we share with the other social sciences and with the natural physical sciences as well.

There must be some important reasons why this ambivalence persists, over time, in sociology and other disciplines. I will leave it to the sociologists of science to provide an understanding of why we have such mixed feelings toward applied work. My task here is to examine more closely the relationships between applied and basic work in our field, through taking up four themes:

First, I will explore what the differences are between basic and applied work, showing that the lines between the two are difficult, if not impossible, to draw.

Second, I will attempt to show that there has been considerable commerce between the two parts of our discipline, to the considerable enrichment of both. For polemical reasons, I will emphasize the contributions that applied research has made to basic sociology, an argument that may not give appropriate attention to the fact that the commerce between basic and applied work is certainly a two-way exchange. No one knows whether, in the long- or short-run, the balance of trade favors the one or the other side of the exchange—although there are many opinions on this issue.

Third, I will belabor the obvious a bit, pointing out what many of us already know quite well—namely, that applied social research has enjoyed a boom period over the last two decades, a prosperity in which sociology as a discipline has not participated strongly.

As my final theme, I will point out some of the implications of the current boom in applied social science for our field, indicating why we should become more involved and how we can do so.

There are several varieties of applied work in sociology, including clinical practice, policy analysis, consultation to businesses and government agencies on substantive issues, and applied social research. My main concern will be with applied social research, although many of the points made are, most likely, relevant to other kinds of applied work in sociology. I confine myself to applied social research mainly because I know more about that subfield of applied sociology.

The general argument running through this article is that applied social research has been a firm and fruitful part of the tradition of our discipline. Practitioners and their applied social research have not been given sufficient credit for their contributions to the development of our discipline. I argue for greater recognition to be given to applied social research—in the form of more attention to that activity within our curricula, in our professional journals, and in the rewards our profession gives for scholarly accomplishments.

A Modest Contribution toward Conceptual Clarity

It is easy to distinguish between applied and basic work in sociology if you consider only extreme cases. Using examples from my own work, it appears to be quite obvious that *After the Clean-Up . . .* (Wright et al., 1979) is applied social research, being an attempt to estimate the long-term effects of natural disasters (tornadoes, floods and hurricanes) on the population and housing stocks of small areas. It is, clearly, applied research because its main purpose was to assess whether there was any need for changes in federal policy to provide aid to communities and neighborhoods ameliorating the impacts of long-term negative effects of such natural-hazard happenings. In contrast, an apparently clear example of basic research is my recent article (Alves and Rossi, 1978) on uncovering the normative bases for distributive justice judgments concerning household earnings. What is it that appears to differentiate between the two examples? First of all, the research on the long-term effects of natural disasters was designed to serve an existing need for information for social policy decision making while the distributive justice study is far from current policy concerns. In contrast, income redistribution issues involving wage rates are not explicit issues on current political agendas of legislatures. Nor is there any consideration given to regulating earnings in the interest of increasing popular feelings about how just income discrepancies are among households. The fact that we found no discernible long-term community effects, stemming out of a decade's experience with natural disasters, meant that no relief measures designed to meet the needs of small areas were necessary. In contrast, there likely are no policy makers who are currently concerned with altering wages in order to increase the average sense of justice.

Note, however, that implied in the foregoing is the argument that for a given research to be clearly applied requires the existence of policy makers who would be interested in the outcome as well as in the possibility or existence of some policy to which the research was immediately addressed. So far, this does not appear to be a very satisfactory definition as its main provisions are not linked to the research activities per se but, rather, to the purposes to which the research might be put by some segment of our society. Indeed, this consideration points to the historical specificity of just what is often called applied social research. Because the interests and concerns of policy makers and other political partisans change, over time, research that is applied in one historical period may be basic research in another period—and vice versa. Legislatures might not be concerned *currently* with establishing new (and presumably more equitable) principles for setting wages and salaries, but it is certainly conceivable that they *might* do so. In that event, Alves and Rossi's (1979) study would become applied social research! (We will return again to this issue of historical specificity.)

A second point of difference between the two research examples lies, in the first instance, in the existence of a "client" who underwrote the costs of the research and who also had a direct interest in the outcomes of the research. The NSF-RANN program financed the research that went into the *After the Clean-Up* study because it showed promise (indeed, we actually *did* promise) to be relevant to policy issues. But the social science panel of NIMH that approved the distributive justice study apparently "couldn't have cared less" whether the research was needed for social policy or not. Nor did that grant application promise policy-relevant findings.

Having a client who is concerned about the outcome of a research project that he or she has sponsored often means a continuing connection between the researcher and client throughout the conduct of the research project. While the connection may take a variety of forms, all the way from "oppressive" supervision at the one extreme and benign neglect at the other extreme, applied research always means that somewhere there is less autonomy for the applied (as compared with the basic) researcher.

In applied work, there is more time-consciousness, deadlines to be met, interim and final reports to clients to be written; and, often, there is some specific person or group that is intensely enough interested in your work to attempt constantly to find out what you are up to, and doing, in quite specific terms. In contrast, even though basic researchers have to report to someone and be responsible in a general way for doing a job, the detail and constancy of supervision is simply not as great. I am sure that this is one of the reasons that applied research is valued less than basic research in which the autonomy of the researcher is always greater. Although the NSF-RANN program was far from the most intrusive clients I have had dealings with, my colleagues and I did have to suffer, albeit mildly, through a succession of advisory committee meetings, conferences with RANN staff members, and consultations with federal agencies that were concerned with disaster relief.

Basic research presents quite a contrast. I never again heard from NIMH, once the distributive justice grant was awarded. As requested, I sent copies of articles published as a result of work done under the support grant. If anyone on the NIMH staff actually read any of them, he/she certainly did not feel impelled to communicate either pleasure or displeasure or, for that matter, any other reaction. From almost any researcher's point of view, it is better to operate either without a client or with a very unintrusive client. Certainly, in that sense, basic research is to be preferred over applied research.

The major implication that flows from this discussion, so far, is that whether or not a given research or scholarly work is regarded as basic or applied is, at least partially, historically bound. Thus, subsequent history may treat very differently research conducted in an earlier period. For example, the now-classic 1947 NORC (Reiss, 1961) study of occupational prestige was originally applied social research conducted for a federal agency concerned at the time with the recruitment of natural scientists to federal employment. The agency was convinced that the prestige of federal employment had suffered a severe blow in the years immediately following World War II and had sponsored the research simply to get a better reading on how government scientists were viewed by the general public—as compared with their views of scientists who worked for private industry or for universities. What was, originally, a piece of applied work done for a particular client has become, with time, primarily basic research—at least in the way it has been viewed in the sociological literature.

Indeed, the NORC-North-Hatt occupational prestige research is rather instructive as an illustration of the interpenetration of applied and basic interests. It certainly would have been possible for NORC to have undertaken a study of occupational prestige that was focused exclusively on the occupations of interest to the client. However, the researchers in charge of the project convinced the client that the agency's interests would be better served by a research design that placed the occupations of policy interest into the context of the entire occupational structure. This enlargement of the original narrowly focused interest of the client into a much larger framework was guided largely by North and Hatt's interests in social stratification. The resulting study, of course, was considerably enhanced in value for basic sociology as a consequence.

Another example of applied research becoming basic is W. Lloyd Warner's study of Newburyport (Warner and Lunt, 1941). The immediate motivation for the Yankee City studies was to add a community context to the study of industrial work forces, an interest that had grown out of the Mayo and Roethlisberger Western Electric Hawthorne studies (Roethlisberger and Dickson, 1939). One can dispute whether or not the Yankee City studies were even at all relevant to the applied concerns they were designed to serve, but the fact remains that, initially, Warner was working for a client with quite specific interests and the Yankee City studies were initially conceived of as applied research.

The course of history may also turn a basic study into an applied one. Thus, Benjamin Bloom's work (Bloom, 1964) on the stability of IQ during early childhood became one of the intellectual pillars of President Johnson's Head Start program, as policy makers and their staffs searched through existing literature to find data on where best in the lives of children to intervene with compensatory educational programs. Bloom's work noted the apparent stability of IQ from the first grade on, suggesting that preschool intervention would be most effective. In the same way, E. Franklin Frazier's monograph (Frazier, 1939) on the Negro family had almost slipped into scholarly oblivion when revived by Moynihan (Moynihan, 1965) to provide an apparent explanation of why there was so much "pathology" among contemporary black families.

I started out this discussion of the differences between basic and applied social research with examples in which the distinction is easy to make. For many researches, particularly large-scale studies, the distinctions are much more difficult to draw and, at the margins, the lines between applied and basic research are almost impossible to discern. For example, the Income Dynamics Study (Morgan et al., 1974) was sponsored initially by the Office of Economic Opportunity to provide information on how often, and why, families apparently drifted back and forth across the poverty line. Currently, this longitudinal study is being supported as basic research by the Social Science Division of the National Science Foundation. There can be little doubt that this study has simultaneously contributed to applied concerns and basic social science, aided considerably by the wide diffusion of public use data sets. Essentially nothing about the design changed as sponsorship shifted from OEO to NSF, nor was there much shift in subject matter. The question of whether this is basic or applied social research is somewhat irrelevant—as it seems to have been both. Of course, it should be emphasized that the principal investigators played an important role in designing the study (and its continuation) so that findings would be relevant both to current social welfare policy and to a host of basic concerns in several social sciences.

Another interesting example is the Equality of Educational Opportunity report (Coleman et al., 1966) and the data sets lying behind that report. In its inception, Coleman and Associates' study was certainly applied, mandated as it was in the 1964 equal rights legislation. While there were some policy concerns expressed specifically in the federal legislation it was relatively general in character, calling as it did for a survey to ascertain whether or not educational opportunities were equal across ethnic groups. The design created—and the analysis followed—by Coleman and his associates went considerably beyond the mandate of the Congress to provide, among other things, estimates of the effects of schools on the achievement of pupils. Is this an example of basic or applied research? The answer is that it is both: it is applied research—by virtue of having a client and a set of policy concerns to which the research was at least partially directed; but it is also basic research—in that the researchers went beyond a narrow research mandate to consider some general issues in the sociology of education.

Special note should be taken of the important role played by the researchers in the design of applied social research. A particular applied-research study may be so narrowly designed that basic sociological (or other social science) concerns would find very little of interest in the study or in its findings, or it can be designed to accommodate basic concerns as well. Indeed, given the fact that policy concerns are often vaguely stated and, furthermore, often shift over short periods of time, broad-focused applied studies are likely to be more useful to policy interests, as well as being of greater disciplinary interest.

I hope that I have given sufficient examples to illustrate that the line between basic and applied research is a fuzzy one, subject to redefinition by the researchers, by clients, and by the drift of historical change. The implication of such fuzzy boundaries is that it is difficult to decide what is applied work and what is not. Good applied work tends to be transformed into basic contributions, a process that does not disturb the conventional view of applied social research being, somehow, lower in value and quality than is basic research. A proper recognition of the origins of much of the work that we value in our history would support a more positive view of the contribution of applied work to the growth of our discipline.

Of course, the counter-arguments can be made that (1) I have selectively skimmed the best of applied social research in order to bolster unfairly the argument I have presented, and (2) the bulk of applied social research is of poor quality and hardly likely to contribute even to the discipline, let alone to the solution of social problems. There is some truth in this argument: Much of applied social research is best left in the fugitive Xerox reports in which they were issued. But low quality and irrelevance are characteristic of most of the work in our discipline, whether applied or basic. For example, more than three-fourths of all articles submitted to *ASR* are turned down. Whether or not applied work has more low-quality output than does basic work is problematic.

Some General Contributions of Applied Social Research to Basic Concerns

Because the line between basic and applied work in sociology is fuzzy and indeterminate, it is correspondingly difficult to make a definitive assessment of the interchange between basic and applied work. Today's applied work may be tomorrow's basic science—and vice versa.

Furthermore, what I may classify as "basic" may be defined by another sociologist as "applied." Indeed, about the best one can do is to provide examples of notable transactions in which applied work has made obviously significant contributions to basic social science concerns, in the optimistic hope that these outstanding examples are fair indicators of what is true generally.

To begin with, it is important to recognize that many of the most prominent sociologists have devoted some significant portions of their careers to applied social research: Even an incomplete listing of such persons is quite impressive, including Durkheim, Giddings, Ogburn, Stouffer, Park, Hughes, Lazarsfeld, K. Davis, Phillip Hauser, Sewell, O. D. Duncan, and Coleman. Indeed, a rough and informal count I made of the last thirty Presidents of the American Sociological Association yielded an estimate that eighteen had been involved significantly in applied social research. Clearly, this is an underestimate, relying as it does on the fallibility of one observer's memory. Among the remaining twelve, there are a few who did some applied work—as, for example, Pitirim Sorokin, though his general work was certainly not of that vein. Indeed, there are likely more than a few among the twelve others who may have been "closet" applied social researchers.

Most interesting and revealing, in light of the earlier discussion about the lower esteem accorded to applied work, is that so many of the eighteen Presidents are not generally remembered as applied social researchers because, over time, some of their most important applied research has been redefined as basic work. For example, how many of us recall (or ever knew) that Lazarsfeld's seminal work on personal influence (Katz and Lazarsfeld, 1955) stemmed from very applied work financed by MacFadden Publications in an effort to obtain evidence that would convince would-be advertisers that placing ads in *True Story* magazine would reach opinion leaders? Or that the very influential series of researches by Sewell and his associates (Sewell et al., 1976) on status attainment had its beginnings in a state-sponsored survey of Wisconsin high school seniors, the major purpose of which was to forecast the demand for higher education in that state?

One of the more visible characteristics of prominent sociologists who have participated in applied work is the predominance of quantitative empirical researchers in that group. This is a feature of some importance, as I will develop more fully, later in this discussion. For the present, this characterization is noteworthy as evidence that one of the most important contributions of applied research has been to technical developments in research methods.

Most of us are familiar with the fact that much of the basic work in statistical methods has come out of applied concerns in other fields, experimental work in agriculture, psychological test construction, quality control in industry, and so on. The outstanding examples are quite numerous, as the following list demonstrates: Student's T, analysis of variance, factor analysis, regression, and on and on. By and large, sociologists have been net borrowers of research methods vis-à-vis other fields, especially with respect to statistical models.

Sociologists have played a more important role in the development of data collection methods. Along with other social scientists, we have made important contributions to the science and art of sample surveys. Much of the early work that went into the development of sampling methods, interview construction, and the like was undertaken within applied contexts, by sociologists in collaboration with other social scientists. Area probability sampling was developed at the Bureau of the Census out of a need to conduct valid periodic estimates of the labor force. Psychologists and sociologists—working for the most part with the advertising industry, newspapers, and political candidates—developed the attitude survey. Scaling methods, at least in part, developed out of the work in Stouffer's (Stouffer et al., 1950) Research Branch in the Information and Education Division of the War Department. Indeed, the Research Branch, a decidedly applied activity, did a great deal to train a fairly large set of young sociologists who became the postwar specialists in sample surveys and, more generally, helped to establish the use of sample surveys of all sorts as a prime research tool for our field. The most recent development in sample surveys, random digit dialing, was developed initially by persons working on commercial sample surveys; it is now gradually finding its way into use by sociological researchers.

In addition, one must recall that Hollerith was a Census employee when he came up with the idea of the punch card and mechanical tabulating equipment. And the first commercial version of the electronic computer, UNIVAC I, was at least partially speeded toward development by the demand for its use in the 1950 Census of Population and Housing.

Other frequently used techniques in social research were also either developed in pursuit of applied interests or heavily influenced by being used in applied research. For example, the intense postwar public policy interest in the issue of overpopulation gave a large impetus to the further development of demographic methods. Sociometric techniques had their start as Moreno (1934) developed a device for optimizing the residential arrangements of young women in a training school. Social field experiments had an early start with Dodd's (1934) randomized experiment in Syria on techniques for instilling appropriate drinking water treatment procedures in rural areas.

Qualitative research methods also have roots in applied work. The earliest American community study (Williams, 1906), conducted by a rural sociologist housed in an experimental station, was concerned with the impact of changing agricultural technology on a rural town. As mentioned earlier, Warner's study of Newburyport grew out of the Mayo and Roethlisberger work on worker productivity. The Lynds' first Middletown study (Lynd and Lynd, 1938) was financed by a foundation with a concern for studying the impact of social change on the moral life of Americans.

The technological and methodological contributions of applied to basic sociology loom large because new methods are easier to transfer across substantive fields. In contrast, theory and empirical knowledge are more closely tied to substantive fields and, hence, do not travel as easily. Perhaps most of the transfer from applied to basic work in substantive theory and empirical findings has occurred because much applied work has dealt with subject matters that are at the very heart of sociological concerns—stratification and inequality, organizations, collective behavior, deviance and social control, race relations and discrimination, life chances and health care, family, work and occupations, and so on. Indeed, there are few substantive areas that have not been studied with an applied focus.

Yet, it is difficult to point out clear one-way contributions of applied work to basic work—mainly because the interchange has been an interactive one. Some contributions of applied work (such as the concept of personal influence and opinion leadership) are directly and easily traceable to the applied work from which they originated. Others (such as the concept of relative deprivation) are indirect contributions arising out of commentaries upon or secondary analyses of applied work. In still other cases, concepts arising in theoretical work have been refined in applied work—and vice versa. Examples of the joint development of concepts include status attainment, occupational prestige, and anomie. Other examples, I am sure, will occur to the reader. Of course, one of the more important contributions of applied social research has been to the refinement of—and sometimes negations of—concepts derived from general sociology. The empirical testing of concepts that are incorporated into applied research has led often enough to their being disconfirmed. For examples: there is little empirical evidence for labelling theory, virtually no evidence for a "culture of poverty," meager support for differential association, and so on. Although it would certainly be an exaggeration to state that sociology would not have advanced at all over the past three decades without the help of applied work, it would be equally foolish to claim that applied work has not made a strong contribution to the progress that sociology has made in that period. Basic and applied work in our discipline are complementary endeavors.

Some Challenges and Pitfalls of Applied Social Research

The products of applied social research have made, as I have attempted to show, strong contributions to the development of our discipline. Although the hope of making such contributions might be sufficient to motivate one to engage in applied social research, there are also some intrinsic satisfactions to be derived from the inherent nature of applied work. There are also some pitfalls of which the would-be applied social researcher must at least be aware. Both the attractions and the dangers of applied social research have their roots in the politicized nature of such research, as I will now attempt to show.

Applied social research often demands greater technical skills than does basic research. Because the results of applied social research may be used in the political process, it is clearly important that it be done well. After all, an article in a major professional journal, or a monograph, has little consequence except upon the career of the writer and, perhaps, except for attracting the attention of the handful of other social scientists who have been doing work on the same topic. In contrast, the product of applied social research might be used in the formation and change of public policy; and an error in applied work might have consequences not only for the social scientists involved but also for institutions, agencies, policy makers, and the intended beneficiaries of the policies in question.

Although the discipline may be concerned with whether research reported upon in an *ASR* or *AJS* article is based on good samples, used good measurement instruments, used a research design of appropriate power and methods of analyses that are robust, such concerns are not as salient as in applied work. This is one of the main reasons why much applied social research is on a so-much-larger scale than discipline-oriented research. Of course, editors and readers do deduct points for defects in any of these respects, but the fact of the matter is that there are so few good ideas and data in sociology that Type I errors are clearly less important than Type II errors in the judging of articles. In contrast, for example, estimating the work disincentive effects of transfer payments calls for good samples, precision and validity in measurement, and virtuosity in analysis—because an error in any of these might be translated into social policy affecting the well-being of poor households across the nation.

Of course, the fact that applied social research may be used in policy formation is also one of the pitfalls into which it is easy to stumble. It is very easy to become the center of rancorous controversy; contending parties in some policy dispute may use and abuse your work. There are few major applied researches that have not sparked controversy, as, for example, in the cases of the Coleman Report, the evaluation of Head Start, or the Income Maintenance experiments. Indeed, the controversial character of applied social research has spawned part-time employment of researchers as methodological critics hired by partisans to provide devastating criticisms of some applied social research.

Conflict over the results of applied social research is not without its positive side, however. The competition among divergent points of view has led to a considerable acceleration of progress toward higher and higher quality in applied social research. For example, the heavy criticism directed at quasi-experimental estimates of program effectiveness has sparked considerable progress toward understanding the role of a priori theory in devising research designs (Heckman, 1980).

New methodologies have been invented, old ones adapted to new problems; and interdisciplinary transfer of ideas has been accelerated through the critical evaluation of applied research. Of course, so-called basic and theoretical work is also subject to criticism, but the critical process for applied work is more timely and more intense, characteristics which may be somewhat more painful to the applied social researcher but which are more productive of relatively rapid progress in technical and conceptual quality.

The political character of applied social research is the source of another of its attractions, at least to those of us who are still concerned with improving our society. There is the possibility that the results of applied social research will do some good. Thus, one might be tempted to undertake research on the child-care arrangements used by working mothers in the hope that the resulting findings might pave the way for better and more effective child-care policies. There are, however, some restrictions on what one can do as an applied social researcher. First of all, problems are not set by the researcher alone but, often, are worked out on the initiative of some policy-oriented agency and, sometimes, by negotiation between the researcher and the agency in question. In practice, this means that, as an applied social scientist, one is not entirely free to frame research problems in the form that appears most fruitful to the researcher. One is ordinarily restricted to what is called "policy space"—that is, the range of alternative remedies for a social policy that appears to be politically acceptable. Thus, for example, the income maintenance experiments defined a set of payment plans that were thought to span what would be acceptable to Congress, a space that did not include what you and I might consider to be very generous payment plans. In short, there are politically imposed limits on subject matter and policy issues that can be considered in applied social research.

Applied social research tends to be conservative, devoted mainly to the examination of policy alternatives that are not radically different from existing social policies. Fine tuning, rather than revolution, is on the political agenda. At best, applied social research is politically congenial, both to those who are liberals and to the right of liberals.

While applied social research will not bring about revolutionary changes, it is in practice neither reactionary nor completely supportive of the status quo. At least in the present historical period, applied social research has had the characteristics of being a demystifying instrument, exposing the faults and inadequacies of existing institutions. Criminologists in their research have had profound impacts on American prisons. Applied research on schools has raised fundamental questions about what schools do. Applied research on alcoholism has certainly changed our understanding of that disorder. Such examples can be easily multiplied. The fact of the matter is that the social policy of the past few decades was conceived by persons with—at best—amateur social scientist status and, hence, most of it (as well as most long-standing institutions) cannot stand up very well under the scrutiny of a thoroughgoing empirical testing.

It should be kept in mind, however, that applied social research is no occupation for would-be philosopher kings. The applied researcher ordinarily does not get very close to the seats of decision making and policy formation. Often enough, policy appears impermeable to both the results of research and the advice of the researcher. Even at its best, applied social research does not substitute for the political process. It merely provides another input into the policy-making process. Indeed, who would have it otherwise? One of the virtues of our political system is that decisions are made often enough as the outcome of the pulling and hauling among a variety of interest groups, a process that may value the input of social science work but does not place the work on a pedestal of absolute authority.

It is not at all clear to me how one may measure whether the attractions of applied social research are outweighed by the negative aspects of politicized work, or vice versa. If you are attracted both to technically challenging work involving the fine tuning of our existing society and to the opportunities presented by relatively high levels of resources, and are not put off by the possibility that your work will be seemingly ignored by decision makers and possibly come under attack from fellow social scientists, then applied social research is an attractive activity. If you cannot stand verbal and written abuse, or want to work on fundamental changes in our society, then applied social research is clearly not for you.

These foregoing remarks are addressed to the question of whether one would or would not find the politicized aspects of applied social research a deterrent to participation. The negative consequences account, in part, for the reasons that applied social research is undervalued in our discipline. But such arguments do not address themselves to the scholarly contributions such work may make to the growth of our discipline. Applied social research is harder on the participant, but that is at least partially balanced by the greater intellectual challenges such work may provide and by the positive contributions it may make to our society and to the discipline.

The TARP Experiments: Concrete Illustration

The technically challenging nature of applied social research (as well as some of its politicized aspects) is perhaps best conveyed through a concrete illustration. I will draw upon work recently completed by my colleagues and myself (Rossi, Berk, and Lenihan, 1980; Berk, Lenihan, and Rossi, 1980), mainly because some of the more interesting aspects of such work do not get much attention in the formal publications.

The policy origin of the Transitional Aid Research Project (TARP) experiments was a legislative directive to the Department of Labor to help ex-felons to attain productive employment. The Department had tried a variety of programs including skills training, special employment placement efforts, and small amounts of financial aid. Some hope resulted from a small-scale experiment (Lenihan, 1978) which appeared to show that modest payments simulating unemployment insurance benefits were effective in lowering rates of arrest for property crimes.

These promising findings were not convincing enough to call for a national program as the pilot experiment was small, it was conducted in only one jurisdiction, and the payments were not administered by an established government agency. An experiment, the Department reasoned, conducted by a dedicated set of social science researchers, was not the same thing as a program that would be run by state or federal civil servants.

The TARP experiments, conducted with persons released in 1976 from the state prisons of Georgia and Texas, were designed to test whether or not a modest extension of unemployment insurance benefits to released prisoners would aid such persons to make the transition to civilian life without reverting to crime. Although aimed at reducing recidivism, the TARP experiments were far from being either revolutionary or even a very radical departure from existing social policy. The TARP intervention did nothing about the prison systems of the two states involved, nor did it address in a very direct way the causes of crime. Furthermore, TARP was scarcely responsive to several important theoretical systems current in criminology, especially the conflict and labelling perspectives. It was concerned with the current policy space of policy makers: an extension of unemployment insurance coverage to persons released from state prisons seemed to Department of Labor officials to be a policy change likely to achieve enough support in Congress to have a good chance to be enacted into policy, were it possible to show that such an extension could help released prisoners and have substantial equity benefits.

Yet the TARP experiments were not raw empiricism. A very respectable, quite traditional theoretical strain in criminology links together poverty and crime and regards criminal activities as attractive alternative occupations to poorly skilled and inexperienced poor youths. Abundant cross-sectional data link together poverty conditions and crime rates, but such correlations are hardly proof of causal relations. While no one with any shred of morality would ever design an experiment that used poverty as a treatment, it is quite acceptable to most moral sensibilities to provide the exactly opposite treatment, to make persons better off economically. The TARP experiments, then, may be regarded as part of the crime and poverty theoretical tradition providing some experimental evidence on a significant theoretical issue.

Technically, the TARP experiments were very challenging. First of all, it was necessary to design a research project that could produce results that would have high internal validity. A randomized controlled experiment was, therefore, decided upon as the study design. Persons released over a six-month period from the Georgia and Texas prisons were randomly assigned either to: (1) one of three treatment groups that were offered eligibility for at least 13 weeks of unemployment insurance benefits at the minimum benefit levels prevalent in the two states—$63 and $70 per week, respectively, in Georgia and Texas; or (2) an experimental group that was offered employment counseling through the state employment security agency; or (3) one of two controlled groups. Approximately 2,000 released prisoners in each of the two states participated in the experiment. About half of the participating prisoners were followed for a year beyond release with periodic personal interviews asking about their post-release employment experiences and tracking other important life events. In addition, all participating ex-prisoners were tracked through the computerized state arrest criminal justice files which recorded arrests and convictions and through unemployment insurance records which contained quarterly reports on earnings that were subject to the taxes on employers that supported the unemployment benefit systems. Following this extraordinarily transient group, over time, was a considerable tour de force of field work: the high response rates of over 85% for each of three follow-up interviews is an accomplishment that is a considerable tribute to the local interviewing crews and the overall orchestration by Kenneth Lenihan.

The resulting data constitute a considerable body of information on the post-release experiences of ex-felons, better and more detailed than any other data set, a considerable mine of information about the lives of members of this segment of the American underclass. These data are now in the public domain and can be used by any social scientist for a variety of purposes.

As often happens, the policy implications of the experiments' findings are a mixture of good and bad news. First of all, when looked at in the traditional analysis of variance perspective, the treatments clearly failed as administered. Experimental groups were neither more nor less likely to be rearrested in the postrelease year. Hence, it is clear that an extension of unemployment benefits within the existing unemployment benefit system will not avert arrests and the crimes that such arrests represent. Additional bad news was that the benefits had rather strong work disincentive effects, with those treatment groups receiving payments working considerably less, a perverse outcome for a treatment that was supposed to aid the ex-prisoners into normal civilian life.

In order to obtain good news from the experimental results, it was necessary to reexamine the theoretical foundations of the experiment and to depart from the traditional analysis of variance perspective. The reexamination of theoretical foundations led us to consider the recently revived economic interpretation of crime and to draw upon sociological thinking on occupations and work. The resulting theoretical model explained both the seeming lack of an effect of payments on arrests and the work disincentive effects of the payments. Briefly, the payment lowered arrests by lowering the incentives to criminal activities but increased arrests by lowering employment. These two effects cancel out each other, leaving no differences between experimentals and controls in rearrest rates but considerable differences in employment experiences. Modelling this process through simultaneous equation procedures produced results that were consonant with the theoretical model and which were similar in both Georgia and Texas. In addition we were able to show that the payments had the positive effect of enhancing wage rates when employed for persons in the experimental groups that were eligible for payments—the financial cushion supplied by payments allowed some of the leisure to find better employment.

The extended results obtained by broadening the theoretical bases of the experiments and departing from the analysis of variance technique has already generated controversy. We believe that the results justify attempting to set up a benefit system—the "severance pay model," as we have called it—that does not have a severe work disincentive built into it as does the unemployment benefit system that provides benefits only when one is unemployed. Our critics point to the analysis of variance results and claim that the more complicated and, hence, more delicate simultaneous equation modelling is speculative at best. My colleagues and I are sure to be under attack on substantive and technical grounds as soon as the monograph comes under review.

The point of mentioning the controversy that exists now (and that will most likely escalate) is not to elicit sympathy for the "pure scientists" under attack. Rather, it is to illustrate that politics and methodology are not separable. The failure of the policy as tested led the liberal-minded investigators to doubt that the analysis of variance results were the only and best description of the outcome of the experiments and to analyze the data further with more sophisticated approaches. More conservative minds, who might believe that criminals are not to be coddled because they are incorrigible, distrust the results of a more sophisticated approach and prefer to rest—content with the more robust analysis of variance.

The interpenetration of politics and methodology has both positive and negative effects. On the negative side, it can mean that policy-related research is rarely regarded as definitive. On the positive side, it certainly sharpens one's sense of responsibility concerning method, enhancing the care with which data are treated and the level of skill employed.

Has the experiment affected social policy? So far, not very much. When I made a presentation of the findings to the Secretary of Labor last year, I found that Secretary Marshall was quite intrigued with the ideas and was inclined to start policy changes; but subsequent federal budget difficulties have placed any such changes on the shelf.

The TARP experiments have all of the characteristics I outlined earlier. First, on the positive side, the problem of the relationship between poverty and crime is theoretically relevant, with the experimental results providing important new data illuminating the relationship. Hence, we hope that it makes a contribution to basic sociology and, especially, to theories of criminality. Second, the research was technically challenging, with sufficient resources made available to employ a powerful (and expensive) research design and to afford extensive data processing and advanced analytical methods. Third, the policy being tested was a "good" one—that is, if enacted it would provide benefits to persons who were in need and deprive no one else of very much. Indeed, because potential savings through averted arrests can be considerably greater than the costs of payments, taxpayer dollars can be saved.

On the negative side, first, one might view the experiment as tinkering with a system that needs major overhauling. The TARP policy was scarcely radical or very innovative. Second, controversy exists and is bound to increase over whether or not the experiments actually showed the results that we believe they showed. Finally, the experiments did not immediately affect social policy. The changes (in the way in which we treat ex-prisoners) that may result from the experiment have been postponed to the future—how far in the future, one may only speculate, given the possible changes in our federal government that this year's election might produce.

What is the balance between positive and negative? Given the slowness of the process by which new research findings are assimilated into the body of sociology, it is clearly too early to tell. Interestingly enough, the negative is experienced immediately, but the positive outcomes will take place, if at all, over the next decade. My colleagues and I have great hopes that, eventually, the score will show a decided positive balance.

Opportunities in Applied Social Research Today

Applied social research may or may not be as attractive as I have described it, but if there are no opportunities for participation in such activities, then that message may be enlightening—but irrelevant—to our discipline. Such is decidedly not the case, however: applied social research must be described as having been, during the last two decades, one of the growth sectors in our economy. Estimates (Abt, 1980) of the amounts spent in the federal budgets of the last few years on applied social research, in one form or another, vary from a high of two billion to a low of about one billion. The amount spent on the evaluation of educational programs alone was estimated to be more than 100 million in 1976. Whatever estimate one accepts, the numbers indicate that applied social research is big, strong, and flourishing—especially when compared to the support of basic social research program funds, which amount to only several hundred million for all the social sciences combined. In addition, private foundations also provide support, although on a smaller scale.

Note that these funds count only the federal budget allocations. State and local governments also support applied social research, perhaps not to the same extent but adding at least several hundred million more.

With all these funds going for applied social research, one might predict that sociology and sociologists would be wallowing in prosperity. Such appears to be far from the current state of our discipline. While we are not on the threshold of destitution and widespread unemployment, it does not appear that sociologists generally have participated fully in the last two decades of growth in applied social research. There are a variety of reasons for our failure to participate.

First of all, applied social research is certainly not the monopoly of sociology. Economics, psychology, political science, education, geography, anthropology, and such hybrid fields as business, communication, social work, and operations research are all fields that can and do participate in applied social research. Indeed, my best estimate is that economists, psychologists, and educational researchers are more deeply involved in applied social research than any other field. Of course, the three leaders in applied social research are bigger fields than sociology and, hence, such leadership reflects in part the fact that there are simply many more psychologists, educators, and economists than sociologists. But it also reflects the fact that, within those fields, applied work is accorded higher regard than in sociology. Sociology has been primarily an academic field, with far fewer sociologists employed outside the groves of academe, compared to the profile of economists and psychologists.

Second, much of the funds available for applied social research does not go to universities but to nonacademic organizations. Within the last decade, new organizations have appeared on the scene to provide the applied social research that was demanded by the existence of research funds. Older, nonacademic research organizations have also responded by increasing their staffs. Research organizations loosely connected with universities, such as NORC and Michigan's ISR, have expanded as well. Some of the larger research firms now dwarf university social science divisions. For example, Abt Associates, Inc., located in Cambridge, employs more social science Ph.D.'s on its staff than any of the universities in the Boston area. The Rand Corporation in Santa Monica has enough social scientists on its staff to offer a Ph.D. program in the social sciences.

The rapid increase in effective demand for applied social research has not been without accompanying problems. First of all, it appears that legislators often imposed impossible research demands—yet, at the same time, offered funds to do those impossible tasks, a situation that is a structured strain toward hypocrisy, and fraud on the part of suppliers. Literally scores of "beltway bandit" firms were started to bid on the offered contracts, often to accomplish tasks that could be done or completed within the allotted time or budget or with existing research technology. Second, the demand for applied social researchers exceeded the available supply, with the result that there were many persons who became social researchers by fiat. A serious negative consequence of the rapid growth in applied social research was that the quality of much of such research was exceedingly poor—especially in the earliest part of the growth period and especially on the state and local level. Fortunately, there is an apparent secular trend toward increased quality accompanied by a high failure rate among the "beltway bandits." The low quality of some of this research, however, has not helped to raise the status of applied social research among the academic disciplines.

Not all of the quality problems of applied social research have been solved. There are still many applied research projects that are ill-conceived, sloppily executed, and presented in a misleading fashion. Yet the best of applied social research is as good as the best social science going. Furthermore, the average quality is rising as quality standards diffuse more widely and as the critical process described earlier takes hold.

It is especially striking that the research-oriented universities and social science departments have not participated very much in the burgeoning market for applied social research. One of the major reasons has just been alluded to: often, the research tasks were impossible to accomplish and inadequately funded. But there are also additional reasons. First, applied social research is not very high on the academic totem pole of prestige. Second, even when the research tasks are attractive and of intrinsic interest, academicians find it difficult to respond quickly to the research procurement process and to mount the often-extensive, large-scale research projects that are called for. Whatever the reasons, the concentration of applied social research activities outside the university walls has meant that sociology as primarily an academic discipline has not been reached by this new prosperity.

Third, applied social research demands skills that not all sociologists have received through training or through practice. By and large, applied social research is quantitative research and I would venture that the level of relevant skills achieved on the average by sociologists is considerably below that of the social science fields that have been more successful in applied work. In addition, applied work requires an attentiveness to policy-related structural features of our society. For example, the TARP experiments are concerned not with altering the etiological conditions of crime in our society but with slight, inexpensive, and politically expedient alterations in our social services system that might avert a very small proportion of all crimes.

Whatever the reasons for our discipline's lack of participation in applied social research, it is clear that we are missing out on some important opportunities. Primary among these missed opportunities are those that relate to the intellectual health of our discipline. Applied social research, as I have tried to argue earlier, here, provides important opportunities to learn more about how society works for building theory and for strengthening our base of empirical knowledge.

There are also organizational reasons for building a stronger level of participation in applied social research. Academic employment for sociologists at present—and even more so in the future—is drying up. We can no longer afford to run undergraduate curricula that lead to no specific vocational opportunities for graduates. Few departments in the country are experiencing any increases in undergraduate enrollment and in majors. We appear to be losing out to fields in which there are clear employment opportunities beyond graduation. I suggest that junior-level applied research opportunity is one such vocational goal that we could emphasize in our teaching and training programs.

Applied social research is even more important for graduate-level training in our discipline. There are jobs out there in the applied social research "industry" that properly trained new sociology Ph.D.'s could enter.

In order to take greater advantage of these occupational opportunities for our undergraduate majors and those who earn graduate degrees, it is necessary for the discipline to build linkages to applied social research as an institutionalized activity. This means that academic sociologists will have to participate more in applied work. We have to learn how to compete successfully for the funds that are available, learn how to undertake the more complicated and extensive research tasks that are involved. This last point may mean that we have to build research organizations within universities or loosely connected with them that can efficiently and skillfully carry out the research tasks involved. Building such organizations will mean that academicians will have the experience to understand properly the research tasks involved and will be able to provide the apprenticeship-like hands-on experiences for undergraduate majors and graduate students that can make academicians attractive as researchers. We will also have to revamp our undergraduate and graduate curricula to reflect a greater emphasis on quantitative research and on policy-oriented research. Note that I am not suggesting that sociology become a monolithic discipline as far as research methods are concerned. All I am advocating is greater emphasis, especially at the undergraduate level.

Of course, all the changes suggested above imply a greater recognition to be given to applied work as crucial to the health of our discipline. That means that we should not lift our eyebrows when our better graduate students take nonacademic research positions, conveying the notion that by so doing they have left the company of the elect. It means that we evaluate applied social research as research and not automatically deduct points because it is applied. It also means that those of us who do both basic and applied work should be as proud of the latter as of the former. Applied social researchers should come out of the closet!

There are other institutional changes that would also be helpful. Our professional association currently has an academic bias that manifests itself in a variety of ways: our employment bulletin lists few nonacademic positions, and the editors of our professional journals are reluctant to consider articles that are clearly applied research. The American Sociological Association should be taking the lead in helping to bridge the distance between our discipline and the intellectual and employment opportunities in applied social research.

Sociology has deep roots in humanity's desire to understand and control the turmoil of social change and in its striving for ways to lessen the toll of humankind. It would be to deny our heritage to become more and more turned inward to an increasingly precious academic discipline. For the sake of our intellectual growth and our disciplinary strength, we need to provide a more respectable place for applied work, a move that will surely redound to our benefit in excitement, theoretical progress, and a renewed sense of the relevance of our work to the world around us.

REFERENCES

Abt, C. C.
1980 "What's wrong with social policy research?" In C. C. Abt (ed.), Problems in American Social Policy Research, Cambridge: Abt Books.

Alves, W., and P. H. Rossi
1978 "Who should get what? Fairness judgments in the distribution of earnings." American Journal of Sociology, 34:541–64.

Berk, R. A., K. Lenihan, and P. H. Rossi
1980 "Crime and poverty: some experimental evidence from ex-offenders." American Sociological Review, 45:766–86.

Bloom, B.
1964 Stability and Change in Human Characteristics. New York: Wiley.

Coleman, J. S., E. Q. Campbell, C. Hobson, J. McPartland, A. Mood, F. Weinfeld, and R. L York
1966 Equality of Educational Opportunity. Washington, D.C.: U.S. Government Printing Office.

Dodd, S. C.
1934 A Controlled Experiment on Rural Hygiene in Syria. Beirut: Publications of the American University of Beirut Social Science Series, No. 7.

Frazier, E. F.
1939 The Negro Family in America. Chicago: University of Chicago Press.

Heckman, J.
1980 "Sample selection bias as a specification error." In E. W. Stormsdorfer and G. Farkas (eds.) Evaluation Studies Annual Review. Beverly Hills: Sage.

Katz, E. and P. F. Lazarsfeld
1955 Personal Influence. Glencoe: Free Press.

Lenihan, K.
1978 Opening the Second Gate. Washington, D.C.: U.S. Government Printing Office.

Lynd, R. S., and H. M. Lynd
1928 Middletown. New York: Harcourt Brace.

Moreno, J. L.
1934 Who Shall Survive? Washington: Nervous and Mental Disease Publishing Co.

Morgan, J. N., K. Dickinson, J. Dickinson, J. Bemus, and G. Duncan
1974 Five Thousand American Families: Patterns of Economic Progress, (Vol. 1.) Ann Arbor: Institute for Social Research.

Moynihan, D. P.
1965 The Negro Family: The Case for National Action. Washington, D.C.: U.S. Government Printing Office.

Reiss, A. E.
1961 Occupations and Social Status. Glencoe: Free Press.

Roethlisberger, F., and W. J. Dickson
1939 Management and the Worker. Cambridge: Harvard University Press.

Rossi, P. H., R. A. Berk, and K. Lenihan
1980 Money, Work and Crime. New York: Academic Press.

Sewell, W. H., R. M. Hauser, and D. Featherman
1976 Schooling and Achievement in American Society. New York: Academic Press.

Stouffer, S. A., L. Guttman, E. Suchman, P. Lazarsfeld, S. Star, and J. Clausen
1950 Measurement and Prediction: Studies in Social Psychology in World War II, Vol. 4. Princeton: Princeton University Press.

Warner, W. L., and P. S. Lunt
1941 The Social Life of a Modern Community. New Haven: Yale University Press.

Williams, J. M.
1906 An American Town. New York: Lippincott.

Wright, J. D., P. H. Rossi, S. R. Wright, and E. Weber-Burdin
1979 After the Clean-Up: Long Range Effects of Natural Disasters. Beverly Hills: Sage.

MANUSCRIPTS FOR THE *ASA ROSE SOCIOLOGY SERIES*

Manuscripts (100 to 300 typed pages) are solicited for publication in the *ASA Arnold and Caroline Rose Monograph Series*. The Series welcomes a variety of types of sociological work—qualitative or quantitative empirical studies, and theoretical or methodological treatises. An author should submit three copies of a manuscript for consideration to the Series Editor, Professor Suzanne Keller, Department of Sociology, Princeton University, Princeton, NJ 08540.

SOCIAL PROCESS AND HIERARCHY FORMATION IN SMALL GROUPS: A COMPARATIVE PERSPECTIVE*

IVAN D. CHASE
State University of New York at Stony Brook

American Sociological Review 1980, Vol. 45 (December):905–924

The author investigates the theory, methods, and findings of animal behaviorists and social scientists studying dominance hierarchies in small groups. In both disciplines: (1) the literature argues that the explanations of hierarchy structure are based upon differences in individual characteristics among group members; (2) although critical examination reveals that these explanations require stringent conditions to account for commonly occurring kinds of hierarchies in humans and animals, the available data indicate such conditions are not met; and (3) the hierarchy-formation process has not been adequately studied. In an attempt to alleviate the current problems in hierarchy research, I present the results of a particular animal study and develop a general explanation as to how hierarchy structures arise. This approach applies to both humans and animals and serves as a model of how the cumulative patterns of interactions among individuals produce group social structures.

Despite great differences in social behavior in many human and animal groups, their dominance hierarchies are remarkably similar (see Brown, 1975; and Wilson, 1975, for general background on dominance hierarchies). Why should species so diverse as animals are from one another, and as all the animals are from humans, form dominance hierarchies so similar in structure? And, even more basically, how are we to study and explain the formation of dominance structures in small groups of men and animals? The goal, here, is to make a start toward answering these questions by developing a new approach to the study of hierarchy structures. This approach explains how hierarchies emerge from the interaction among group members rather than being generated by differences among those individuals. It treats hierarchy formation as a developmental process in which the outcomes of previous interactions influence the course of successive ones, and it indicates how patterns of interaction fit together to form the kinds of dominance hierarchies commonly observed.

This paper is in five parts: (1) a summary of the data collected on dominance hierarchies and the methods used to gather those data in both the animal behavior and the human social sciences literature; (2) a critical evaluation of the theories that have been proposed to explain hierarchy structures in both disciplines; (3) a presentation of the results of a study of hierarchy formation in an animal group; (4) a new approach to the theory of hierarchy formation; and (5) an evaluation of the new approach as well as a discussion of the implications of the results for continuing research.

---

* Direct all correspondence to: Ivan D. Chase; Department of Sociology; State University of New York (SUNY); Stony Brook, New York 11794.

An earlier version of this paper was presented at the 1978 Annual Meeting of the American Sociological Association in San Francisco. Many colleagues have commented on and contributed to this paper, and I am grateful for their support. In particular, I would like to thank Jack Hailman for his incisive discussion and general encouragement. Jeffrey Baylis, Ann Becker, Shelly Cohen, Ken Feldman, Mark Granovetter, Gene Weinstein, Bill Wimsatt, and one anonymous reviewer provided many helpful criticisms of earlier versions of the paper, and I am most appreciative of their efforts. I would like to thank Gordon Stephenson for help with the SSR System, Sara Edwards and Nancy Raffetto for assistance with data collection, and Erica Jen for computer wizardry and assistance with techniques of analysis. Veronica Abjornson, Carole Roland, and Rosemarie Sciales speedily turned rough draft into final copy, and Mary Ann Huntington drew the figures. Part of the research reported here was conducted under a grant to the author from the Harry Frank Guggenheim Foundation, and the observational data were gathered in the Department of Zoology, University of Wisconsin-Madison. The support of both the Foundation and the Department are gratefully acknowledged.

HIERARCHY STRUCTURES IN ANIMAL AND HUMAN GROUPS

The Animal Behavior Approach

One of the distinguishing features of the animal behavior or ethological tradition is the emphasis on behavioral observation and the use of ethograms (or catalogs of behavior) developed for each kind of animal or human group studied. In a typical ethological study, a group is observed for a period of time sufficiently long to discover the direction of dominance relationships among all possible pairs in the group. The direction of a dominance relationship in a pair is typically determined by asymmetries in the agonistic (a category including both aggressive and submissive behaviors) behavior of the individuals toward one another. For example, the individual who delivers all or the majority of aggressive acts in a pair is considered dominant, and the individual who receives the acts or initiates most of the submissive gestures is considered subordinate. The pattern of the relationships in a group is frequently represented in a "dominance matrix." A dominance matrix has either a "0" or a "1" in all off-diagonal cells: a "1" in the i, j cell indicates that individual i dominates individual j and a "0" indicates that j dominates i.

All the dominance relationships in a group, taken together, form the hierarchy structure. Theoretically, hierarchy structures can vary from linear, at one extreme, to the other extreme in which each individual in the group dominates an equal number of other individuals. In a linear hierarchy there is an individual A who dominates all others, an individual B who dominates all but A, and so forth, down to the last individual who dominates no one. An example of a dominance matrix for a linear hierarchy with five individuals is given in Table 1, and a matrix for a hierarchy as far away from linearity as possible—in which each animal dominates an equal number of animals—is given in Table 2.

Table 1. Dominance Matrix Showing a Linear Hierarchy

Dominant Animal	Dominated Animal					Number Dominated
	A	B	C	D	E	
A	—	1	1	1	1	4
B	0	—	1	1	1	3
C	0	0	—	1	1	2
D	0	0	0	—	1	1
E	0	0	0	0	—	0

A surprisingly uniform finding by ethologists is that hierarchies in small groups (less than about ten members) are frequently and perhaps predominantly linear or near-linear. This finding is stable across many individual researchers' work and across an extremely broad range of animal species from insects to primates. For example, linear and near-linear hierarchies have been found among certain kinds of wasps and bumble bees (Wilson, 1971); various birds—such as chickens (Guhl, 1975), chaffinches (Marler, 1955), and red crossbills (Tordoff, 1954); domestic mammals—such as cows (Schein and Fohrman, 1955) and ponies (Tyler, 1972); wild mammals—such as coyotes (Bekoff, 1976) and buffaloes (McHugh, 1975); and primates in the wild and in captivity—including rhesus monkeys (Sade, 1967), baboons (Hausfater, 1975), and vervets (Struhsaker, 1967). In the primates, hierarchies can become more complex through both the formation of coalitions and what is known as dependent rank (Jolly, 1972). In dependent rank, a particular individual (animal) is able to achieve dominance over another animal in the presence of a third animal (frequently its mother or consort) but not able to do so if the third animal is not present. However, even in those groups where coalitions and dependent rank are found, there is often a linear or near-linear

Table 2. Dominance Matrix Showing a Hierarchy in Which Each Animal Dominates an Equal Number of Other Animals

Dominant Animal	Dominated Animal					Number Dominated
	A	B	C	D	E	
A	—	1	1	0	0	2
B	0	—	1	1	0	2
C	0	0	—	1	1	2
D	1	0	0	—	1	2
E	1	1	0	0	—	2

hierarchy if relationships are examined between pairs when a coalition partner or third party conferring dependent rank is not present.

Researchers in the ethological tradition who have studied human groups have frequently focused on hierarchical relationships; many of these studies have been on preschool children (McGrew, 1972; Missakian, 1976; Strayer and Strayer, 1976), but adolescent groups have also been examined (Savin-Williams, 1977; 1979; 1980). In addition to using nonverbal, behavioral measures of dominance, as in the animal studies, some studies have also employed verbal indices of dominance—for example, if A gives an order to B and B obeys, then A is rated as dominant to B (Savin-Williams, 1977; 1979; 1980). As in the animal studies, findings indicate that human hierarchies are frequently linear or near-linear (Missakian, 1976; Savin-Williams, 1977; 1979; 1980). In those studies where data have been collected for some but not all possible pairwise interactions, the data indicate that only a few interactions deviate from those expected in a linear hierarchy (McGrew, 1972; Strayer and Strayer, 1976).

Another uniform trend is that most studies by ethologists have been static. That is, in the great majority, studies have described a given hierarchy structure at some particular time rather than either explaining how that hierarchy structure was established or examining the processes by which members of the group changed rank over time.

The Human Social Science Approach

The research of sociologists and social psychologists, like that of ethologists, has provided, for the most part, static descriptions of social structure in small groups. There are a few notable exceptions (such as Newcomb's [1961] work) but these tend to utilize self-reported preference (friendship) data rather than observational data on asymmetric relationships. These static descriptions indicate a strong hierarchical component in the social structure of a broad spectrum of human small groups. Three areas of research are of relevance here: the study of human small groups in laboratory settings, the study of these groups in "natural" situations, and the analysis of sociometric data.

When groups of unacquainted individuals are assembled in laboratory settings, a differentiation of members quickly emerges along such dimensions as frequency of originating and receiving various kinds of behavioral acts and ratings of leadership and likeability by fellow group members (see Collins and Raven, 1969; Gibb, 1969; and Hare, 1976, for comprehensive reviews). Some members show behavioral profiles that give them relatively high control over the actions of their fellows and over group activities in general, while other members exert relatively little influence on either group activities or their colleagues (e.g., see Bales and Slater, 1955, and the reviews cited above). Researchers using the status expectation paradigm have shown that external status characteristics—such as social class, race, and gender—influence the distribution of participation and prestige in these groups (see Berger et al., 1972, for a general statement of the paradigm and Fennell et al., 1978, for a provocative discussion of differences in male and female groups).

Although ethologists and laboratory small-groups researchers frequently collect the same kind of data—individual A directs an act of type X to individual B—their analyses of the data are different. As mentioned above, ethologists look at the hierarchy structures produced by all possible pairwise relationships among group members. Laboratory small-groups researchers, however, tend to concentrate on rank measures—the comparison of individuals in terms of total interaction rates or group preference scores. Bales et al. (1951) is a partial exception in that they give pairwise interaction frequencies for 18 aggregated sessions of six-man groups.

Studies in natural settings—such as those by Blau (1955) of a bureaucratic agency, Hanfmann (1935) of kindergarten children, Homans (1950) of an industrial work group, and Whyte (1955) of gangs—provide further confirmation of the hierarchical structure of human small groups. These studies all find clearly differentiated status rank systems based upon both the behavioral actions and sentiments of group members. Although most of these studies, like the laboratory small-groups studies, do not provide data on all possible pairwise relationships in their groups, the work of Hanfmann (1935) is an exception. Her data indicate the presence of a near-linear hierarchy in kindergarten children, comparable to those reported by ethologists for other human and animal groups. (See Mazur, 1973, for a pioneering comparison of hierarchies in humans and animals; his approach and conclusions, however, differ from those presented here.)

The studies of sociometric data by Davis (1970), Hallinan (1974), and Holland and Leinhardt (1970; 1971; 1972) provide some of the most sophisticated descriptions available of social structure in small human groups. Although one might expect preference choices to yield structures considerably different from those produced by dominance relationships, there are in fact considerable similarities. These researchers have discovered (with high consistency across studies and samples) that there is a strong trend toward transitivity in the preference relations of group members (for example, if A chooses B as a friend and B chooses C, than A will choose C also). As will be explained in greater detail, there is a similar strong trend toward transitivity in the dominance relationships of human and animal groups. In a linear hierarchy all possible triads have transitive dominance relationships; and, in a near-linear hierarchy almost all the possible triads have transitive relationships. Davis (1970:850) suggests that, with more refined data where the stronger preference relation for every pair in a group could be determined, "sociometric data would tend to fit the model of a transitive tournament." A transitive tournament is another term for a linear hierarchy and, if Davis's suggestion is correct, then social structures produced properly refined preference relationships, and dominance relationships would be identical.

THEORIES PROPOSED FOR THE EXPLANATION OF HIERARCHY STRUCTURES

I will now examine theories proposed—explicitly and implicitly—in both the animal behavior and human social sciences literature, to explain the formation of hierarchy structures. This examination is based upon previous research by the author (Chase, 1974). The procedure used in that study was this: various theories of hierarchy structure were expressed in their basic (core) form, and that core form was converted into a mathematical model, a simple mathematical expression of what the core form implied. Then the stringency of the conditions needed by each mathematical model to predict the empirically common linear and near-linear hierarchies was examined and the available data inspected, to determine if the conditions were met.

The two mathematical models that proved to encompass most of the explanations of hierarchies were a correlational model and a pairwise interaction model. For example: explanations that are basically correlational models are those that indicate that individuals' positions in a hierarchy are determined by their physical attributes, their genetic endowment, their hormonal state, their past social performance, their personality traits, the social labels that they have been given, or any composite of these factors. In order to account for linear and near-linear hierarchies, correlations between ranks in a hierarchy and their scores on any of the above-mentioned variables or composites would have to be .9 or greater (Chase, 1974). This theoretical result holds for linear and near-linear hierarchies no matter what species of animal, including humans, are involved and no matter what variables or composite of variables are used to predict rank in a hierarchy. This is a high correlation coefficient, indeed, and one which indicates that any correlational explanation must fulfill a stringent mathematical condition.

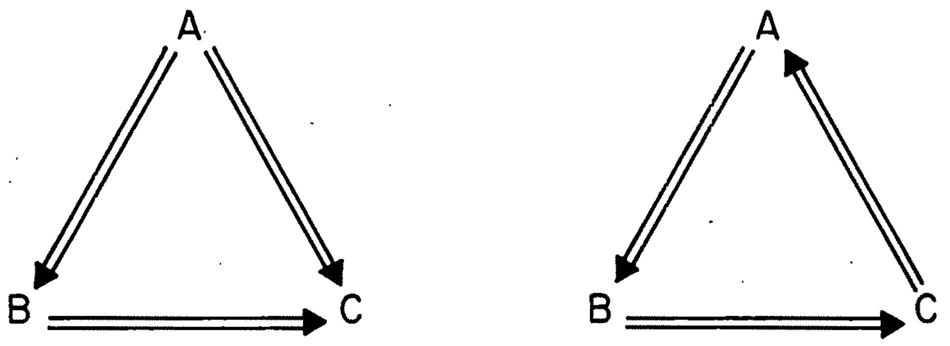
An examination of the literature indicates that correlations of .9 or higher are not usually found between a factor predicting dominance and rank in a hierarchy. Some of the best correlational data available are for animals and, specifically, for the premier animal of dominance studies—the chicken. Correlations vary in size for different investigations and, in studies that examined the association between an individual's aggressiveness—as indicated by its success in fights with other animals, and its place in a hierarchy—correlations ranged from about .4 to .8 (Chase, 1974). These correlations, then, were not as high as the target value of .9, although some were near.

Explanations of hierarchies that are pairwise interaction models in their canonical form are, for example, differences in fighting ability for animals, theories dealing with the interaction of personality types for humans, and exchange theories for humans. In this model it is assumed that each member of a group has a pairwise contest with each other member, that the winner of a contest dominates the loser in the group hierarchy, and that an individual has a particular probability of success in each contest. This model requires that there must be one individual with a .95 probability of dominating each other individual, a second individual with a .95 probability of beating every one but the first individual, and so forth down to the last individual who has only a .05 probability of beating any other individual (Chase, 1974). These results hold, regardless of the species of animal and regardless of what factors are assumed to explain success in pairwise interactions. As in the correlational model, these results indicate that stringent mathematical conditions must be fulfilled with the pairwise interaction model. Again, it has been found that data which best determined if these conditions were met were from animals, specifically chickens, and that the required conditions were *not* fulfilled (Chase, 1974).

It is not that correlational and pairwise encounter theories and measurements give us no useful information about the success of individuals in dominance interactions or the places of individuals in hierarchies; they do, indeed; but the kind of information they give us is not sufficient to account for a group level social structure, the overall hierarchy found in a group. Although individuals in a group may differ, it appears that the differences are not strong enough to explain empirically common dominance structures (see Hallinan, 1974, for a similar argument concerning individual differences and sociometric structures).

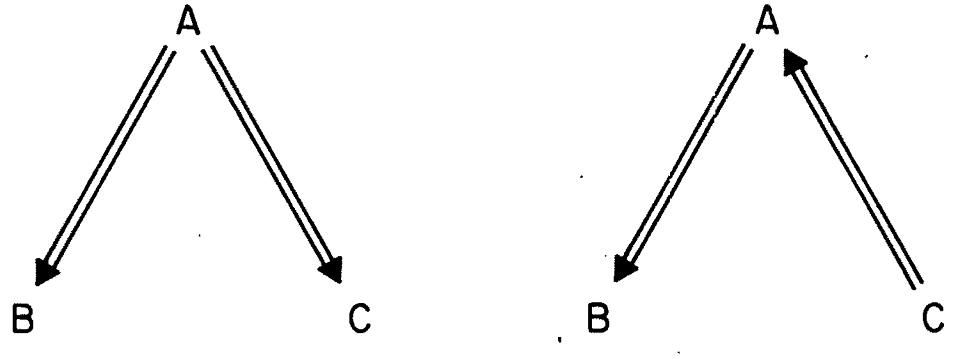
TRIADS AND THE FORMATION OF HIERARCHIES

If many of the currently accepted theories of hierarchies and dominance relations are not adequate, then what might explain the presence of linear and near-linear hierarchies so frequently found in human and animal groups? As discussed above, theories that attempt to explain hierarchy structure by correlations (or the characteristics of single individuals) and theories that use pairwise interactions (or the characteristics of pairs of individuals) appear to be inadequate. As Simmel realized long ago, three is a very important number in sociology, and I shall show that the structure of interaction in triads is very important for explaining the formation of linear and near-linear hierarchies. In order to understand the importance of triadic interaction, one must be aware of a simple mathematical fact: in a linear hierarchy all possible triads have transitive dominance relationships, and if a hierarchy is not linear it contains at least one triad with intransitive dominance relationships—and the fewer the intransitive triads, the closer the hierarchy to linearity. In a triad with transitive dominance relationships, if individual A dominates B and B dominates C, then A also dominates C. So, A dominates the other two members, B dominates C, and C dominates no one. Figure 1A shows a triad with transitive dominance relationships. In a triad with intransitive dominance relationships, the three individuals cannot be arranged in rank order because each individual dominates one other individual. That is, A dominates, say, B; B dominates C; and C dominates, rather than is dominated by, A. Figure 1B shows



A. Transitive Triad B. Intransitive Triad

Figure 1. The Configuration of Dominance Relationships in a Transitive and an Intransitive Triad



A. B.

Figure 2. (A.) Final Configuration of Dominance Relationships in All Triads Having Two Extant Dominance Relationships at the End of the Observation Period. (B.) Initial Configuration of Dominance Relationships in Triad with One Reversal

a triad with intransitive relationships. In a linear hierarchy all individuals can be ranked uniquely from top to bottom by the number of group members they dominate, and, therefore, any three can be ranked with respect to one another and form a transitive triad (this can be seen, for example, by taking all possible subgroups of three in Table 1). In a hierarchy which is not linear, not all individuals are uniquely ranked by the number of group members they dominate—there is at least one tie and, therefore, at least one intransitive triad. Table 3 shows the dominance matrix for a group in which there is a tie in rank between A, B, and C and one intransitive triad involving A, B, and C.

Table 3. Dominance Matrix Showing a Hierarchy with One Intransitive Trait Involving A, B, and C

Dominant Animal	Dominated Animal					Number Dominated
	A	B	C	D	E	
A	—	1	0	1	1	3
B	0	—	1	1	1	3
C	1	0	—	1	1	3
D	0	0	0	—	1	1
E	0	0	0	0	—	0

Experimental Design and Data Collection

Could linear and near-linear hierarchies be so common because animals and humans use behavioral processes which favor the formation of transitive triads and discourage the formation of intransitive triads? (Cf. Davis, 1970; Feld, 1980; Hallinan, 1974; Holland and Leinhardt, 1970; 1971; 1972 for discussions of forces promoting transitivity in preference relationships.)

In order to answer this question, I designed an experiment to determine the actual extent of transitive dominance relationships in groups of three individuals and, if transitivity were common, to discover the behavioral processes by which transitive relationships were generated. The species of choice for this experiment was the chicken; hierarchies in chickens are often linear or near-linear as are hierarchies in more socially complex species such as primates and humans, but at the same time the behavior of chickens is simple enough so that it is more tractable to analysis. In addition, the most extensive dominance research on any species is on the chicken.

In this experiment, groups of three previously unacquainted chickens were placed in a neutral cage, and all aggressive actions among the individuals were recorded. Twenty-four triads were observed; each chicken took part in three triads, and each chicken in a group had been in the same number of triads previously. The data recorded consisted of the identity of the chicken initiating an aggressive action (the attacker), the identity of the chicken attacked (the receiver), the kind of aggressive action (peck, feather-pull, claw (scratch), or jump-on), and the real (clock) time at which each attack occurred. Each triad was observed for four hours to give a grand total of 96 hours of observation time. A combined total of 2,801 aggressive acts was recorded, and the resulting average rate of aggression was 29.2 acts per hour or 116.7 acts per triad.

An SSR Keyboard was used to record the data. The data output from the Keyboard was converted to an electronic signal recorded on audio tape with a standard tape recorder, and the resulting data tapes were transcribed by computer to produce hard copies of the data record and files for later analysis by computer (see Stephenson et al., 1975, and Stephenson, 1979, for a description of the SSR Keyboard and its capabilities). An excerpt from a data record is presented in Table 4.

The Extent of Transitivity in Triads

When groups of previously unacquainted chickens are assembled, there are usually interchanges of aggressive actions between individuals although, in some pairs, one animal immediately submits to the other animal without ever fighting back. After either a short or more protracted series of interchanges, the asymmetric pattern of aggressive acts seen in established hierarchies develops: the interchanges stop and all or almost all of the aggressive actions are performed by one member of a pair against the other. At this point, one can think of a "decision" having been reached: a dominance relationship has been formed with one animal being dominant and the other subordinate.

In order to determine when a dominance relationship had been formed and, thus, to discover the extent of transitivity in the experimental triads, two criteria were used. One animal was considered to be dominant over another if: (1) it delivered any three aggressive actions in a row (any combination of pecks, feather-pulls, jump-ons, or claws) against the other animal and (2) there was a 30-minute period, following the third aggressive act, during which the receiver of the aggression did not attack the initiator. The involvement of either member of the pair

Table 4. Data Excerpt

Act Number	Real Time	Time Interval	Aggressive Acts
0	09:30:25.1	—	Session Begins
1	09:31:01.8	36.7 sec.	3P1[a]
2	09:31:03.5	1.7	3P1
3	09:31:31.8	28.3	1P3
4	09:32:06.8	35.0	1P2
5	09:32:08.6	1.8	1PU2[b]
6	09:32:10.1	1.5	1C2[c]
7	09:32:14.9	4.8	3P1
8	09:32:18.6	3.7	1P2
9	09:32:21.2	2.6	3J2[d]

[a] "P" is the code for a peck.
[b] "PU" is the code for a feather-pull.
[b] "C" is the code for a claw (scratch).
[d] "J" is the code for a jump-on.

with the third member of a triad was not considered in these criteria and neither was the length of time necessary for the completion of the three acts. Once formed, a dominance relationship could be reversed if the subordinate animal fulfilled the criteria. While somewhat arbitrary, these criteria have support as a valid index of stable dominance relationships—of the 54 dominance relationships meeting the criteria in this study, only two were later reversed within the four-hour observation period (afterward, each chicken was returned to its home cage). The average time required to fulfill the criteria in the 52 initially formed relationships was 82.6 minutes, and only two relationships were established within less than 30 minutes before the end of an observation period. There was, then, a sufficient time period after the formation of most relationships for reversals to occur.

An examination of the configuration of relationships at the end of each observation period indicated that 1 triad had no dominance relationships, 17 had two, and 6 had all three possible relationships. The 17 triads with two dominance relationships all had the same configuration—one animal dominated each of the other two. If the two dominance relationships were determined randomly, this configuration would have a probability of .25 and would be expected to occur, by chance, about four times in 17. This configuration is shown in Figure 2A. It is important to note that this configuration guarantees a triad with transitive dominance relationships regardless of which subordinate later comes to dominate the other (preliminary analysis of data from several groups of four chickens each, observed for 12 hours, indicates that subordinate animals tend to form dominance relationships among themselves, in time). If B later dominates C, there is a transitive triad with A on top, B in the middle, and C on bottom. If C later dominates B, there is a transitive triad with A on top, C in the middle, and B on the bottom.

Similarly, the 6 triads with three dominance relationships all had the same configuration, a triad with transitive relationships as shown in Figure 1A. A transitive triad has a probability of .75 of occurring if relationships are determined randomly—and would be expected to occur between four and five times out of six. Taken together, these findings demonstrate a marked tendency toward transitivity in the experimental triads, and this finding corroborates those of the studies of human and animal groups reviewed earlier.

An examination of the two groups with reversals in previously formed dominance relationships gives additional support for the tendency toward transitivity. In one of these triads, animal A initially dominated B but was herself dominated by C (see Figure 2B). With this configuration an intransitive triad would have developed had B come to dominate C or a transitive triad if C had come to dominate B. In actual fact, A and C reversed their relationship, and this produced the configuration shown in Figure 2A which guaranteed a transitive triad. In the other triad, the initial configuration of relationships was intransitive, as shown in Figure 1B. By the end of the observation period, A and B reversed their dominance, with the triad having a transitive relationship shown in Figure 1A resulting.

Behavioral Processes Leading to Transitivity

I now present a data analysis that shows how the interaction process in triads helps to explain the tendency toward transitive dominance relationships. The first part of the analysis treats the long-term structure of the interaction process and the second part, the short-term structure.

1. Long-term Processes. This analysis of long-term processes includes the 17 triads with two relationships and the 6 with three relationships and excludes the 1 triad with no relationships. The analysis examines the effect of "winning" and "losing"—that is, the impact of either gaining dominance or becoming subordinate upon the further aggressive behavior of a chicken.

The analysis begins with the 16 triads in which only two dominance relationships were formed (no reversals) and 1 triad with three relationships in which one animal first became dominant over the other two and then one of the subordinates became dominant over the other. (All other triads will be examined later.) An inspection of the first dominance relationship formed in each of the 16 triads with two relationships reveals that there is one uniform pattern of behavior followed by each animal becoming dominant, and another pattern by each animal becoming subordinate. The rule of strategy followed by each subordinate is to withdraw almost totally from all subsequent aggressive activity—not only against the animal that has become its superior but also against the bystander animal. None of the sixteen animals becoming the first subordinate in these triads ever became dominant over the bystander, nor did any ever reverse its relationship with its superior. In addition, the rate of aggressive activity of these animals dropped to almost zero after they received the third attack from the chicken to which they became subordinate; their average rate of aggressive activity before the third attack was 15.8 acts/hr. and after this attack it was 0.2 acts/hr. (significant at the $p < .02$ level, Wilcoxon signed rank test). On the other hand, each chicken winning the first dominance encounter went on to be aggressive toward the bystander and to gain dominance over her.

There is a slight variation on these rules found in the single triad in which one chicken first became dominant over the two others and then one of the subordinates became dominant over the other. In this triad, the strategy of the initially dominant chicken was the same as in the preceding paragraph; she went on to attack—and win dominance over—the bystander. The loser of the initial dominance encounter, however, showed a small variation in strategy. After the third attack by the animal to which she became initially subordinate, she initiated no aggressive acts at all for 115 minutes; then she began to attack the bystander, later winning dominance over her.

The next part of the analysis concentrates on 4 triads in which three dominance relationships were formed. Figure 3 shows the order of formation of dominance relationships in these triads. In these triads the chicken losing the first dominance encounter lost a second dominance encounter with the bystander, and, then, the animal who won the first dominance contest engaged the bystander (who had won the second dominance contest in the group) in a contest to determine dominance. The strategy of the initial subordinate was the same as in the groups above: she decreased her aggressive activity, and she did not become dominant over the bystander or reverse her relationship with her initial superior. The strategy taken by the winners in these triads is similar to that of the winners in the groups above; winners here directed aggressive acts toward the animal not yet dominated—that is, the winner of the initial contest and of the second contest (the bystander) battled for dominance. Initial dominators won over bystanders in three out of four cases, but no matter which won, a transitive triad was guaranteed by the initial configuration of relationships (A and C both dominated B).

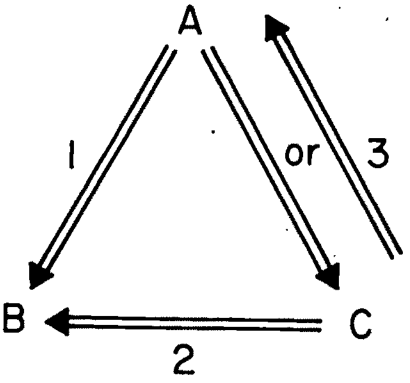


Figure 3. Configuration of Dominance Relationships in the Transitive Triad Resulting When the Initial Dominant and Bystander Engage in a Dominance Contest After Each Has Won over the Initial Subordinate

The final collection of groups to be examined are the 2 triads in which an initially formed dominance relationship was later reversed. There were two extant dominance relationships at the end of the observation period in one of these triads and three relationships in the other. Figures 4A and 4B show the order of formation of relationships in these triads. In the group ending with two relationships, the

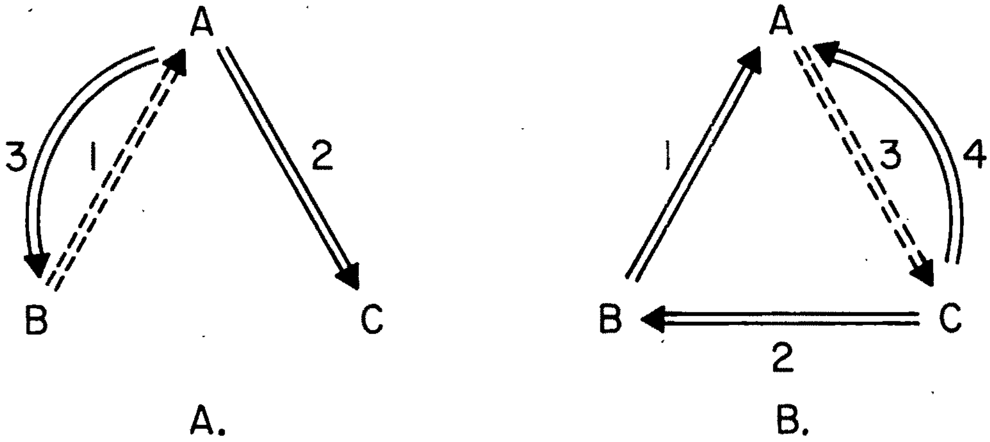


Figure 4. The Order of Formation of Dominance Relationships in the Two Triads with Reversals (Dominance Relationships Later Reversed Are Indicated by Dashed Lines)

initial subordinate became dominant over the bystander, and then reversed her relationship with the initially dominant chicken. The initial subordinate in this group did not follow the same strategy as losers of initial contests in the groups discussed above; she did not withdraw from aggressive activity. The winner of the initial contest also violated the usual strategy—she did not engage the bystander in a dominance contest. However, after the loser of the initial encounter became dominant over the bystander, she followed the usual winner's strategy of engaging the animal not yet dominated, and in this case the engagement resulted in a reversal.

In the group ending with three relationships, the initially dominant chicken was later dominated by the bystander, and the bystander was, in turn, dominated by the initially subordinate chicken. These relationships resulted in an intransitive triad until the bystander reversed her relationship with the initial subordinate to form a transitive triad lasting until the end of the observation period (see Figure 4B). In this triad, the bystander came to dominate the initial dominant rather than the initial subordinate as in the other triads in which the bystander forms a dominance relationship, the initially dominant chicken lost to the bystander (a rare event, it seems—based upon the data collected for this study, anyway), and the initial subordinate withdrew from aggressive activity only briefly and then won a dominance encounter against the bystander. At this point, each member of the triad was both a winner and a loser vis-à-vis the other triad members, and one of them (the bystander) followed a usual winner's strategy of engaging the animal not yet dominated; that engagement then resulted in a reversal and the formation of a triad with transitive relationships.

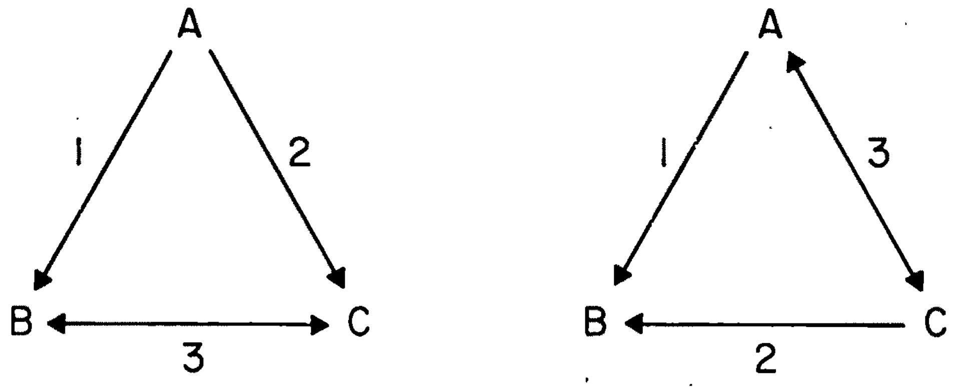
The usual strategies of both winners and losers (discovered here) help to explain the high proportion of triads with transitive relationships and triads guaranteed to be transitive. First, the withdrawal strategy of the initial subordinate guarantees that she will not be the dominant animal in the next relationship formed. Her withdrawal, therefore, obviates the possibility of a configuration of the type in which A dominates B and B dominates C. Without a configuration of this type there is no possibility for the formation of a triad with intransitive relationships (with a configuration of this type a transitive triad could still result, depending upon the direction of the third relationship). Second, the strategy of the initial winner—to engage the animal not yet dominated—guarantees the formation of at least two relationships in a triad. There are three subcases to be examined here. In the first subcase, the bystander does not win a dominance contest against either the initial subordinate or the initial dominant, and the initial dominant wins against the bystander. This results in a triad with a dominance configuration of the type in which A dominates both C and B, and such a triad is guaranteed to have transitive relationships, regardless of which of the subordinates later comes to dominate the other. In the second subcase, the bystander wins a dominance contest against the initial subordinate, and then both the initial dominant and the bystander follow the winner's strategy and engage in a dominance contest with each other. No matter which wins, a transitive triad will result; the transitivity is guaranteed by the configuration formed by the first two dominance relationships—A and C both dominate B. The third subcase is a residual collection in which, for example, a loser violates the usual strategy and wins against the bystander or a bystander wins against the original dominant in forming the second dominance relationship in a triad. In these situations the eventual employment of the usual winner's strategy of engaging the animal not yet dominated can result in a reversal and a transitive configuration or a configuration which guarantees transitivity (see the analysis of the triads with reversals, for examples).

2. *Short-term Processes.* Sequences of two successive aggressive actions can involve either just two, or all three, members of a triad. There are two possible kinds of sequences involving just two members: one animal attacks another twice in a row (Repeat sequence) or one animal attacks another and the receiver returns the attack (Interchange sequence). Acts numbers 1 and 2 in the Data Excerpt given in Table 4 are an example of a Repeat sequence, and acts numbers 2 and 3 are an example of an Interchange sequence.

These two pairwise sequences, in contrast to the triadic sequences that will be defined below, have no implication, in and of themselves, for the formation of transitive or intransitive triads. Therefore, although the pairwise sequences are interesting in their own right, the analysis here will concentrate on the triadic sequences.

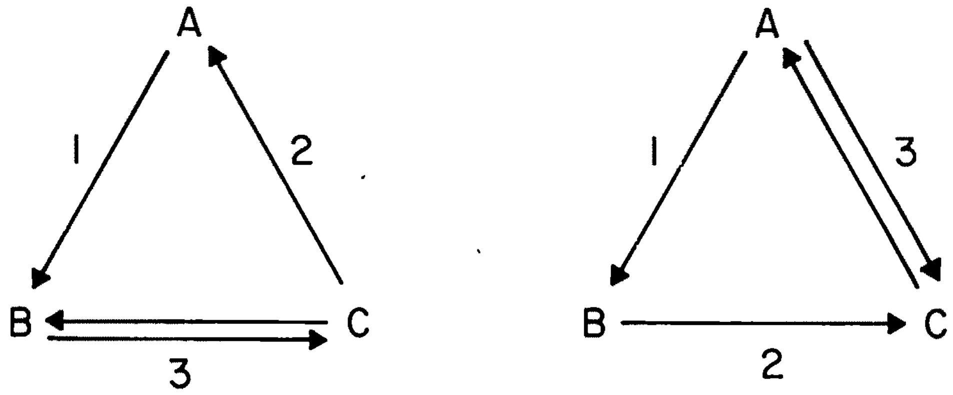
There are four possible sequences involving all three members of a triad. At some point, one member of the triad attacks another member—almost without exception in experimental groups of chickens, attacks involve only two individuals at a time (the problem of coordinated attacks involving three or more individuals at a time, in such animals as primates, will be discussed later). The attacker in this encounter is labeled as A, the receiver as B, and the bystander as C. After A attacks B there are four *and only four* possible sequences involving C. C and A can interact and there are two possibilities here: A can attack C or C can attack A. Or C and B can interact, and there are also two possibilities here: C can attack B or B can attack C. The resulting total of four possible sequences of attacks is shown in Figure 5. Pattern I is the Double Attack sequence: A first attacks B and then A follows with an attack on C. Pattern III is the Attack the Attacker sequence: A attacks B and then A is attacked by C. Pattern II is the Double Receive sequence: A attacks B and then C attacks B. Pattern IV is the Pass On (the Attack) sequence: A attacks B and then B attacks C. In the Data Excerpt of Table 4 acts numbers 3 and 4 form a Double Attack sequence; acts numbers 8 and 9, a Double Receive sequence; acts numbers 7 and 8, a Pass On sequence; and acts numbers 6 and 7, an Attack the Attacker sequence.

Two of these sequences have a different implication for the formation of transitive and intransitive triads based upon attack relationships than do the other two. (The reader should understand that transitivity and intransitivity are properties of relationships in general, not just of dominance relationships. Thus, if A dominates B, B dominates C, and A also dominates C, a transitive triad based on dominance relationships results; and if A attacks B, B attacks C, and A also attacks C, a transitive triad based on attack relationships results.) Patterns I and II, the Double Attack and the Double Receive sequences, guarantee the presence of a triad with transitive attack relationships—no matter what the direction of attack in the third pair of the triad. This lack of dependency upon the attack in the third pair is indicated by the double-headed arrows in Figures 5I and 5II. For example, in the Double Attack sequence, if B later attacks C,



I. Double Attack

II. Double Receive



III. Attack the Attacker

IV. Pass On

Figure 5. The Four Possible Triadic Attack Sequences (Attacks Are Numbered in Order of Occurrence)

the transitive triad in Figure 6A results; or if C later attacks B, the transitive triad in Figure 6B results. The problem of going from transitive triads based upon attack relationships to transitive triads based upon dominance relationships will be treated later.

In direct contrast, the Attack the Attacker and Pass On sequences can give rise to triads with either transitive or intransitive relationships, depending upon the direction of attack in the third pair. For example, in the Attack the Attacker sequence, if C attacks B, the transitive triad in Figure 7A results, but if B attacks C, the intransitive triad in 7B results. This dependency is signified by separate arrows in either direction for the third pair in Figures 5III and 5IV.

Given this difference in attack sequences, a question naturally arises: Are chickens favoring one or both of the sequences that guarantee transitivity over those not favoring transitivity? An analysis was performed to answer this question by determining the usage of the four triadic sequences. In this analysis, the first and second aggressive actions occurring in a triad were examined. If these two actions involved only two members of the triad, then the sequence was categorized as either a Repeat or an Interchange pattern as described above. If, however, the two actions involved first two members of the triad and then the third member, the sequence was categorized as one of the four triadic patterns. After comparing the first and second aggressive actions, the analysis continued by comparing the second and third

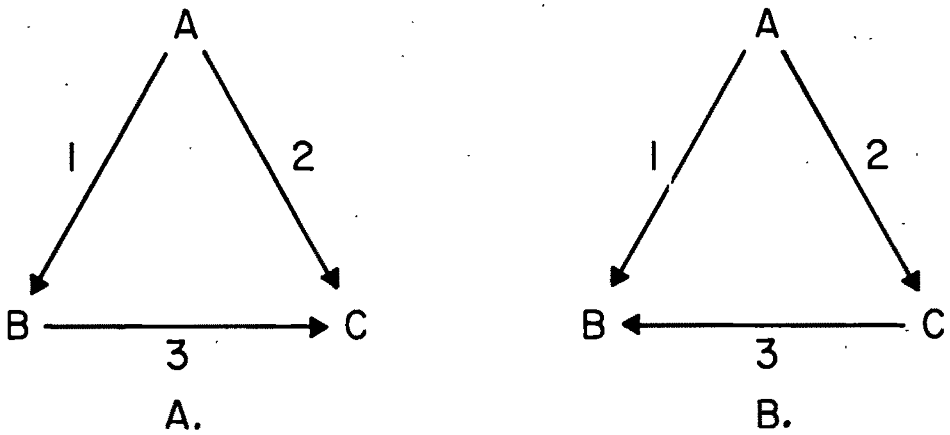


Figure 6. The Two Possible Transitive Triads Resulting from a Third Attack Following the Use of a Double Attack Sequence

actions, the third and fourth, and so on. As a result, in this method of analysis each action is considered once as the first act in a sequence and once as the second act. In addition, this method of analysis reidentifies the initial attacker, the initial receiver, and the bystander for each two-act sequence. Using this method of analysis on the data in Table 4, the first three sequences would be counted in this way: acts 1 and 2 are a Repeat sequence, acts 2 and 3 an Interchange sequence, and acts 3 and 4 a Double Attack sequence.

Distribution of the four sequences as frequencies as well as percentages of all triadic sequences observed in 23 triads combined are given in Table 5. (One triad was omitted from this analysis; this triad had no incidences of triadic sequences and showed only a single Repeat pattern.) Column 6 of the table indicates the total usage of the four sequences not broken down by time between the initial and following acts of the sequence. The Double Attack sequence composes the overwhelming majority of all triadic patterns—86.6%. The Double Receive, Pass On, and Attack the Attacker sequences account for comparatively small percentages—2.7, 5.3, and 5.4, respectively—of all triadic patterns. If attacks are at random, each of the four patterns has an equal (.25) probability of occurring. The actual distribution of sequence patterns is, however, markedly different from that expected by chance

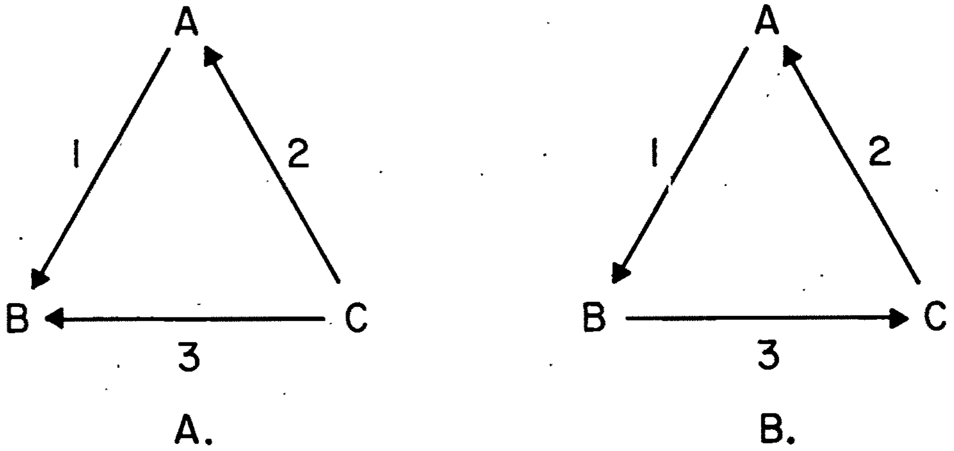


Figure 7. The Possible Transitive (A) and Intransitive (B) Triads Resulting from a Third Attack Following the Use of an Attack the Attacker Sequence

Table 5. Percentage and Frequency Distribution of Triadic Sequences by Time Interval between First and Second Acts (Frequencies Are in Parentheses)

	Time Interval (in sec.)					
Triadic Sequence	0–9.9	10.0–29.9	30.0–59.9	60.0–299.9	Over 300	Total
Double Attack	87.9 (123)	90.3 (102)	88.0 (73)	88.4 (137)	76.8 (76)	86.6 (511)
Double Receive	1.4 (2)	2.7 (3)	2.4 (2)	2.6 (4)	5.0 (5)	2.7 (16)
Pass On	7.1 (10)	.9 (1)	4.8 (4)	4.5 (7)	9.1 (9)	5.3 (31)
Attack the Attacker	3.6 (5)	6.2 (7)	4.8 (4)	4.5 (7)	9.1 (9)	5.4 (32)

(significant at the $p < .001$ level, using a multivariate normal test).

The finding that the Double Attack pattern comprises the large majority of all patterns is robust across sequences, with different time periods between intitial and following acts. This can be seen by examining the first five columns of Table 5 in which sequences are broken down by the time, in seconds, between the initial and following acts. For the first four time periods, the proportion of Double Attacks hovers around 90%, and in the longest period of separation, 300 seconds (five minutes) or more, the proportion drops somewhat to 76.8%.

The finding that most triadic sequences are Double Attacks is also robust across individual experimental groups. Of the 24 experimental triads, there were 10 triads (41.7% of all triads) in which Double Attacks composed more than 90% of all sequences, four triads (16.7%) with between 80% and 89% Double Attacks, five triads (20.8%) with between 70% and 79% Double Attacks, and five triads (20.8%) with 43% or lower Double Attacks. (Considering the distribution of triadic sequences in each triad as a sample point, the vector of mean percentages for the patterns is significant at the $p < .001$ level, using a multivariate normal test.) In 79.2% of the triads, then, Double Attacks composed 70% or more of all triadic sequences, and the mean percentage of Double Attacks in the triads was 74.6%.

These data indicate that: (1) in most triads, most of the time, the chickens use the Double Attack pattern—a kind of sequence that guarantees triads with transitive attack relationships, and (2) in most triads, they only rarely use the Pass On and Attack the Attacker sequences that could possibly lead to intransitive triads. These findings suggest that the extremely high proportion of transitive triads and triads that are guaranteed to be transitive (one animal dominates the other two but the relationship between the subordinates is not settled), based on *dominance* relationships, results from the short-term interaction process of chickens in two ways. First, one chicken in a triad tends to repeatedly enact Double Attacks and, in so doing, she fulfills the dominance criteria vis-à-vis the two other members of the group. Second, although chickens may on occasion use the Pass On and Attack the Attacker sequences, all three chickens in a triad do not enact these sequences in a sufficiently consistent and frequent manner so that dominance criteria over the animals being attacked are actually met. That is, in order to have a triad with intransitive *dominance* relationships there must be sufficient and consistent Pass Ons and/or Attack the Attackers to such a degree that a configuration of the sort in which A dominates B and B dominates C results *and*, further, sufficient and consistent Pass Ons and/or Attack the Attackers to such a degree that C comes to dominate A. Pass On and Attack the Attacker sequences are rare, to begin with, in most triads; and the few that do occur are not used consistently enough by the animals to meet the dominance criteria over their fellows in the proper pattern to form a triad with intransitive *dominance* relationships.

THE FORMATION OF LINEAR AND NEAR-LINEAR HIERARCHIES

I now present two empirically testable hypotheses based upon generalizations from the triad study. I propose that the hypotheses can help to direct research efforts and to explain the formation of linear and near-linear hierarchies—not only in groups of chickens larger than triads but also in other animal species and humans. (Data for some primate species will also be reviewed.) The first hypothesis originates from the long-term structure of the interaction process and the second from the short-term structure.

The first hypothesis is as follows:

> A) the winner of a dominance contest tends to subsequently engage and dominate other individuals not yet dominated, but not individuals to which it is already subordinate;
>
> B) the loser of a dominance contest tends not to attack any bystanders until the individual who has defeated it becomes dominant over the bystanders; and
>
> C) if either or both of the two above conditions are violated, so that either a triad with intransitive dominance relationships or an initial configuration that could lead to an intransitive triad arises, then one individual will reverse an existing relationship in such a manner that a transitive triad or an initial configuration guaranteeing a transitive triad will result.

This necessarily somewhat complicated way of verbally stating the hypothesis simply implies that the initial configuration of dominance relationships (in most of the possible triads making up a large group) will be of the type in which A dominates both B and C or in which A and C both dominate B but not in which A dominates B and B dominates C. If either of the first two configurations is present, a triad is guaranteed to be transitive; and if all or most of the possible triads in a group have transitive dominance relationships or are guaranteed to be transitive, the hierarchy formed by the concatenation of the triads will be linear or near-linear. One possible scenario of hierarchy formation arising from this hypothesis—and which I predict to occur very frequently—is as follows: One individual in a group wins successive dominance contests with all the other group members and each loser, in turn, withdraws from aggressive activity. After this first member has finished, one of the subordinates initiates and wins contests with all the other subordinates who again temporarily withdraw from aggressive activity. Then, a second of the original subordinates gains dominance over all but the initial dominant and the first subordinate, and so on, until a complete linear hierarchy is established. In another likely scenario, two individuals each win one or more dominance contests with other group members. After this, the two winning individuals meet in a dominance encounter, and the winner goes on to dominate any remaining group members not yet dominated, while the loser withdraws temporarily from aggressive activity according to part B of the hypothesis.

As indicated, intransitive triads and initial configurations that could lead to intransitive triads do arise when the usual strategies of winners and losers are not followed. Part C of the hypothesis suggests that (1) these structures are not as stable as actual transitive triads or configurations which guarantee transitivity, and (2) previously formed dominance relationships will be reversed to give transitive structures. Part C, in effect, indicates how individuals respond when there is an exception to parts A and B. Other researchers have also found that intransitive triads are not as stable as transitive ones in animals and tend to be converted to transitive structures (Murchison, 1935; Tordoff, 1954). Similarly, Hallinan (1974) and Holland and Leinhardt (1971) postulate that humans have a strong tendency to transform initially intransitive configurations of preference relations to transitive ones.

The second hypothesis states: The Double Attack pattern will be the most frequent of all sequences of two successive attacks involving more than two individuals. In other words, this hypothesis suggests that, of the four possible triadic sequences and the one possible tetradic sequence (A attacks B followed by C attacks D), the most frequent sequence will be the Double Attack pattern. If this hypothesis is supported, then most sequences that occur will generate configurations of attack relationships that guarantee transitivity and those that generate configurations that do not guarantee transitivity will be rare. I would like to hedge (somewhat) my bet on this hypothesis by suggesting that in groups larger than three and which remain together for longer periods there might be a considerable occurrence of Double Receive sequences. This hedge is based upon preliminary observation of larger groups of chickens and on some data for primates which will be reviewed below. In some groups there is what might be called "scapegoating"—several members successively attack one low-ranking individual. These successive attacks on the "scapegoat" generate large numbers of Double Receive patterns. In any event, the Double Receive pattern, like the Double Attack, guarantees a transitive attack configuration, and either one or both patterns working together would help produce linear and near-linear hierarchies.

AGGRESSIVE SEQUENCES IN PRIMATES

The hypotheses presented above are derived from my own observational studies of chickens, and one might question the applicability of these hypotheses to other animal species and to humans. Are there indeed similar long- and short-term processes in the interactions of humans and other species that help to explain the reported prevalence of linear and near-linear hierarchies? As I mentioned earlier, animal behaviorists, sociologists, and social psychologists have provided relatively few studies of the dynamics by which hierarchies are formed. It is, therefore, not possible at the present time to rigorously test the validity of these hypotheses for human and other animal groups. There are, however, some suggestive reports in the primate literature and these shall now be reviewed. These studies give information about sequences of aggressive acts in primate groups with previously established relationships rather than sequences during the formation of dominance relationships as in the chicken experiment.[1] I have found no quantitative studies of the strategies used by winners and losers during the formation of relationships in primate groups.

Each of the four triadic sequences described has also been reported in various primate species. For example, the Double Receive sequence is reported in rhesus monkeys, Java monkeys, baboons, and langurs (Varley and Symmes, 1966; De Waal et al., 1976; Cheney, 1977; Hall and DeVore, 1965; Jay, 1965). Although the Double Attack sequence figures prominently in the results reported here, it has received relatively little recognition as an explicit sequence in primates (which is not to say that it is rare). It is, however, reported in Java monkeys, baboons, and langurs (De Waal, et al., 1976; Hall and DeVore, 1965; Jay, 1965). The Pass On pattern is most recognized as an explicit sequence in primates, and it is referred to by a variety of terms including "redirected," "transferred," or "displaced" aggression. Pass On sequences are reported for rhesus monkeys, Java monkeys, and baboons (Altmann, 1962; Kaufmann, 1967; Jay, 1965; De Waal, et al., 1976; Hall and DeVore, 1965). In some cases an animal attacked redirects the aggression to an inanimate object (Hall and DeVore, 1965). The Attack the Attacker sequence is frequently observed in primate groups when an individual aids a related animal being attacked, but it also occurs among nonrelated animals. This sequence is reported for pig-tailed monkeys, Java monkeys, and langurs (Tokuda and Jensen, 1968; De Waal, et al., 1976; Jay, 1965).

Of these studies, only the one by De Waal et al. (1976) on Java monkeys, close cousins of the more familiar rhesus monkeys, reports the actual frequencies of occurrence of the four triadic sequence patterns.[2] Their study group was considerably different from those in my experiment; it consisted of 17 animals of both sexes and of varying ages from infants to adults. The individuals had been together for a relatively long time, and it is probably safe to assume that most dominance relationships were firmly established rather than in the process of being formed. Their methods of counting triadic sequences were also different from those used in the chicken study. In order to count as a triadic sequence, A had to attack B and one of these two had to be involved in an attack with C less than 15 seconds later. If the second attack took more than 15 seconds, the interactions counted as separate dyadic attacks rather than as one triadic sequence.

---

[1] A particular aggressive sequence used during the formation of dominance relationships may have a different implication than the same sequence used after dominance relationships have been formed. For example, if A attacks B, and B passes on the attack to C, then there is at least a logical possibility that C might later attack A to form an intransitive triad if A does not already dominate C. On the other hand, if A already dominates both B and C and B dominates C, then the same Pass On sequence would not make an intransitive triad possible because C, in all probability, would not later attack A.

Their data indicate that as proportions of all triadic sequences there were 39.7% Double Receives, 22.2% Double Attacks, 20.6% Pass Ons, and 17.5% Attack the Attackers (De Waal et al., 1976:273). In these data, the Double Recieve pattern was used most frequently, with Double Attack a relatively distant second, followed closely by Pass On and Attack the Attacker. Although the Double Attack pattern does not make up the majority of all sequences in the Java monkeys as it did in the chickens, in both groups sequences which guarantee triads with transitive attack relationships are considerably more frequent than those which can lead to either transitive or intransitive triads. That is, the Double Receive and the Double Attack patterns, together, account for 61.9%, a majority of all triadic sequences in the monkeys. There is, then, agreement on the high proportion of sequences guaranteeing triads with transitive attack configurations between the experiment for chickens and one for a primate group under quite different conditions.

These data provide partial corroboration of the second hypothesis proposed above, but they also suggest certain refinements may be necessary for primates. Additional experiments under controlled conditions for both chickens and primates are required to ascertain the extent of the refinements. One necessary refinement concerns the presence of coalitions in primate groups. By coalition in this context I simply mean two or more individuals directly coordinated, interspersed aggressive acts toward another individual. Coalitions in primate groups are mainly of two sorts: (1) two or more animals join in attacking a third, and (2) one or more animals come to the defense of an animal being attacked and attack the original aggressor. These kinds of coalitions are commonly observed, for example, in baboons, rhesus monkeys, vervet monkeys, langurs, and Java monkeys (Cheney, 1977; Southwick et al., 1965; Varley and Symmes, 1966; Struhsaker, 1967; Jay, 1965; De Waal et al., 1976). The first kind of coalition can be considered as a modified type of Double Receive sequence; in this case the successive attacks are made by animals actively coordinating their aggressive actions. In a similar fashion, the second type of coalition can be considered as a variant of the Attack the Attacker sequence. Thus, the hypothesis proposed above concerning the short-term structure of aggressive interactions can be tested in socially complex species like humans and primates as well as in socially simpler animals using both the simple and modified forms of the triadic sequence patterns.

CONCLUSION

For both social scientists and animal behaviorists there are, arguably, three central issues in the study of social structure in small, face-to-face groups: formation, delineation, and consequence. Confronted with a small-group social structure, a researcher in either field might ask how the relationships came about, what the pattern of relationships was like, and what privileges or liabilities were associated with various positions in the structure. Both social scientists and animal behaviorists have made considerable progress with the last two of these issues, but neither group, as I have shown here through the example of hierarchy studies, has dealt satisfactorily with the formation of social structures. An analysis of the extant research demonstrates that common conceptual and methodological problems underlie the inadequacies of each field in dealing with the formation of hierarchy structures. Although it takes many different outward appearances, the conceptual machinery in both fields mainly tries to explain the formation of dominance hierarchies by differences in individual characteristics (including differences of ability in pairwise encounters). Given this conceptual machinery, social scientists and animal behaviorists have developed a variety of sophisticated techniques to measure differences among individuals along a variety of dimensions. I have indicated here that, no matter how sophisticated the techniques of measurement or the models using individual differences, stringent mathematical conditions must be met before this approach can satisfactorily explain the empirically common linear and near-linear hierarchies. This emphasis on measuring things about individuals before they join a group, and correlating these measurements with their subsequent positions, leaves out what happens in between: the behavioral process by which individuals come to take their positions. Thus, the present theoretical approach in both human social sciences and animal behavior has led to a relative absence of empirical studies of the dynamics of hierarchy formation.

---

[2] Although Kummer (1975) does not count frequencies of behavioral sequences, he does present a stimulating analysis which suggests that interactions in mixed-gender triads can help to explain dominance relationships and sexual alliances in larger groups.

Herein, I offer a new and more satisfactory conceptual and methodological framework for the study of hierarchy formation. Here, the structure of the interaction process is seen as explaining the formation of a hierarchy, and the associated methodology deals with recording and analyzing the interactions among group members. Here, also, this approach is shown to view social structure as emerging from the web of interactions, and it naturally requires studies of the dynamic processes which result in hierarchy formation.

The application of this alternative approach led to the discovery of a short-term process, the predominant use of the Double Attack sequence, and a long-term process (usual patterns of behavior by winners and losers) that helped to explain the presence of actual transitive triads and configurations guaranteeing transitivity. These findings generated two hypotheses which proposed explanations for the presence of linear and near-linear hierarchies in animal and human small groups. A review of the primate literature offered partial support for one hypothesis, and it suggested modifications to make the hypothesis more generally applicable.

The results presented here suggest three major implications for future research on hierarchy formation, in particular—and on other kinds of social structures, in general.

The first implication is that the explicit and implicit assumptions made by explanations for the formation of various kinds of social structures should be critically examined. Other explanations based upon individual differences may also suffer from inherent liabilities similar to those for dominance hierarchies. This critical examination would, in appropriate circumstances, include explanations in both human social science and animal behavior. For example, profitable areas of investigation are likely to be wealth and income distribution in humans, resource distributions (mates, territories, etc.) in animals, and leadership and role structure in human small groups (see Chase, 1980, for a criticism of population genetics models as proximate explanations of cooperative and noncooperative behavior in animals).

The second implication is that dynamic studies of dominance and other kinds of relationships can greatly increase our knowledge of small-group phenomena. The lack of such studies is partly explained, as discussed above, by the conceptual framework of present approaches and, partly, it seems likely, by the previous lack of efficient equipment for the recording, storage, and analysis of the necessarily large volumes of interactional data. The growing availability of microcomputer systems and computer-assisted devices for dealing with interactional data is rapidly alleviating this problem.

The third implication is that there is a certain "lawfulness" in the structure of interactions that can explain the configurations of social relationships in small groups. This article demonstrates "lawfulness" in the interactions of chickens in triads, and it goes on to suggest the units of interaction that are responsible for hierarchy formation in larger groups across species. The model developed here indicates how interactions at a micro level—among individuals—can be cumulated to explain how a macro level structure—a dominance hierarchy—is produced.

I suggest that this kind of approach has applicability beyond hierarchy research to the study of other types of social structure in human and animal groups. Future research is needed to assess the validity of this implication.

REFERENCES

Altmann, S. A.
1962 "A field study of the sociobiology of rhesus monkeys." Annals of the New York Academy of Sciences 102:338–435.

Bales, R. F., and P. E. Slater
1955 "Role differentiation in small decision-making groups." Pp. 259–306 in T. Parsons and R. F. Bales (eds.), Family, Socialization and Interaction Process. New York: Free Press.

Bales, R. F., F. L. Strodtbeck, T. M. Mills, and M. E. Roseborough
1951 "Channels of communication in small groups." American Sociological Review 16:461–8.

Bekoff, M.
1977 "Quantitative studies of three areas of classical ethology: social dominance, behavioral taxonomy, and behavioral variability." Pp. 1–46 in B. A. Hazlett (ed.), Quantitative Methods in the Study of Animal Behavior. New York: Academic Press.

Berger, J., B. P. Cohen, and M. Zelditch, Jr.
1972 "Status characteristics and social interaction." American Sociological Review 37:24–255.

Brown, J. L.
1975 The Evolution of Behavior. New York: W. W. Norton.

Blau, P. M.
1955 The Dynamics of Bureaucracy. Chicago: University of Chicago Press.

Chase, I. D.
1974 "Models of hierarchy formation in animal societies." Behavioral Science 19:374–82.

1980 "Cooperative and noncooperative behavior in animals." American Naturalist 115:827–57.

Cheney, D. L.
1977 "The acquisition of rank and the development of reciprocal alliances among free-ranging immature baboons." Behavioral Ecology and Sociobiology 2:303–18.

Collins, B. E., and B. H. Raven
1969 "Group structure: attraction, coalitions, communication, and power." Pp. 102–204 in G. Lindzey and E. Aronson (eds.), Handbook of Social Psychology. Sec. Ed. Reading, Mass.: Addison-Wesley.

Davis, J. A.
1970 "Clustering and hierarchy in interpersonal relations: testing two graph theoretical models on 742 sociograms." American Sociological Review 35:843–52.

De Waal, F. B. M., J.A.R.A.M. van Hoof, and W. J Netto
1976 "An ethological analysis of types of agonistic interaction in a captive group of Java-monkeys (*Macaca fascicularis*)." Primates 17:257–90.

Feld, S.
1980 "The structure of inequality in groups." Unpublished, Department of Sociology, SUNY, Stony Brook, New York.

Fennell, M. L., P. R. Barchas, E. G. Cohen, A. M. McMahon, and P. Hildebrand
1978 "An alternative perspective on sex differences in organizational settings: the process of legitimation." Sex Roles 4:589–604.

Gibb, C. A.
1969 "Leadership." Pp. 205–82 in G. Lindzey and C. Aronson (eds.), Handbook of Social Psychology. (2nd Ed.) Reading, Mass.: Addison-Wesley.

Guhl, A. M.
1975 "Social behavior of the domestic fowl." Pp. 156–201 in M. Schein (ed.), Social Hierarchy and Dominance. Stroudsburg, Penn.: Dowden, Hutchinson, and Ross.

Hall, K. R. L., and I. DeVore
1965 "Baboon social behavior." Pp. 53–110 in I. DeVore (ed.) Primate Behavior. New York: Holt, Rinehart, and Winston.

Hallinan, M. J.
1974 The Structure of Positive Sentiment. Amsterdam: Elsevier.

Hanfmann, E.
1935 "Social structure of a group of kindergarten children." American Journal of Orthopsychiatry 5:407–10.

Hare, A. P.
1976 Handbook of Small Group Research. (2nd Ed.) New York: Free Press.

Hausfater, G.
1975 "Dominance and reproduction in baboons." Contributions to Primatology, Vol. 7. Basel: Karger.

Holland, P. W., and S. Leinhardt
1970 "A method for detecting structure in sociometric data." American Journal of Sociology 76:492–513.

1971 "Transitivity in structural models of small

groups." Comparative Group Studies 2:107–24.
1972 "Some evidence on the transitivity of positive interpersonal sentiments." American Journal of Sociology 77:1205–9.

Homans, G. C.
1950 The Human Group. New York: Harcourt Brace and World.

Jay, Phyllis
1965 "The common langur of North India." Pp. 197–249 in I. DeVore (ed.) Primate Behavior. New York: Holt, Rinehart, and Winston.

Jolly, A.
1972 The Evolution of Primate Behavior. New York: Macmillan.

Kaufmann, John H.
1967 "Social relations of adult males in a free-ranging band of rhesus monkeys." Pp. 73–98 in S. A. Altmann (ed.) Social Communication Among Primates. Chicago: University of Chicago Press.

Kummer, H.
1975 "Rules of dyad and group formation among captive gelada baboons (*Theropithecus gelada*)." Pp. 129–59, Proceedings of the Symposia of the Fifth Congress of the International Primate Society. Tokyo: Japan Science Press.

McGrew, W. C.
1972 An Ethological Study of Children's Behavior. New York: Academic Press.

McHugh, T.
1975 "Social behavior of the American buffalo. (*Bison bison bison*)." Pp. 213–21 in M. Schein (ed.), Social Hierarchy and Dominance. Stroudsburg, Penn.: Dowden, Hutchinson, and Ross.

Marler, P.
1955 "Studies of fighting in chaffinches (1) Behaviour in relation to the social hierarchy." British Journal of Animal Behaviour 3:111–7.

Mazur, A.
1973 "A cross-species comparison of status in small established groups." American Sociological Review 38:513–30.

Missakian, E. A.
1976 "Aggression and dominance relations in peer groups of children 6–45 months of age." Paper presented at the annual meeting of the Animal Behavior Society, Boulder, Colorado.

Murchison, C.
1935 "The experimental measurement of a social hierarchy in Gallus Domesticus: IV. Loss of body weight under conditions of mild starvation as a function of social dominance." Journal of General Psychology 12:296–312.

Newcomb, T. M.
1961 The Acquaintance Process. New York: Holt, Rinehart and Winston.

Sade, Donald S.
1967 "Determinants of dominance in a group of free-ranging rhesus monkeys." Pp. 99–114 in S. A. Altmann (ed.) Social Communication Among Primates. Chicago: University of Chicago Press.

Savin-Williams, R. C.
1977 "Dominance in a human adolescent group." Animal Behavior 25:400–6.
1979 "Dominance hierarchies in groups of early adolescents." Child Development 50:923–35.
1980 "Dominance hierarchies in groups of middle to late adolescent males." Journal of Youth and Adolescence 9:75–85.

Schein, M. W., and M. H. Fohrman
1955 "Social dominance relationships in a herd of dairy cattle." British Journal of Animal Behaviour 3:45–55.

Southwick, C. H., M. A. Beg, and M. R. Liddiqi
1965 "Rhesus monkeys in North India." Pp. 111–59 in I. DeVore (ed.), Primate Behavior. New York: Holt, Rinehart, and Winston.

Strayer, F. F. and J. Strayer
1976 "An ethological analysis of social agonism and dominance relations among preschool children." Child Development 47:980–9.

Stephenson, G. R.
1979 "PLEXYN: a computer-compatible grammar for coding complex social interactions." Pp. 157–84 in M. E. Lamb, S. J. Suomi, and G. R. Stephenson (eds.), Social Interaction Analysis. Madison: University of Wisconsin Press.

Stephenson, G. R., D. P. Smith, and T. W. Roberts
1975 "The SSR system: an open format event recording system with computerized transcription. Behavior Research Methods and Instrumentation 7:497–515.

Struhsaker, T. T.
1967 "Social structure among vervet monkeys (*Cercopithecus aethiops*)" Behaviour 29:83–121.

Tokuda, K., and G. D. Jensen
1968 "The leader's role in controlling aggressive behavior in a monkey group." Primates 9:319–22.

Tordoff, H. B.
1954 "Social organization and behavior in a flock of captive, non-breeding red crossbills." Condor 56:346–58.

Tyler, S. J.
1972 "The behaviour and social organization of the New Forest ponies." Animal Behaviour Monographs 5:87–196.

Varley, M., and D. Symmes
1966 "The hierarchy of dominance in a group of macaques." Behaviour 27:54–75.

Whyte, W. F.
1955 Street Corner Society. (2nd Ed.) Chicago: University of Chicago Press.

Wilson, E. O.
1971 The Insect Societies. Cambridge: Belknap Press of Harvard University Press.
1975 Sociobiology. Cambridge: Belknap Press of Harvard University Press.

WEBER'S LAST THEORY OF CAPITALISM: A SYSTEMATIZATION*

RANDALL COLLINS
University of Virginia

American Sociological Review 1980, Vol. 45 (December):925–942

A systematic formulation is given of Weber's theory of the origins of large-scale capitalism, based upon the lectures given just before his death. This last theory is predominantly institutional, unlike the emphasis upon religious ideas and motivations in his early Protestant Ethic thesis, and unlike his analyses of the world religions. Weber's institutional theory involves a sequence of causal conditions. The outcome of the sequence is capitalism characterized by the entrepreneurial organization of capital, rationalized technology, free labor, and unrestrained markets. Intermediate conditions are a calculable legal system and an economic ethic combining universal commercialization with the moderate pursuit of repetitive gains. These conditions are fostered by the bureaucratic state and by legal citizenship, and more remotely by a complex of administrative, military, and religious factors. The overall pattern is one in which numerous elements must be balanced in continuous conflict if economic development is to take place. Weber derived much of this scheme in explicit confrontation with Marxism. His conflict theory criticizes as well as deepens and extends a number of Marxian themes, including a theory of international capitalism which both criticizes and complements Wallerstein's theory of the world system.

Max Weber had many intellectual interests, and there has been considerable debate over the question of what constitutes the central theme of his life work. Besides treating the origins of capitalism, Weber dealt extensively with the nature of modernity and of rationality (Tenbruck, 1975; Kalberg, 1979; 1980; Seidman, 1980), and with politics, methodology, and various substantive areas of sociology. Amid all the attention which has been paid to these concerns, one of Weber's most significant contributions has been largely ignored. This is his mature theory of the development of capitalism, found in his last work (1961), *General Economic History*.

This is ironic because Weber's (1930) first major work, *The Protestant Ethic and the Spirit of Capitalism*, has long been the most famous of all. The argument that the Calvinist doctrine of predestination gave the psychological impetus for rationalized, entrepreneurial capitalism is only a fragment of Weber's full theory. But many scholars have treated it as Weber's distinctive contribution, or Weber's distinctive fallacy, on the origins of capitalism (e.g., Tawney, 1938; McClelland, 1961; Samuelsson, 1961; Cohen, 1980). Debate about the validity of this part of Weber's theory has tended to obscure the more fundamental historical and institutional theory which he presented in his later works.

The so-called "Weber thesis," as thus isolated, has been taken to be essentially idealist. Weber (1930:90) defines his purpose in *The Protestant Ethic* as "a contribution to the manner in which ideas become effective forces in history." He (1930:183) polemically remarks against the Marxists that he does not intend to replace a one-sided materialism with its opposite, but his correcting of the balance sheet in this work concentrates largely on ideal factors. The germ of Weber's institutional theory of capitalism can also be found in *The Protestant Ethic* (1930:58, 76).[1] But it remained an undeveloped backdrop for his main focus on the role of religious ideas. The same may be said about his (1951; 1952; 1958b) comparative studies of

---

* Direct all correspondence to: Randall Collins; Department of Sociology; University of Virginia; Charlottesville, VA 22903.

I am indebted to Vatro Murvar and other participants at the Max Weber Symposium at the University of Wisconsin at Milwaukee, March, 1978, and to Samuel W. Kaplan, Stephen Kalberg, Guenther Roth, Walter Goldfrank, Norbert Wiley, and Whitney Pope, for their suggestions on an earlier version of this argument.

[1] The list of institutional characteristics given on pp. 21–25 of the English-language edition of *The Protestant Ethic* (1930), however, are not in the 1904–5 original, but are from an introduction written in 1920 (1930:ix–x).

the world religions. These broadened considerably the amount of material on social, economic, and political conditions, but the main theme still stressed that divergent ideas made an autonomous contribution to the emergence of world-transforming capitalism in the Christian West rather than elsewhere in the world.[2] Thus, Parsons (1963; 1967) treats these works as extending the early Weber thesis from Protestantism to Christianity in general, describing an evolution of religious ideas and their accompanying motivational propensities from ancient Judaism up through the secularized achievement culture of the modern United States.

From these works, and from (1968) Part II of *Economy and Society*, it is possible to pull out an extensive picture of institutional factors which Weber includes in his overall theory of capitalism. But *Economy and Society* is organized encyclopedically, by analytically defined topics, and does not pull together the theory as a whole. There is only one place in Weber's works where he brings together the full theory of capitalism as a historical dynamic. This is in the *General Economic History*, and, especially, in the 70-page section comprising Part IV of that work. These lectures, delivered in the winter and spring of 1919–20, before Weber's death that summer, are Weber's last word on the subject of capitalism. They are also the most neglected of his works; *General Economic History* is the only one of Weber's major works that remains out of print today, both in English and in German.

One important change in the *General Economic History* is that Weber pays a good deal more attention to Marxian themes than previously. This is a significant difference from the anti-Marxist comments scattered through *The Protestant Ethic* (e.g., pp. 55–56, 61, 90–91, 183). In the *General Economic History*, Weber reduces the ideal factor to a relatively small place in his overall scheme. During this same period, to be sure, Weber was preparing a new introduction and footnotes for the reissue of *The Protestant Ethic* among his collected religious writings, in which he defended his original thesis about Calvinism. But his claims for its importance in the overall scheme of things were not large, and the well-rounded model which he presents in *General Economic History* does not even mention the doctrine of predestination. Instead, what we find is a predominantly institutional theory, in which religious *organization* plays a key role in the rise of modern capitalism but especially in conjunction with particular forms of political organization.

In what follows, I will attempt to state systematically Weber's mature theory of capitalism, as it appears in the *General Economic History*, bolstered where appropriate by the building blocks presented in *Economy and Society*. This argument involves a series of causes, which we will trace backward, from the most recent to the most remote. This model, I would suggest, is the most comprehensive general theory of the origins of capitalism that is yet available. It continues to stand up well in comparison with recent theories, including Wallerstein's (1974) historical theory of the capitalist world-system.

Weber himself was primarily concerned with the sensitizing concepts necessary for an interpretation of the unique pattern of history and, in his methodological writings, he disavowed statements in the form of general causal principles (cf. Burger, 1976). Nevertheless, Weber's typologies contain implicit generalizations about the effects of institutional arrangements upon each other, and statements of cause-and-effect abound in his substantive writings. There is nothing to prevent us from stating his historical picture of changing institutional forms in a more abstract and generalized manner than Weber did himself.

---

[2] Cf. the closing words of *The Religion of China:* "To be sure the basic characteristics of the 'mentality,' in this case practical attitudes towards the world, were deeply co-determined by political and economic destinies. Yet, in view of their autonomous laws, one can hardly fail to ascribe to these attitudes effects strongly counteractive to capitalist development" (1951:249), and of *The Religion of India:* "However, for the plebeian strata no ethic of everyday life derived from its rationally formed missionary prophecy. The appearance of such in the Occident, however—above all, in the Near East—with the extensive consequences borne with it, was conditioned by highly particular historical constellations without which, despite differences of natural conditions, development there could easily have taken the course typical of Asia, particularly of India" (1958b:343).

Weber's model continues to offer a more sophisticated basis for a theory of capitalism than any of the rival theories of today. I put forward this formalization of Weber's mature theory, not merely as an appreciation of one of the classic works of the past, but to make clear the high-water mark of sociological theory about capitalism. Weber's last theory is not the last word on the subject of the rise of capitalism, but if we are to surpass it, it is the high point from which we ought to build.

THE COMPONENTS OF RATIONALIZED CAPITALISM

Capitalism, says Weber (1961:207–8, 260) is the provision of human needs by the method of enterprise, which is to say, by private businesses seeking profit. It is exchange carried out for positive gain, rather than forced contributions or traditionally fixed gifts or trades. Like all of Weber's categories, capitalism is an analytical concept; capitalism can be found as part of many historical economies, as far back as ancient Babylon. It became the indispensable form for the provision of everyday wants only in Western Europe around the middle of the nineteenth century. For this large-scale and economically predominant capitalism, the key is the "rational permanent enterprise" characterized by "rational capital accounting."

The concept of "rationality" which appears so often in Weber's works has been the subject of much debate. Marxist critics of capitalism, as well as critics of bureaucracy, have attacked Weber's alleged glorification of these social forms (e.g., Hirst, 1976). On the other hand, Parsons (1947), in his long introduction to the definitional section of *Economy and Society,* gives "rationalization" both an idealist and an evolutionary bent, as the master trend of world history, involving an inevitable upgrading of human cognitive and organizational capacities. Tenbruck (1975) claims the key to Weber's works is an inner logic of rational development found within the realm of religious solutions to the problem of suffering.

It is clear that Weber himself used the term "rationalism" in a number of different senses.[3] But for his *institutional* theory of capitalist development, there is only one sense that need concern us. The "rational capitalistic establishment," says Weber (1961:207), "is one with capital accounting, that is, an establishment which determines its income yielding power by calculation according to the methods of modern bookkeeping and the striking of a balance." The key term is *calculability;* it occurs over and over again in those pages. What is distinctive about modern, large-scale, "rational" capitalism—in contrast to earlier, partial forms—is that it is methodical and predictable, reducing all areas of production and distribution as much as possible to a routine. This is also Weber's criterion for calling bureaucracy the most "rational" form of organization.[4]

---

[3] In Part I of *Economy and Society* (written 1918–20), Weber distinguishes formal and substantive rationality of economic action (1968:85–6). In "The Social Psychology of the World Religions" (written 1913), Weber (1946:293–4) defines three different types of rationalism: (1) a systematic world view based on precise, abstract concepts; (2) practical means-ends calculations; (3) a systematic method, including that of magic or prayer. In *The Protestant Ethic* (1904–5), Weber (1930:76–78) attacks the notion that the spirit of capitalism is "part of the development of rationalism as a whole," and says he is interested in "the origin of precisely the irrational element which lies in this, as in every conception of a calling." Kalberg (1980) points out that under one or another of Weber's types of rationality, *every* action, even the most superstitious, might be called "rational." Kalberg argues that only one type of rationality is relevant for the methodical conduct of affairs.

[4] It is plain that Weber (1968:85–6) is referring to what in *Economy and Society* he calls "formal" rationality, efficiency based on quantitative calculation of means, rather than "substantive" rationality, the adequacy of actions for meeting ultimate values. Such values could be criteria of economic welfare, whether maximal production, quality of life, or a socialist economic distribution, or they could be ethical or religious values. Weber makes it clear that formal and substantive rationality can diverge widely, especially in his late political writings about the dangers of bureaucracy (1946:77–128; 1968:1393–1415). Weber himself tended to defend the formal rationality of modern capitalism as coinciding to a fair degree with substantive rationality in meeting the value of maximizing the economic welfare of the population at large (1968:108–9). It goes

For a capitalist economy to have a high degree of predictability, it must have certain characteristics. The logic of Weber's argument is first to describe these characteristics; then to show the obstacles to them that were prevalent in virtually all societies of world history until recent centuries in the West; and, finally, by the method of comparative analysis, to show the social conditions responsible for their emergence.

According to his argument, the components of "rationalized" capitalism are as follows:

There must be *private appropriation of all the means of production,* and their concentration under the control of entrepreneurs. Land, buildings, machinery, and materials must all be assembled under a common management, so that decisions about their acquisition and use can be calculated with maximal efficiency. All these factors must be subject to sale as private goods on an open market. This development reaches its maximal scope when all such property rights are represented by commercial instruments, especially shares in ownership which are themselves negotiable in a stock market.

Within this enterprise, capital accounting is optimized by a *technology which is "reduced to calculation to the largest possible degree"* (1961:208). It is in this sense that mechanization is most significant for the organization of large-scale capitalism.

Labor must be free to move about to any work in response to conditions of demand. Weber notes that this is a formal and legal freedom, and that it goes along with the economic compulsion of workers to sell their labor on the market. Capitalism is impossible without a propertyless stratum selling its services "under the compulsion of the whip of hunger" (1961:209), for only this completes a mass market system for the factors of production which makes it possible to clearly calculate the costs of products in advance.

Trading in the market must not be limited by irrational restrictions. That is to say, noneconomic restrictions on the movement of goods or of any of the factors of production must be minimized. Such restrictions include class monopolies upon particular items of consumption (such as sumptuary laws regulating dress), or upon ownership or work (such as prohibitions on townspeople owning land, or on knights or peasants carrying on trade; more extensively, caste systems in general). Other obstacles under this heading include transportation difficulties, warfare, and robbery—which make long-distance trading hazardous and unreliable.

Finally, there must be *calculable law, both in adjudication and in public administration.* Laws must be couched in general terms applicable to all persons, and administered in such a way as to make the enforcement of economic contracts and rights highly predictable. Such a legal system is implicated in most of the above characteristics of rational capitalism: the extension of private property rights over the factors of production; the subdivision and easy transferability of such rights through financial instruments and banking operations; formal freedom for laborers; and legally protected markets.

The picture that Weber gives us, then, is of the institutional foundations of the market as viewed by neoclassical economics. He sees the market as providing the maximal amount of calculability for the individual entrepreneur. Goods, labor, and capital flow continuously to the areas of maximal return; at the same time, competition in all markets reduces costs to their minimum. Thus, prices serve to summarize all the necessary information about the optimal allocation of resources for maximizing profit; on this basis, entrepreneurs can most reliably make calculations for long-term production of large amounts of goods. "To sum up," says Weber (1961:209), "it must be possible to conduct the provision for needs exclusively on the basis of market opportunities and the calculation of net income."

It is, of course, the model of the laissez-faire capitalist economy that Weber wishes to ground. At the extreme, this is an unrealistic view of any economy that has ever existed. Weber treats it as an ideal type and, hence, in a fuller exposi-

---

without saying that this is an empirical, not an analytical judgment.

tion would doubtless have been prepared to see it as only partially realized even in the great capitalist takeoff period of the nineteenth century. But it is worth noting that a critique of Weber along these lines could certainly not be a classical Marxian one. The central dynamic of capitalism in Marx's theory, in fact, depends even more immediately than Weber's on the unrestricted competitiveness of the open market for all factors of production (cf. Sweezy, 1942). And Weber and Marx agree in claiming that the initial breakthrough to an industrial society had to occur in the form of capitalism. Thus, although Weber may have a personal bias toward the neoclassical market economy, both as analytical model and as political preference, this would give no grounds for a critique of the adequacy of his explanation of this phase of world history. Even for a later period, Weber is hardly dogmatic. As we shall see, he recognizes the possibility of socialism emerging, once capitalism has matured—although he does not admire the prospect—and he even gives some indications of the forces that might produce it. Like German and Austrian non-Marxist economists of his generation, Weber includes socialism within his analytical scheme.

Weber's model of the modern economy is particularly striking with regard to the concept of the "industrial revolution." For it is not mechanization per se that is the key to the economic transformation, despite the far-reaching consequences of shifts from agrarian to inanimate-energy-based technologies (cf. Lenski, 1966). In Weber's scheme, technology is essentially a dependent variable. The key *economic* characteristic of mechanization is that it is feasible only with mass production (Weber, 1961:129, 247). The costs of even simpler machines such as steam-powered looms would make them worthless without a large-scale consumers' market for cloth, as well as a large-scale producers' market in wool or cotton. Similar considerations apply a fortiori to machinery on the scale of a steel rolling mill. But large-scale production is impossible without a high degree of predictability that markets will exist for the products, and that all the factors of production will be forthcoming at a reasonable cost. Thus, mechanization depends on the prior emergence of all the institutional factors described above.

Weber does not elaborate a systematic theory of technological innovation, but it would be possible to construct one along these lines. He does note that all the crucial inventions of the period of industrial takeoff were the result of deliberate efforts to cheapen the costs of production (1961:225–6, 231). These efforts took place because previous conditions had intensified the capitalist pursuit of profits. The same argument could be made, although Weber did not make it, in regard to the search for methods to improve agricultural production that took place in the seventeenth and eighteenth centuries. The "green revolution" which preceded (and made possible) the industrial revolution was not a process of mechanization (agricultural mechanization took place only in the late nineteenth century) but was, more simply, the application of capitalist methods of cost accounting to hitherto traditional agriculture. Thus, it is the shift to the calculating practices of the capitalist market economy which makes technological innovation itself predictable, rather than, as previously, an accidental factor in economic life (1961:231).[5]

THE CAUSAL CHAIN

What are the social preconditions for the emergence of capitalism as thus described?

Note, first of all, that economic life, even in the most prosperous of agrarian societies, generally lacked most of these traits. Property systems frequently tied land ownership to aristocratic status, while commercial occupations were often prohibited to certain groups and monopolized by others. The labor force was generally unfree—being either slaves or tied to the land as serfs. Technologies of mass production hardly existed. The market was generally limited either to local areas or to long-distance trade in luxuries, due to numerous near-confiscatory tax barriers, unreliable and varying coinage, warfare, robbery, and poor transportation. And legal systems, even in literate states, tended to be characterized by patrimonial or magical-religious procedures, by differential application to different social groups and by different localities, and by the practices of officials seeking private gain. Reliable financial transactions, including the operation of a banking system relatively free from political interference and plundering, were particularly handicapped by these conditions.

---

[5] Weber does mention "rational science and in connection with it a rational technology" (1961:232) as one of the features of the West important for modern capitalism. On the other hand he says: "It is true that most of the inventions of the 18th century were not made in a scientific manner. . . . The connection of industry with modern science, especially the systematic work of the laboratories, beginning with Justus von Liebig [i.e., *Circa* 1830], enabled industry to become what it is today and so brought capitalism to its full development." On the balance, I think science comes out as a secondary factor in the model.

The social preconditions for large-scale capitalism, then, involved the destruction of the obstacles to the free movement or economic transfer of labor, land, and goods. Other preconditions were the creation of the institutional supports for large-scale markets, especially the appropriate systems of property, law, and finance.

These are not the only preconditions of capitalism, but, specifically, Weber is seeking the organizational forms that made capitalism a world-transforming force in the West but not elsewhere. By a series of comparisons, Weber shows that a number of other factors that have been advanced to account for the Western takeoff cannot have been crucial. Against Sombart, he points out that standardized mass production for war cannot have been decisive for, although a good deal of this existed in Europe in the seventeenth century, and thereafter, it also existed in the Mogul Empire and in China without giving an impetus to capitalism (1961:229). Similarly, the enormous expenditures for court luxury found in both Orient and Occident were incapable of generating a mass market (1961:229–30). Against the simpler arguments of Adam Smith, which attribute the industrial division of labor to the extension of trade, Weber points out that trade can be found everywhere, even in the Stone Age. In ancient Babylon, for example, trade was such as to disintegrate "primitive economic fixity" to a considerable degree (1961:232). On the other hand, politically determined agrarian economies show how "specialization takes place without exchange" (1961:103). Nor is the pursuit of profit per se the crucial motive for mass capitalism; the "ruthlessness" and "unscrupulousness" of the traditional foreign trader was incapable of transforming the economy at large (1961:232). Nor can population growth have been the cause of Western capitalism, for the same trend occurred in China without the same result (1961:258–9). Neither, finally, can the price revolution of the sixteenth century, due to the influx of precious metals from the Americas, have been decisive (see the later discussion on Wallerstein).[6]

The features that Weber finds unique to the West constitute a causal chain.[7] I have represented this schematically in Figure 1. The characteristics of rational capitalism itself are the entrepreneurial organization of capital, rational technology, free labor, unrestricted markets, and calculable law. These make up a complex: the markets for goods, labor, and capital all mesh around entrepreneurial property using mass production technology; the operation of all of these factors together creates further pressures to both rationalize technology and expand each factor market—while yet distributing wealth in such a way as to further the demand. The legal system is both an ongoing prop for all of these features and a causal link backward to their

---

[6] Weber (1961:260) also mentions geographical conditions as more favorable to capitalism in Europe than in China or India, due to transportation advantages in the former via the Mediterranean sea and the interconnecting rivers. But he goes on (p. 261) to discount this, in that no capitalism arose in Mediterranean antiquity, when civilization was predominantly coastal, whereas early modern capitalism in Europe was born in the cities of the interior.

[7] Weber does not clearly describe a chain, and sometimes he lumps characteristics of rational capitalism with its preconditions. Although some of these preconditions continue into the operation of modern capitalism, a logical chain of explanation, I believe, requires something like the separation I have given. It should be understood that Weber gives a highly condensed summary in these lectures.

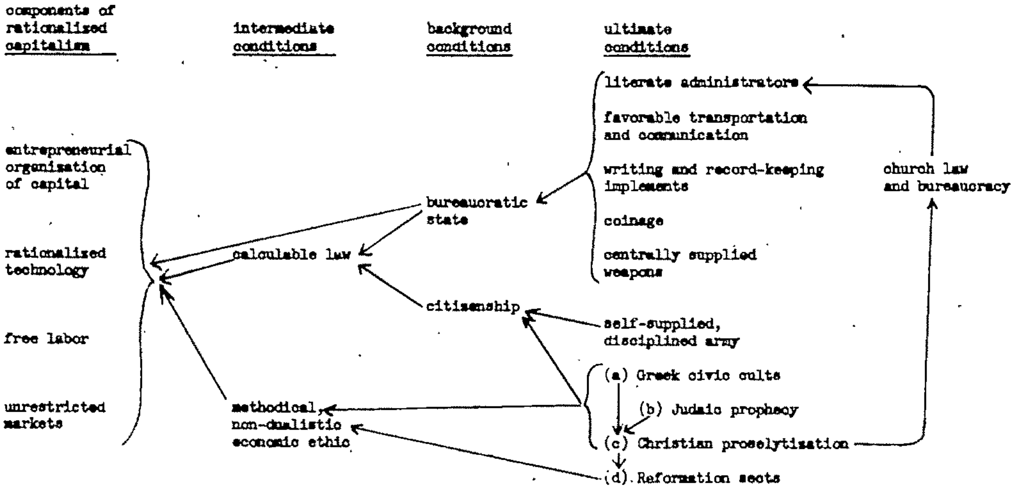

components of rationalized capitalism
intermediate conditions
background conditions
ultimate conditions
literate administrators
favorable transportation and communication
writing and record-keeping implements
coinage
centrally supplied weapons
church law and bureaucracy
entrepreneurial organization of capital
rationalized technology
free labor
unrestricted markets
bureaucratic state
calculable law
citizenship
self-supplied, disciplined army
(a) Greek civic cults
(b) Judaic prophecy
(c) Christian proselytization
(d) Reformation sects
methodical, non-dualistic economic ethic

Figure 1. The Weberian Causal Chain

social preconditions. At this intermediate causal level there is a second crucial factor which, like the law, is essentially cultural, although not in the sense of disembodied ideas, but, rather, in the sense of beliefs expressed in institutionalized behavior. This is the "lifting of the barrier . . . between internal and external ethics" (1961:232).

In virtually all premodern societies there are two sharply divergent sets of ethical beliefs and practices. Within a social group, economic transactions are strictly controlled by rules of fairness, status, and tradition: in tribal societies, by ritualized exchanges with prescribed kin; in India, by rules of caste; in medieval Europe, by required contributions on the manor or to the great church properties. The prohibition on usury reflected this internal ethic, requiring an ethic of charity and the avoidance of calculation of gain from loans within the community (cf. Nelson, 1949).[8] In regard to outsiders, however, economic ethics were at the opposite extreme: cheating, price gouging, and loans at exorbitant interest were the rule. Both forms of ethic were obstacles to rational, large-scale capitalism: the internal ethic because it prevented the commercialization of economic life, the external ethic because it made trading relations too episodic and distrustful. The lifting of this barrier and the overcoming of this ethical dualism were crucial for the development of any extensive capitalism. Only this could make loans available regularly and promote the buying and selling of all services and commodities for moderate gain. Through innumerable daily repetitions, such small (but regular) profits could add up to much more massive economic transactions than could either the custom-bound or the predatory economic ethics of traditional societies.

What, then, produced the calculable legal system of saleable private property and free labor and the universal ethic of the pursuit of moderate economic profit? The next links in the causal chain are political and religious. The bureaucratic state is a crucial background determinant for all legal and institutional underpinnings of capitalism. Moreover, its legal system must be based on a concept of universal citizenship, which requires yet further political preconditions. The religious factor operates both as a direct influence on the creation of an economic ethic and as a final level of causality implicated in the rise of the rational-legal state and of legal citizenship.

The state is the factor most often overlooked in Weber's theory of capitalism. Yet it is the factor to which he gave the most attention; in *Economy and Society,* he devoted eight chapters of 519 pages to it, as opposed to one chapter of 236 pages to religion, with yet another chapter—the neglected but very important chap. XIV of Part II—to the relations between politics and religion. In the *General Economic History,* he gives the state the two penultimate chapters, religion the final chapter. For Weber, this political material was not an extraneous interest but, instead, the key to all of the *institutional* structures of rational capitalism. Only the West developed the highly bureaucratized state, based on specialized professional administrators and on a law made and applied by full-time professional jurists for a populace characterized by rights of citizenship. It is this bureaucratic-legal state that broke down feudalism and patrimonialism, freeing land and labor for the capitalist market. It is this state that pacified large territories, eliminated internal market barriers, standardized taxation and currencies. It is this state that provided the basis for a reliable system of banking, investment, property, and contracts, through a rationally calculable and universally applied system of law courts. One may even argue that the bureaucratic state was the proximate cause of the impulse to rationalization, generally—above all, via the late seventeenth- and eighteenth-century spirit of enlightened absolutism, which set the stage for the industrial revolution.

---

[8] Hence the role of "guest peoples" such as the Jews and the Caursines in Christian Europe, or the Christians in Islamic societies, or the Parsees in India, as groups of tolerated outsiders who were available for making loans, which otherwise would not be forthcoming within the controlled internal economy (1961:267).

There are three causal questions about the rational/legal state. Why did it rise to predominance? Where did its structural characteristics come from? How did its legal system take the special form of conceiving of its subjects as holding the rights of citizenship?

The first question is easily answered. The bureaucratic state rose to predominance because it is the most efficient means of pacifying a large territory. It is effective externally in that it can supply a larger military, with better weapons, than can nonbureaucratic states; and it is effective, internally, as it tends to be relatively safe against disintegration by civil war or political *coup.*[9]

The sources of the bureaucratic state are, to a degree, quite familiar. In the widely reprinted section on bureaucracy from *Economy and Society* (1968:956–1005), Weber outlines the prerequisites: literate administrators, a technology of long-distance transportation and communication, writing and record-keeping materials, monetary coinage. The extent to which these could be put into effect, however, depended on a number of other factors. Geographical conditions such as easy transportation in river valleys, or favorable situations for state-controlled irrigation (1961:237), fostered bureaucratic centralization, as did intense military competition among adjacant heartlands. Types of weapons which are centrally (rather than individually supplied) also favor bureaucratization. If such conditions make central control easy, however, bureaucratization need not proceed very deeply, and the society may be ruled by a thin stratum of officials above a local structure which remains patrimonial. In China, for example, this superficial bureaucratization constituted a long-term obstacle to capitalism, as it froze the economy under the patrimonial control of local clans.

The most thorough bureaucratization, as well as that uniquely favorable to capitalism, is that which incorporates a formalistic legal code based on citizenship. Citizenship meant, first of all, membership in a city; by extension, membership in a state and hence holder of political rights within it. This was an alien concept throughout most of history. In the patrimonial state, political office was a form of private property or personal delegation, and even in most premodern quasi-bureaucratic states the populace at large was only subject to the state, not holders of rights within it. The latter condition arose only in the West. In both Mediterranean antiquity and the European Middle Ages, cities came under the control of brotherhoods of warriors banded together for mutual protection. Such cities had their own laws and courts, administered by the citizens themselves, all of whom stood under it in relation of formal equality. Such citizenship rights remained historically significant after the original civic forms changed or disappeared. The formal rights and legal procedures originally applied only to a local elite, but when cities were incorporated into large-scale bureaucratic states, they provided the basis for a much more widely inclusive system of adjudication. This was the case when Rome, originally one of these military-fraternity cities, became an empire and, again, in the Middle Ages, when cities in alliance with kings lost their independence but contributed their legal structures to the larger states.[10]

---

[9] The main exception is that revolutions can occur after the military breakdown of the state itself due to foreign wars. But historical instances of these have occurred mainly in states which have been only partially bureaucratized. (See Skocpol, 1979.)

Nearing the end of our chain of causality, we ask: What factors enabled this distinctive type of city to arise in the West? Weber gives two conditions: one military, the other religious.

The military condition is that in the West the city consisted of "an organization of those economically competent to bear arms, to equip and train themselves" (1961:237). This was the case in the formative period of the ancient Greek and Italian cities and, again, in the medieval cities with their disciplined infantries fielded by the guilds. In both cases, the money power of the cities bolstered their military power and, hence, democratization and concomitant legal citizenship. In the Orient and in ancient Egypt, on the contrary, the military princes with their armies were older than the cities and, hence, legally independent cities did not arise; Weber attributed this pattern to the impetus to early centralization given by irrigation.

The second condition is that in the East, magical taboos prevented the organization of military alliances among strangers and, hence, did not allow formation of independent cities. In India, for example, the ritual exclusion of castes had this effect. More generally, in Asia and the Middle East, the traditional priests held monopolies over communion with the gods, whereas in Western antiquity it was the officials of the city who themselves performed the rites (1961:238). In the one case, the boundaries of religious communion reinforced preexisting group divisions; in the other, religious boundaries were an explicit political tool by which civic alliances could be established and enlarged. It is at this point that the two main lines of Weber's chain of causality converge.

We have been tracing the causal links behind the emergence of the rational/legal state, which is one of the two great intermediate conditions of the emergence of an open market economy. The other great intermediate condition (noted earlier) is an economic ethic which breaks the barrier between internal and external economies. Now we see that the religious factors that produced the citizenship revolution and those that produced the economic ethic are essentially the same.

Our last question, then, is: What brought about this religious transformation? Weber gives a series of reasons, each intensifying the effects of the last (1961:238). Ethical prophecy within ancient Judaism was important, even though it did not break down ritual barriers between Jews and Gentiles, because it established a tradition of hostility to magic, the main ethos within which barriers flourished. The transformation of Christianity from a Jewish sect into a proselytizing universal religion gave this tradition widespread currency, while the pentacostal spirit of Christian proselytization set aside the ritual barriers among clans and tribes, which still characterized the ancient Hellenistic cities to some degree. The Judeo-Christian innovations are not the whole story, however; the earlier development of Greek religion into the civic cults had already done much to make universalistic legal membership possible.

The religious factors, as we have seen, entwine with political ones, and their influence in the direction of legal citizenship and upon an economic ethic have fluctuated historically. There is no steady nor inevitable trend toward increasing rationalization of these spheres, but Western history does contain a series of episodes which happen to have built up these effects at particular points in time so that, eventually, a whole new economic dynamic was unleashed. On the political side, the Christian cities of the Middle Ages, drawing upon the institutional legacies of the ancient world, were able to establish religiously sworn confraternities which reestablished a legal system based on citizenship. A second political factor was fostered by religion: the Christian church provided the literate administrators, the educational system, and the example of its own bureaucratic organization as bases upon which the bureaucratic states of the West could emerge. And, on the strictly motivational side, the development of European Christianity gave a decisive ethical push toward rationalized capitalism.

---

[10] Contractual forms of feudalism also contributed somewhat to legal citizenship. Weber neglected this in the *General Economic History*, but considered it in *Economy and Society* (1968:1101). The earlier preconditions (military and religious) for contractual feudalism and for independent cities, however, are essentially the same.

Here, at last, we seem to touch base with Weber's original Protestant Ethic thesis. But in the mature Weber, the thesis is greatly transformed. Protestantism is only the last intensification of one of the chains of factors leading to rational capitalism. Moreover, its effect now is conceived to be largely negative, in the sense that it removes one of the last institutional obstacles diverting the motivational impetus of Christianity away from economic rationalization. For, in medieval Christianity, the methodical, disciplined organization of life was epitomized by the monastic communities.[11] Although the monasteries contributed to economic development by rationalizing agriculture and promoting their own industries, Weber generally saw them as obstacles to the full capitalist development of the secular economy. As long as the strongest religious motivation was siphoned off for essentially otherworldly ends, capitalism in general could not take off (1961:267–9). Hence, the Reformation was most significant because it abolished the monasteries. The most advanced section of the economy would, henceforth, be secular. Moreover, the highest ethics of a religious life could no longer be confined to monks but had to apply to ordinary citizens living in the world. Calvinism and the other voluntary sects were the most intense version of this motivation, not because of the idea of Predestination (which no longer receives any mention in Weber's last text) but only because they required a specific religious calling for admission into their ranks, rather than automatic and compulsory membership in the politically more conservative churches. Weber's (1961: 269–70) last word on the subject of Protestantism was simply this:

> The development of the concept of the calling quickly gave to the modern entrepreneur a fabulously clear conscience—and also industrious workers; he gave to his employees as the wages of their ascetic devotion to the calling and of co-operation in his ruthless exploitation of them through capitalism the prospect of eternal salvation, which in an age when ecclesiastical discipline took control of the whole of life to an extent inconceivable to us now, represented a reality quite different from any it has today. The Catholic and Lutheran churches also recognized and practiced ecclesiastical discipline. But in the Protestant ascetic communities admission to the Lord's Supper was conditioned on ethical fitness, which again was identified with business honor, while into the content of one's faith no one inquired. Such a powerful, unconsciously refined organization for the production of capitalistic individuals has never existed in any other church or religion.

WEBER'S GENERAL THEORY OF HISTORY

Is there an overall pattern in Weber's argument? It is not a picture of a linear trend toward ever-increasing rationality. Nor is it an evolutionary model of natural selection, in the sense of random selection of the more advanced forms, accumulating through a series of stages. For Weber's constant theme is that the *pattern of relations among the various factors* is crucial in determining their effect upon economic rationalization. Any one factor occurring by itself tends to have opposite effects, overall, to those which it has in combination with the other factors.

---

[11] Weber did not live to write his planned volume on medieval Christianity. If he had, I believe he would have found that the High Middle Ages were the most significant institutional turning point of all on the road to the capitalist takeoff. His commitment to the vestiges of his Protestantism argument may have kept him from recognizing this earlier. I will deal with this point in a subsequent article, "The Weberian Revolution of the High Middle Ages."

For example, self-supplied military coalitions produce civic organizations and legal systems which are favorable to capitalism. But if the self-armed civic groups are too strong, the result is a series of guild monopolies which stifle capitalism by overcontrolling markets. Cities, on the other hand, have to be balanced by the bureaucratic state. But when the state is too strong by itself, it, too, tends to stifle capitalism. This can happen by bolstering the immobility of labor (as in the case of "the second serfdom" produced in Russia and eastern Europe as absolutist states developed in the seventeenth and eighteenth centuries); or by directly controlling the division of labor by forced contributions instead of allowing a market to develop. In the areas of the world where bureaucratization was relatively easy, as in ancient Egypt or China, or the Byzantine Empire, the unrestrained power of the state stereotyped economic life and did not allow the dynamics of capitalism to unfold.

The same is true of the religious variables. The creation of the great world religions, with their universalism and their specialized priesthoods, was crucial for the possibility of breaking the ritual barriers among localized groups, with all the consequences this might have for subsequent developments. But, in the absence of other factors, this could actually bolster the obstacles to capitalism. This happened in India, where the development of Hinduism fostered the caste system; the universalistic religion set an external seal upon the lineup of particularistic groups that happened to exist at the time. Even in Christianity, where moral prophecy had a much more barrier-breaking and world-transforming effect, the Church (in the period when it was predominant) created another obstacle against its capitalist implications. This was the period of the High Middle Ages in Europe, when monasticism proliferated and, thus, channeled all the energy of religious motivation into a specialized role and away from the economic concerns of ordinary life.[12]

Weber saw the rise of large-scale capitalism, then, as the result of a series of combinations of conditions which had to occur together. This makes world history look like the result of configurations of events so rare as to appear accidental. Weber's position might well be characterized as historicist, in the sense of seeing history as a concatenation of unique events and unrepeatable complexities. Once a crucial conjuncture occurs, its results transform everything else—and not just locally but also in the larger world of competing states. This was true of the great charismatic revelations of the world religions, which shut off China, India, or the West from alternative lines of development as well as determined the ways that states upon these territories would interact with the rest of the world. Similarly, the full-scale capitalist breakthrough itself was a once-only event, radiating outward to transform all other institutions and societies. Hence, the original conditions necessary for the emergence of capitalism were not necessary for its continuation. The original religious ethic could fade, once the calculability of massive economic transactions had become a matter of routine. Hence, late-industrializing states need not follow the route of classic capitalism. In the advanced societies, the skeleton of the economic structure might even be taken over by socialism.

Weber's account of the rise of capitalism, then, is in a sense not a theory at all, in that it is not a set of universal generalizations about economic change. Nevertheless, on a more abstract level, Weber is at least implicitly proposing such a theory. On one level, he may be read as a collection of separate hypotheses about specific processes and their effects.[13] The foregoing caveat about the necessary balance among factors may be incorporated by specifying that the causal variables must operate at a given strength—that is, by turning them into quantitative generalizations specified to a given range of variation.

---

[12] This was also the time when the church took the offensive against incipient capitalism, in the form of pronouncements against usury (Weber, 1968:584–6).

[13] One clearly formulated proposition, for example, is that armies based on coalitions of self-supplied individuals produce citizenship rights. (For a series of such propositions, see Collins, 1975:356–64.)

On a second level, one may say that the fundamental generalizations in Weber's theory of capitalism concern the crucial role of balances and tensions between opposing elements. "All in all," says Weber in a little-known passage (1968:1192–3), "the specific roots of Occidental culture must be sought in the tension and peculiar balance, on the one hand, between office charisma and monasticism, and on the other between the contractual character of the feudal state and the autonomous bureaucratic hierarchy."[14] No one element must predominate if rationalization is to increase. More concretely, since each "element" is composed of real people struggling for precedence, the creation of a calculable, open-market economy depends upon a continuous balance of power among differently organized groups. The formal egalitarianism of the law depends upon balances among competing citizens and among competing jurisdictions. The nondualistic economic ethic of moderated avarice depends upon a compromise between the claims of in-group charity and the vicious circle of out-group rapaciousness.

The capitalist economy depends on this balance. The open-market system is a situation of institutionalized strife. Its essence is struggle, in an expanded version of the Marxian sense, but with the qualification that this could go on continuously, and indeed must, if the system is to survive.[15] Hence, if there is any generalization implicit in Weber's theory applicable to economic history after the initial rise of capitalism, it is this: The possibility for the follower-societies of the non-Western world to acquire the dynamism of industrial capitalism depends on there being a balance among class forces, and among competing political forces and cultural forces as well. In the highly industrialized societies also, the continuation of capitalism depends on continuation of the same conflicts. The victory of any one side would spell the doom of the system. In this respect, as in others, Weber's theory is a conflict theory indeed.

AN ASSESSMENT: WEBER'S CONFRONTATION WITH MARXISM

How valid is Weber's theory? To fully answer this question would require extensive comparative analyses and a good deal of explication of principles on different levels of abstraction. These tasks are beyond the scope of any one paper. What I can present is a confrontation between Weber's theory and the one rival theory of capitalism which claims a comparable degree of historical and theoretical comprehensiveness, Marxism. This is especially appropriate because Weber himself devoted a great deal of attention in the *General Economic History* to the points at which his analysis impinges on Marxist theories.

The book begins and ends on Marxian themes. The first chapter deals with the question of primitive agrarian communism. Characteristically, Weber finds it to be only one variant of primitive agriculture; where it does exist, it is usually the result of fiscal organization imposed from above (1961:21–36). The closing words of the book speak of the threat of working class revolution which appears once capitalism matures and work discipline loses its religious legitimation (1961:270). In between, there are numerous references to Marxism, far more than in any other of Weber's works. His attitude is critically respectful, as in his comment on the Engels-Bebel theory of the origins of the family: "although it is untenable in detail it forms, taken as a whole, a valuable contribution to the solution of the problem. Here again is the old truth exemplified that an ingenious error is more fruitful for science than stupid accuracy." (1961:40)[16]

---

[14] In other words, the main features of the West depend on a tension between the routinization of religious charisma in the church and the participatory communities of monks, and on a tension between the democratizing tendencies of self-supplied armies and the centralized bureaucratic state. These give us Weber's two great intermediate factors, a nondualistic religious ethic and calculable law, respectively.

[15] . . . . the formal rationality of money calculation is dependent on certain quite specific substantive conditions. Those which are of a particular sociological importance for present purposes are the following: (1) Market struggle of economic units which are at least relatively autonomous. Money prices are the product of conflicts of interest and of compromises; they thus result from power constellations. Money is not a mere "voucher for unspecified utilities," which could be altered at will without any fundamental effect on the character of the price system as a struggle of man against man. "Money" is, rather, primarily a weapon in this struggle, and prices are expressions of the struggle; they are instruments of calculation only as estimated quantifications of relative chances in this struggle of interests (Weber, 1968:107–8).

Weber's intellectual maturity coincides with a period of high-level debate in Germany and Austria between Marxian and non-Marxian economists. In the years between 1885 and 1920 appeared Engels's editions of the later volumes of *Capital*, as well as the principal works of Kautsky, Hilferding, and Luxemburg. On the other side, Sombart, Bortkiewitz, and Tugan-Baranowski provided what they considered to be revisions in the spirit of Marxian economics, while Böhm-Bawerk (1898) and Schumpeter (1954) launched explicit efforts to shore up the weaknesses of neoclassical economics vis-à-vis Marxism, and attacked the technical weaknesses of Marxian theory.[17] This period was in many ways the high-water mark in political economy for an atmosphere of balanced debate is beneficial for intellectual advance. Weber in particular was concerned to meet the Marxian challenge on its own grounds, leaving out nothing that must be conceded, but also turning up whatever factors the Marxists left out. Moreover, the German Marxists had suddenly become stronger with the end of the World War and the downfall of the German monarchy. Weber delivered his lectures in Munich just after the short-lived Communist commune of 1919, and his lecture room contained many radical students. It is not surprising that Weber was so much more explicitly concerned with Marxism in his last work than in the religious studies he published while the war was going on.

Weber had one great advantage over the Marxists. The discipline of historical scholarship reached its maturity around the end of the nineteenth century. Not only had political and military history reached a high degree of comprehensiveness and accuracy, but so had the history of law, religion, and economic institutions not only for Europe and the ancient Mediterranean but for the Orient as well. The historical researches of the twentieth century have not brought to light any great body of facts about the past that has radically changed our view of world history since Weber's day. Weber was perhaps the first great master of the major institutional facts of world history. By contrast, Marx, pursuing his assiduous researches in the 1840s and 50s, had much narrower materials at his disposal (Hobsbawm 1964:20–7). The histories of India, China, Japan, or Islam had scarcely begun to be available; the permeation of the ancient Greco-Roman world by religious institutions was only beginning to be analyzed; and the complex civilization of the European High Middle Ages was hidden beneath what Marx considered the "feudal rubbish" of the *Ancien Regime* of the eighteenth century. Marx wrote before the great coming-of-age of historical scholarship; Weber, just as it reached its peak. Weber thus represents for us the first and in many ways still the only effort to make a truly informed comparative analysis of major historical developments.

It should be borne in mind that Marx and most of his followers have devoted their attention primarily to showing the dynamics of capitalism, not to the preconditions for its emergence. Weber's concerns were almost entirely the reverse. Hence, it is possible that the two analyses could be complementary, Marx's taking up where Weber's leaves off. Only in the 1970s have there been efforts comparable to Weber's from within the Marxian tradition, notably that of Wallerstein (1974). Interestingly enough, Weber anticipated Wallerstein's major points in the *General Economic History*. On the other side, Wallerstein's revision of Marxism is in many ways a movement toward a more Weberian mode of analysis, stressing the importance of external relations among states.

---

[16] Weber goes on to say, "A criticism of the theory leads to consideration first of the evolution of prostitution, in which connection, it goes without saying, no ethical evaluation is involved." There follows (1961:40–53) a brilliant outline of a theory of the organization of the family as one set of variants on sexual property relations, in which material transactions and appropriations are fundamentally involved. Later versions of this line of theory are found in Levi-Strauss (1968), and in Collins (1975:228–59).

[17] Thus, Böhm-Bawerk (1898) and Schumpeter (1954) developed a previously missing link in classical and neoclassical economics, a theory of capitalist profits. This they based on time-lags in the competitive process and resulting time-preference among investment returns, displacing the Marxian theory of profit based on the exploitation of labor. Böhm-Bawerk also made an analysis of socialist economies. He regarded these as possible *politically* (as did Schumpeter and Weber), but denied that production would be organized differently than in capitalism. Socialism could affect only the distribution of capitalist profits among the populace. For the economic thought of this period, see Schumpeter (1954:800–20, 843–55, 877–85) and Sweezy (1942:190–213).

The classical Marxian model of the preconditions for capitalism covers only a few points (Marx, 1967: I, 336–70, 713–64; II, 323–37, 593–613; 1973: 459–514). Some of these are a subset of Weber's model, while two of them are distinctive to Marx. Weber and Marx both stressed that capitalism requires a pool of formally free but economically propertyless labor; the sale of all factors of production on the market; and the concentration of all factors in the hands of capitalist entrepreneurs. Marx did not see the importance of the *calculable* aspect of technology; at times, he seemed to make the sheer productive power of technology the central moving force in economic changes, while at others, he downplayed this as part of a larger economic system—much in the way Weber did. Unlike Weber, Marx gave no causal importance at all to calculable law, nor did he see the earlier links in Weber's causal chain: economic ethics, citizenship, bureaucratization, and their antecedents.[18]

The uniqueness of Marx's discussion is in two factors: primitive accumulation, and revolution. About the latter, Marx had surprisingly little to say beyond the dramatic imagery of revolution breaking the bonds imposed by the property system upon the growing engines of production (Marx, 1959: 43–4). Primitive accumulation takes up nearly the whole of his historical discussion. It means the accumulation of enough raw materials, tools, and food for laborers to live on before subsequent production was completed; hence, it is the quantitative prerequisite for any takeoff into expanded economic production. Such accumulation took place historically in two ways. One was by the expropriation of peasants from their land, which simultaneously concentrated wealth in the hands of the capitalists who received the lands and required the expropriated masses to sell their labor on the market. The other means of primitive accumulation was by usury and merchants' capital. Marx downplayed the importance of monetary factors by themselves, as they operated only in the realm of circulation and did nothing to productive relations; but he did assert that the growth of money capital furthered the dissolution of the feudal economy once it was already under way (1967:III, 596–7).

Of these two factors, Weber says almost nothing explicitly about primitive accumulation. However, the entire earlier sections of the *General Economic History* (1961:21–203) deal with the various forms of appropriation of material and financial means, which have made up, among other things, the capitalism that has been omnipresent throughout history, although not in a rationalized form. The idea that there must be a specific accumulation of surplus for the purpose of a capitalist takeoff, I suspect, is one that Weber would reject. The assumption ought to be subjected to proof. After all, agrarian societies already have the most extreme concentration of wealth at the top of the social hierarchy of any type of society in world history (Lenski, 1966); the industrial takeoff need only have been fueled by a shift in the use of this wealth, not by a further extraction process. As Weber understood, and as subsequent research has shown, capitalists do not have to rise "from below," having amassed their own wealth; it has been far more typical for the aristocracy themselves to go into capitalist production (Stone, 1965; Moore, 1966).[19]

---

[18] Marx (1973:459–514) gave a very general outline of early forms of property as based on family and tribal membership, and he recognized that the ancient cities were military coalitions. He missed the central organizing role of religion in these developments, and failed to see the crucial effect of the revolutions within the ancient cities upon the uniquely Western legal tradition. For Marx, the rise of cities simply meant the growing separation of town and country, an instance of dialectical antithesis, and of the progress of the division of labor (1967:I, 352). For the period immediately preceding the capitalist takeoff, Marx noted that the state had hastened the transition from feudalism to capitalism by creating public finance and conquering foreign markets. These effects Marx subsumed under his concept of "primitive accumulation."

Weber is somewhat more sympathetic to the importance of revolutions. Perhaps the final conditions for the capitalist takeoff in England were the revolutions of 1640 and 1688. These put the state under the control of political groups favorable to capitalism, thus fulfilling the condition of keeping markets and finances free of "irrational" and predatory state policies. Of more fundamental institutional consequence were the revolutions within the cities of ancient Greece and of medieval Italy. The latter, Weber lists among "the five great revolutions that decided the destiny of the occident" (1951:62).[20] For it was the uprising of the plebeians which replaced the charismatic law of the older patrician class with the universalistic and "rationally instituted" law upon which so much of the institutional development of capitalism was to depend (Weber, 1968:1312–3, 1325). In effect, this was a revolution in a system of property, but not in the gross sense of a replacement of one form of appropriation with another. For Weber, a system of property is a complex of daily actions—above all, the making of transfers and contracts and the adjudication of disputes. Hence, political revolutions are most crucial where they set the pattern for ongoing legal actions in a highly calculable form, with all the consequences noted above.

Wallerstein's (1974) theory, as developed in volume I, emphasizes two conditions in the origins of capitalism. One is the influx of bullion from the European colonies, which caused the price inflation of the 16th century. During this period, wages remained approximately constant. The gap between prices and wages constituted a vast extraction of surplus which could be invested in expanding capitalist enterprises (Wallerstein, 1974:77–84).[21] This is Wallerstein's version of the primitive accumulation factor.

Wallerstein's (1974:348) second condition also emerges from the international situation. "[C]apitalism as an economic system is based on the fact that economic factors operate within an arena larger than that which any political entity can totally control. This gives capitalists a freedom of maneuver that is structurally based." He (1974:355) goes on to say that the different states must be of different strengths, so that not all states "would be in the position of blocking the effective operation of transnational economic entities whose locus were in another state." Capitalists in effect must have opportunities to shift their grounds among varied political climates to wherever the situation is most favorable.

Weber (1961:259) was generally aware of both conditions. Regarding the effects of gold and silver influx, however, he was largely unfavorable.

> It is certainly true that in a given situation an increase in the supply of precious metals may give rise to price revolutions, such as that which took place after 1530 in Europe, and when other favorable conditions are present, as when a certain form of labor organization is in the process of development, the progress may be stimulated by the fact that large stocks of cash come into the hands of certain groups. But the case of India proves that such an importation of metal will not alone bring about capitalism. In India in the period of the Roman power, an enormous mass of precious metal—some twenty-five million *sestertii* annually—came in exchange for domestic goods, but this inflow gave rise to commercial capitalism only to a slight extent. The greater part of this precious metal disappeared into the hoards of the rajahs in-

---

[19] Weber also anticipated Barrington Moore's (1966) theory of the political consequences of different property modes in the commercialization of agriculture (1961:81–94).

[20] The others were "the Netherland revolution of the sixteenth century, the English revolution of the seventeenth century, and the American and French revolutions of the eighteenth century."

[21] To this, Wallerstein adds the argument that surplus is further extracted by coerced labor on the periphery, to be consumed in the core, where however (somewhat contrary to the point about the price revolution) labor is well enough paid to constitute a potential consumers' market for capitalist production.

> stead of being converted into cash and applied in the establishment of enterprises of a rational capitalistic character. This fact proves that it depends entirely upon the nature of the labor system what tendency will result from an inflow of precious metal.

In another passage, Weber (1961:231) does say that the price revolution of the sixteenth and seventeenth centuries "provided a powerful lever for the specifically capitalistic tendencies of seeking profit through cheapening production and lowering the price." This came about for industrial (but not agricultural) products, because the quickened economic tempo put on pressures toward further rationalizing economic relations and inventing cheaper technologies of production. Weber thus gives the influx of precious metals a place as a contributory factor, though apparently not an indispensable one, *within* the framework of economic institutions which had already appeared in Europe at the time.[22]

Weber (1961:249) largely agrees, however, with Wallerstein's argument about the international character of capitalism. Modern cities, he points out,

> came under the power of competing national states in a condition of perpetual struggle for power in peace or war. This competitive struggle created the largest opportunities for modern Western capitalism. The separate states had to compete for mobile capital, which dictated to them the conditions under which it would assist them to power. Out of this alliance of the state with capital, dictated by necessity, arose the national citizen class, the bourgeoisie in the modern sense of the word. Hence it is the closed national state which afforded to capitalism its chance for development—and as long as the national state does not give place to a world empire capitalism will also endure.

Here the coincidence with Wallerstein is remarkable. Weber does not emphasize the contours of Wallerstein's world system, with its tiers of core, semiperiphery, and periphery, but Weber does show the central importance of mobile capital among militarily competing states, and he gives a more specific analysis than Wallerstein of the mechanism by which this is transformed into an advantage for capitalism.

In general, there is considerable convergence, as well as complementarity, between Weber's last theory of the origins of capitalism, and the mature Marxian theory which is only now emerging. Weber largely rejects Marxian theories of primitive accumulation, or at least relegates them to minor factors. On the other side, Wallerstein, as well as modern Marxism in general, has moved the state into the center of the analysis. Weber had already gone much further in that direction, so that the main Weberian criticism of the Marxian tradition, even in its present form, is that it does not yet recognize the set of institutional forms, especially as grounded in the legal system, upon which capitalism has rested.

For Weber, the state and the legal system are by no means a superstructure of ideas determining the material organization of society. Rather, his theory of the development of the state is to a considerable extent an analogy to the Marxian theory of the economy. The key factor is the form of appropriation of the material conditions of domination. We have seen the significance of the organization of weapons for Weber's chain of causes of capitalism. In this connection, Weber (1961:237) remarks:

> Whether the military organization is based on the principle of self-equipment or on that of military equipment by an overlord who furnishes horses, arms and provisions, is a distinction quite as fundamental for social history as the question whether the means of economic production are the property of the

---

[22] Weber's (1961:223) comment on the economic benefits of the colonies is even more negative.

> This accumulation of wealth brought about through colonial trade has been of little significance for the development of modern capitalism—a fact which must be emphasized in opposition to Werner Sombart. It is true that the colonial trade made possible the accumulation of wealth to an enormous extent, but this did not further the specifically occidental form of the organization of labor, since colonial trade itself rested on the principle of exploitation and not that of securing an income through market operations. Furthermore, we know that in Bengal for example, the English garrison cost five times as much as the money value of all goods carried thither. It follows that the markets for domestic industry furnished by the colonies under the conditions of the time were relatively unimportant, and that the main profit was derived from the transport business.

> worker or of a capitalistic entrepreneur . . . [T]he army equipped by the war lord, and the separation of the soldier from the paraphernalia of war, [is] in a way analogous to the separation of the worker from the means of production. . . ."

Similarly, state bureaucracy depends upon a set of material conditions, and upon the separation of the administrator from treating the office and its incomes as private property (1968:980–3). Weber diverges from the Marxian analogy by being a more thoroughgoing conflict theorist. As we have seen, and as the quotation given above on the international basis of capitalism bears out, for Weber the conditions of rationalized organization, in political and economic spheres alike, depend upon a continuous open struggle.[23]

The main disagreements between Marx and Weber have less to do with the origins of capitalism than with its future. Weber thought that capitalism could endure indefinitely as an economic system, although political factors could bring it down. As we have seen, he thought that the disappearance of religious legitimation in mature capitalism opened the way for workers to express their discontents in the form of a political movement for socialism. Ironically, it is the rationalized world view promoted by the underlying conditions of capitalism that gave birth to rational socialism, a doctrine that proclaims that the social order itself, rather than the gods, is to blame for economic distress; and that having been deliberately instituted, that order is capable of being consciously changed (1961:217–8). For Weber, however, economic crises may be endemic to modern capitalism, but they are not caused by a fundamental contradiction in it, nor is there any necessary tendency for them to worsen toward an ultimate breakdown. He attributes crises to overspeculation and the resulting overproduction of producers' (but not consumers') goods (1961:217). To decide who is right on these points requires further consideration than can be given here.

CONCLUSION

Weber's last theory is still today the only comprehensive theory of the origins of capitalism. It is virtually alone in accounting for the emergence of the full range of institutional and motivational conditions for large-scale, world-transforming capitalism. Even so, it is incomplete. It needs to be supplemented by a theory of the operation of mature capitalism, and of its possible demise. And even on the home territory of Weber's theory, there remain to be carried out the comprehensive tests that would provide adequate proof. But sociological science, like any other, advances by successive approximations. The theory expressed in Weber's *General Economic History* constitutes a base line from which subsequent investigations should depart.

REFERENCES

Böhm-Bawerk, Eugen von
1898 Karl Marx and the Close of his System. London: T.F. Unwin.
Burger, Thomas
1976 Max Weber's Theory of Concept Formation. Durham, North Carolina: Duke University Press.
Cohen, Jere
1980 "Rational capitalism in Renaissance Italy." American Journal of Sociology 85:1340–55.
Cohen, Jere, Lawrence E. Hazelrigg, and Whitney Pope
1975 "De-Parsonizing Weber: a critique of Parsons' interpretation of Weber's sociology." American Sociological Review 40:229–41.
Collins, Randall
1975 Conflict Sociology: Toward an Explanatory Science. New York: Academic.
Hirst, Paul Q.
1976 Evolution and Social Categories. London: Allen and Unwin.
Hobsbawn, E. J.
1964 "Introduction." Pp. 9–65 in Karl Marx, Pre-Capitalist Economic Formations. New York: International Publishers.
Kalberg, Stephen
1979 "The search for thematic orientations in a fragmented oeuvre: the discussion of Max Weber in recent German sociological literature." Sociology 13:127–39.
1980 "Max Weber's types of rationality: cornerstones for the analysis of rationalization

---

[23] It is true that Weber continues to leave more room for religious conditions than any of the Marxians. Yet even here, military conditions play a key role in the ultimate determinants of religions. The earliest Greek civic cults were war coalitions; and the this-worldly, antimagical character of Judaism derives from the cult of Jahweh, the war god of the coalition of Jewish tribes.

processes in his history." American Journal of Sociology 85:1145–79.

Lenski, Gerhard E.
1966 Power and Privilege. New York: McGraw-Hill.

Levi-Strauss, Claude
1968 The Elementary Forms of Kinship. Boston:
(1949) Beacon Press.

Marx, Karl
1959 Preface to A Contribution to the Critique of
(1856) Political Economy. In L. Feuer (ed.), Marx and Engels: Basic Writings on Politics and Philosophy. New York: Doubleday.
1967 (1867, 1885, 1894) Capital. New York: International Publishers.
1973 (1857–1858) Grundrisse. New York: Random House.

McClelland, David C.
1961 The Achieving Society. Princeton: Van Nostrand.

Moore, Barrington
1966 Social Origins of Dictatorship and Democracy. Boston: Beacon Press.

Nelson, Benjamin
1949 The Idea of Usury. Princeton: Princeton University Press.

Parsons, Talcott
1947 "Introduction." In Max Weber, The Theory of Social and Economic Organization. New York: Oxford University Press.
1963 "Introduction." In Max Weber, The Sociology of Religion. Boston: Beacon Press.
1967 Societies: Comparative and Evolutionary Perspectives. Englewood Cliffs: Prentice-Hall.

Samuelsson, Kurt
1961 Religion and Economic Action. New York: Basic Books.

Schumpeter, Joseph A.
1954 A History of Economic Analysis. New York: Oxford University Press.

Seidman, Steven
1980 Enlightenment and Reaction: Aspects of the Enlightenment Origins of Marxism and Sociology. Unpublished Ph.D. dissertation, University of Virginia.

Skocpol, Theda
1979 States and Social Revolutions. New York: Cambridge University Press

Stone, Lawrence
1965 The Crisis of the Aristocracy. New York: Oxford University Press.

Sweezy, Paul M.
1942 The Theory of Capitalist Development. New York: Oxford University Press.

Tawney, R. H.
1938 Religion and the Rise of Capitalism. Harmondsworth: Penguin.

Tenbruck, F. H.
1975 "Das werk Max Webers." Koelner Zeitschrift fuer Soziologie und Sozialpsychologie 27:663–702.

Wallerstein, Immanuel
1974 The Modern World System. New York: Academic.

Weber, Max
1930 (1904–1905) The Protestant Ethic and the Spirit of Capitalism. Translated by Talcott Parsons. New York: Scribner's.
1946 (1922) From Max Weber: Essays in Sociology. Translated by Hans H. Gerth and C. Wright Mills. New York: Oxford University Press.
1947 (1922) The Theory of Social and Economic Organization. Translated by A. M. Henderson and Talcott Parsons. New York: Oxford University Press.
1949 (1904, 1906, 1917–1918) The Methodology of the Social Sciences. Translated by Edward A. Shils and Henry A. Finch. Glencoe, Ill.: Free Press.
1951 (1916) The Religion of China. Translated by Hans H. Gerth. Glencoe, Ill.: Free Press.
1952 (1917–1919) Ancient Judaism. Translated by Hans H. Gerth and Don Martindale. Glencoe, Ill.: Free Press.
1954 (1922) Max Weber on Law in Economy and Society. Translated by Edward Shils and Max Rheinstein. Cambridge, Mass.: Harvard University Press.
1958a (1922) The City. Translated by Don Martindale and Gertrud Neuworth. Glencoe, Ill.: The Free Press.
1958b (1916–1917) The Religion of India. Translated by Hans H. Gerth and Don Martindale. Glencoe, Ill.: Free Press.
1961 (1923) General Economic History. Translated by Frank H. Knight. New York: Collier Books.
1963 (1922) The Sociology of Religion. Translated by Ephraim Fischoff. Boston: Beacon Press.
1968 (1922) Economy and Society. Edited by Guenther Roth and Claus Wittich. New York: Bedminster Press.

IMMIGRATION AND THE EXPANSION OF SCHOOLING IN THE UNITED STATES, 1890–1970*

JOHN H. RALPH
University of Delaware

RICHARD RUBINSON
Johns Hopkins University

American Sociological Review 1980, Vol. 45 (December):943–954

Our research analyzes the effects of immigration on the rate of growth of educational expansion in the United States from 1890 to 1970. Previous studies have shown that the presence of nativist, Protestant-millenial groups was associated with the early expansion of public primary enrollments. Immigrant groups provide an important contrast to such groups, and several interpretive histories of United States education stress the relationship between immigration and schooling. Using aggregate, time-series analyses, we find that: (1) immigration decreased the rate of growth of public primary and secondary enrollments between 1890 and 1924; (2) immigration increased the rate of growth of private primary and secondary enrollments during this period; (3) immigration increased the rate of growth of public primary and secondary enrollments between 1925 and 1970; and (4) immigration decreased the rate of growth of private primary and secondary enrollments during the period. These findings show that the effect of immigration on the aggregate rate of growth of schooling has varied as a function of the character of the immigrants themselves.

Formally organized education has become such a central institution for both national and individual development that we often assume that the explanation for its tremendous growth is not problematic. Government supports education because of its assumed importance to national goals, and individuals demand education because of its assumed importance to personal mobility. Educational expansion in today's world seems to be an almost self-generating process.

For example, an extensive cross-national study of educational expansion by Meyer et al. (1977) found that the rapid worldwide growth of enrollments from 1950 to 1970 could be explained primarily as a function of the available school-age population to be educated and by the level of enrollments in 1950. This study concludes that education has become of such importance that it now expands independently of the constraints and stimuli that economic, political, and social structures must have provided in previous times.

The United States is certainly one country where education is seen as fundamental to national purposes and where individuals and groups constantly demand more education. But if this characterization is true today, this has not always been the case. For during most of the period of educational expansion in the United States, the links between education and national development and between education and mobility were not assumed. In order to understand the growth of educational enrollments, then, we must analyze the long-term historical conditions that created the presently high levels of education. The purpose of this paper is to study one social factor that has often been suggested as important to educational expansion in the United States, the role of immigration. We want to investigate how the influx of immigration has affected the growth of educational enrollments.

While there are many interpretations of the rise of education in the United States, there are few systematic, long-term national studies. A recent study by Meyer et al. (1979) shows that it is important to separate the formalization of schooling—the rise of bureaucratic,

---

* Direct all communications to: John H. Ralph; Department of Educational Studies; University of Delaware; Newark, DE 19711.

An earlier version of this paper was presented at the annual meeting of the Society for the Study of Social Problems in San Francisco, August, 1978. We thank Mead Cain, James Crouse, Charles B. Keely, John Meyer, and Ludwig-Mosberg for critical readings of earlier versions. We also thank W. Vance Grant for making data available to us.

state-funded, and professionally managed education—from the creation and spread of enrollments. Using state-level data in a series of seven cross-sectional analyses every ten years from 1870 to 1930, the study finds that indicators of urban industrialization have consistently large effects on two measures of educational formalization, the average expenditures per pupil and the length of the school term. But these factors do not affect the measure of the spread of education, the level of public primary-school enrollments. Rather, the analyses show that two indicators of what they refer to as nation-building social movements, a measure of Republican party strength and a measure of evangelical Protestantism, increase the level of public primary-school enrollments. Their argument is that the expansion of schooling reflected the widespread ideology of the traditional Protestant-millenial groups in the United States. Local public education was a mechanism for organizing local community. Their study finds that the greater the presence in a state of such groups, the higher the total level of public primary enrollments. They argue that not only was it the children of these groups who went to school but that the greater the presence of these groups, the greater the total level of enrollment.

If the presence of such traditional groups as these had the consequence of increasing educational levels, then it is also necessary to study how the presence of other major groups affected the growth of enrollments. In the United States, immigrant groups provide an important contrast, for such groups were often viewed as fundamentally opposite to the traditional, nativist groups (Higham, 1974).

The assimilation of the immigrant through schooling has been a prominent concern in the interpretive histories of public education (Bowles and Gintis, 1976; Butts, 1978; Everhart, 1977; Greer, 1972). Recent revisionist histories of education suggest that public schooling was imposed on immigrant groups to acculturate them to the dominant economic, political, and social patterns of the country. Such studies simultaneously emphasize the resistance by these groups to attempts at imposition (Collins, 1976).

Census data reported in Table 1 show that as early as 1900 the rates of school enrollment for children aged six to sixteen were remarkably similar for both the children of immigrants and of native-born fathers. In addition, despite the concerns at that time over the problems of educating immigrants, data from this period summarized in Table 2 show that rates of school retardation were almost the same for the children of native-born and of foreign-born fathers, and even for foreign-born students. Thus, *rates* of attendance and of retardation were similar for the children of immigrants and of native-born fathers.

But to know that the rates of attendance were similar for these groups does not tell us what the consequences of immigration were for the general rate of educational enrollment. The influx of immigration may have caused an overall increase in the rate of growth of school enrollments if it initiated a status competition process in which other groups increased their rates of school enrollment to preserve their status distinctions over immigrants (Collins, 1971). Immigration may also have increased rates of school enrollments for everyone if the perceived threat posed by immigration to dominant cultural values caused local authorities to expand schooling as a mechanism of cultural solidarity. Immigration may also have decreased the rate of growth of *public* schooling and increased the rate of growth of *private* schooling either because immigrants chose or were subtly channeled to private schools, or because nonimmigrant groups sought to protect their status through private schooling. The effects of immigration on the rate of schooling involved social forces that may have altered

Table 1. Percent of Children 6 and under 16 at School by Nativity

Parentage Subgroup	% at School
Native-born Father	84.6
Foreign-born Father	84.2
Foreign-born Student	83.3

SOURCE: Twelfth Census of the United States, 1900, Vol. I, p. 763.

Table 2. Retardation Percentages by Parentage: 1910

Subgroup	% Retarded*
Native-born Father	25
Foreign-born Father	26
Foreign-born Student	26

SOURCE: Retardation—U.S. Immigration Commission, 1911.

* This calculation is based on the number of pupils sampled in the subgroup who are retarded (two years older than the average) *and* who also have been in school more years than their grade in school. This calculation controls for students who begin school late but are not "slow learners."

behaviors and expectations for a large number of groups. To answer the question of how immigration affected the growth of schooling, we need to study the aggregate relationship between immigration and enrollment rates. The aggregate, national analyses we present here cannot tell us why immigration affected the growth of schooling, but they can tell us if it did and how it affected that growth. Such analyses as these can also point to fruitful directions for other studies and, combined with more micro studies, can lead toward the development of an explanation for the growth of schooling in the United States.

Immigration, Public Policy, and Popular Reaction

The amount and character of international migration to the United States has always been subject to political policy. The United States initially had an open immigration policy, and after the Civil War the immigrant population rapidly increased. The number of immigrants declined during the depression of the 1890s but rose rapidly until World War I. During the war immigration declined, but public concern over the prospect of large-scale immigration in the 1920s led to the National Origins Quota Acts. Until 1921, the federal government had only restricted the immigration of Orientals and "defective individuals" such as prostitutes and criminals. By 1924, a quota system based on the individual's national origin was established, shifting the character of immigration from those coming from eastern, southern and central Europe to those from northern and eastern Europe.

Table 3 shows the changes in immigration by European regions. Until 1890, immigrants from northern and western Europe were the overwhelmingly predominant groups. The peak of this immigration occurred from 1881 to 1890 after 30 years in which this immigration exceeded 2,000,000 per decade. This amount was 100 times greater than the number of immigrants from eastern, central, and southern Europe. Between 1901 and 1910, the situation changed dramatically. During this decade the number of immigrants from eastern, central, and southern Europe was twice as large as the number from northern and eastern Europe. Furthermore, the total number of immigrants during the decade was more than double the level of the previous decade. Figure 1 shows the variation in total number of immigrants by year. After 1924 there was a decline in both the magnitude and variation of immigration. It is important for our analyses of schooling to highlight this change.

It was probably not the number of immigrants that provoked widespread public concern, but the differences in the social characteristics of these new immigrants. Professor Cubberly, a noted educational spokesman at Stanford, argued that the new immigrant was different and needed to be Americanized (Cubberly, 1909:15–6). These new immigrants were different. They came from different countries, their native languages were unfamiliar, and they were predominantly Catholic. The public clamor over these groups was a reaction to these social characteristics, but also a reaction to political changes. During this period, labor unions grew from 447,000 in 1897 to over two million by 1904 (U.S. Census, 1976). Labor strikes were becoming more frequent. In 1911 socialist party candidates were elected mayors in 73 municipalities, and socialist ideas became prominent in unions and local political groups. The political strength of socialism at the ballot box peaked in 1918, but strong anticapitalist sentiment and support for mass strikes lasted through the 1920s (Weinstein, 1968). In the public mind, radical politics and recent immigration were closely linked (Higham, 1974).

Table 3. European Immigration by Region: 1819 to 1954

			Regions			
Period	Total[1] Immigration	All Europe	North and West	East and Central	South	Other
Total, 1819 to 1954	40,173	33,764	19,457	8,549	5,715	43
1819 to 1820[2]	8	8	8	—	—	—
1821 to 1830[2]	143	99	96	—	3	—
1831 to 1840[3]	599	496	490	—	5	—
1841 to 1850[4]	1,713	1,598	1,592	1	5	—
1851 to 1860[4]	2,598	2,453	2,431	2	20	—
1861 to 1870[5]	2,315	2,065	2,032	12	21	—
1871 to 1880	2,812	2,272	2,070	126	75	1
1881 to 1890	5,247	4,737	3,779	627	331	1
1891 to 1900	3,688	3,559	1,643	1,211	704	—
1901 to 1910	8,795	8,136	1,910	3,915	2,310	1
1911 to 1920	5,736	4,377	998	1,918	1,452	8
1921 to 1930	4,107	2,478	1,300	590	565	23
1931 to 1940	528	348	205	58	84	2
1941 to 1950	1,035	622	492	48	77	4
1951 to 1954	850	518	411	41	63	3

SOURCE: Taeuber (1965).

NOTE: [In thousands. Data are for years ending June 30, except as noted. 1819 to 1867, figures represent alien passengers arriving in steerage; 1868 to 1897, immigrant aliens arriving; 1892 to 1894 and 1898 to 1954, immigrant aliens admitted; 1819 to 1868, by nationality; 1869 to 1898, by country of origin or nationality; 1899 to 1954, by country of last permanent address.]

[1] Includes some immigrants of unspecified origins.

[2] October 1 to September 30.

[3] October 1, 1830, to December 1, 1840.

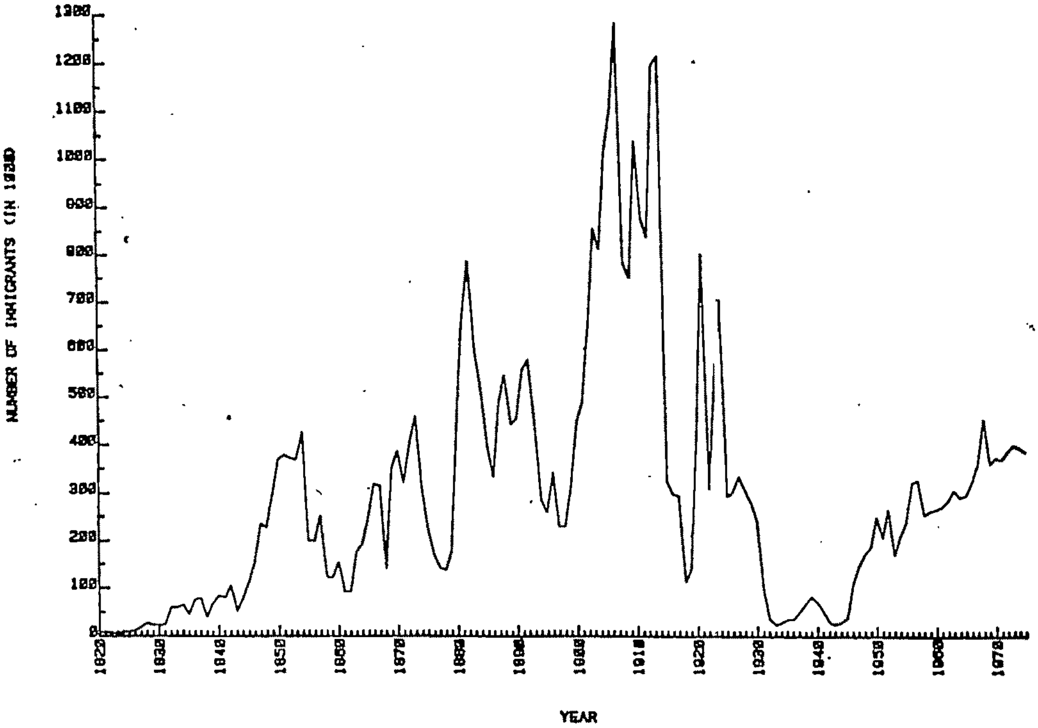
[4] Calendar years.

[5] January 1, 1861 to June 30, 1870.

Civic leaders of this period proclaimed their faith in the ability of schools to indoctrinate consensual values and desirable attitudes in the new groups. Good citizenship, they argued, could overcome class politics, and the public school system was to be the agent through which immigrant children would learn American ways (Roberts, 1912). Schooling and the character of immigration became linked during this period.

Revisionist educational histories emphasize the struggle over the imposition of schooling. Bowles and Gintis (1976:175) argue that immigrants did not oppose education, but they did oppose education they could not control. These issues have direct consequences for the relationship between immigration and schooling. During the Progressive Era, the influx of immigration stimulated political, economic, and cultural elites to strengthen public education as an agent of assimilation and stratification. As a consequence, the influx of immigration and the reaction it provoked may have led to an expansion of the overall rate of schooling. But immigrant groups also resisted the imposition of public schooling. Historically, the public schools had been stamped with the character of Protestant values and outlook (Tyack, 1970; Meyer et al., 1979), and one of the prominent features of these new immigrants was their Catholic heritage. After 1900, Greek Orthodox, Roman Catholic, and Lutheran schools flourished in most metropolitan areas (Ravitch, 1978). And Smith (1967) has documented the desire of immigrants to initiate their own educational programs. These considerations imply that the influx of this immigration may have decreased the rate of public school growth and increased the rate of private school growth.

The empirical analyses we now present investigate the relationship between the influx of immigration and the growth of schooling in the United States from 1890 to 1970. These aggregate, national analyses do not distinguish among the several reasons why the rate of schooling was affected by immigration, but they can show us if it was, and how. These analyses investigate the relationships



Source: U.S. Bureau of the Census, Historical Statistics of the United States, 1976.
Figure. 1. Immigration, 1820–1970 (in thousands)

between immigration and the growth of primary and secondary school enrollments, both public and private, distinguishing between the periods 1890 to 1924 and 1924 to 1970.

Model Specification and Data

The analyses. consist of a series of time-series regression analyses using annual national enrollment and immigration figures. In order to estimate the effects of immigration on enrollments, we need to develop a basic model of the growth of enrollments within which the effects of immigration can be studied. Following the work of Nielsen and Hannan (1977) and of Meyer et al. (1977), the basic model for the growth of primary education is as follows:

$$\begin{aligned} &P_t - P_{\Delta t} = \\ &x + B_1 P_{\Delta t}(\mathrm{PRIPOP}_{\Delta t} - P_{\Delta t}) \\ &+ B_2 \mathrm{PRIPOP}_{\Delta t} \\ &+ B_3 \mathrm{IMM}_{\Delta t} + e, \qquad (1) \end{aligned}$$

where: P_t and $P_{\Delta t}$ = primary-school enrollment at time t and an earlier time Δt; PRIPOP = primary age-group population; and IMM = number of immigrants.

This model is based on the following ideas. As an institution, schooling is to some extent self-perpetuating. Once in school, we would not expect parents to withdraw students from school or government agencies to totally withdraw support. Therefore, the maintenance of a constant number of students in school between time t and some previous time Δt is not problematic. What is to be explained is the change in the number of students in school. The dependent variable, then, is the difference in the number of students between time t and Δt. The growth of enrollments, as in most growth processes, is not linear. The rate of educational expansion begins slowly at low levels of enrollment, increases rapidly at middle levels of enrollment, and slows down as enrollments approach their ceilings of the total eligible age-group population (OECD, 1971). The term $P_{\Delta t}(\mathrm{PRIPOP}_{\Delta t} - P_{\Delta t})$ models this S-curve process of growth. This term says that the growth of enrollments in primary school is a function both of the earlier number of people in school, $P_{\Delta t}$, and of the number of people in the eligible age-group not in school, $PRIPOP_{\Delta t} - P_{\Delta t}$. The eligible age-group population at the earlier time, $PRIPOP_{\Delta t}$, is also included in the model to control for the effect of changes in the size of the age-group population of changes in the number of students enrolled. These terms in Equation 1, then, model the organizational and demographic factors affecting the growth of school enrollments. The effect of immigration and other variables is analyzed within the context of this model. The model used for secondary education is the same except that since the number of students enrolled in secondary education depends on the number in primary education, a term representing this succession process is included.

The analyses consist of a number of time-series regressions on annual, national data for primary and secondary education, both public and private, from 1890 to 1970. School enrollment data are from the *Biennial Survey of Education* (called the *Annual Report of the United States Commissioner of Education* until 1916).[1] Data for the age-group population are from *Current Population Reports* (United States Bureau of the Census, 1974). For the primary age-group population we use the population 5 to 13 years of age, and for the secondary age-group population we use the population 14 to 17 years of age.[2] The number of immigrants per year is from the *Historical Statistics of the United States* (1976). Since the effects of immigration on enrollments necessarily imply some time period for their operation, we use both 5- and 10-year time lags (Δt).[3]

The Growth of Public School Enrollments

We begin by analyzing the effects of immigration on public school enrollments at primary and secondary levels from 1890 to 1970. We divide this time period into two parts, 1890 to 1924 and 1925 to 1970.[4] We analyze these periods separately to take into account the dramatic change in the character of immigration brought about by the National Origins and Quota Acts of 1924.

Table 4 presents the regression estimates and t-scores for public primary and secondary enrollments lagged 5 and 10 years for the period 1890–1924 in the top panel, and the estimates for the period 1925–1970 in the bottom panel. Inspecting the equations for primary enrollment (equations A, B, E, F), we see that both the term modelling the S-curve growth process and the eligible age-group popu-

---

[1] These enrollment figures are from annual national population surveys of schools. The *U.S. Historical States, Colonial Times to 1970* presents these figures for even-numbered years after 1918 (Series 422, 424, 427, 429). The figures for the omitted years were supplied by W. Vance Grant of the United States Office of Education.

[2] In preliminary analysis of the school data, there was a strikingly high proportion of the eligible age-group population enrolled in public primary school as early as 1870. This observation led to a reexamination of the raw data. One conjecture was that the simple calculation of an enrollment rate for schooling (number of pupils enrolled/number of eligible-age individuals) contains a source of bias in the denominator. The bias derives from differential rates of grade-level promotion (the advancement of pupils from one grade-level to the next) that have changed over time. The possibility arises that a significant portion of enrolled students may be older than the range of eligible ages standardized for in the denominator. In this case, the enrollment rate will be biased upward for those historical periods characterized by large amounts of grade level "flunking." We constructed two other enrollment series based on calculations of the number of overage students in primary and secondary school. The new enrollment figures were not substantially changed, at least not to the extent that would "explain away" the high levels of enrollments with which we were initially concerned. Second, preliminary analyses were run using all three sets of enrollment figures, the original data plus the two transformed time-series. The resulting estimated effects of the coefficients also were substantially unchanged. (See Ralph, 1978, for full details.)

[3] The choice of lag periods in which the process by which immigration affects enrollment were selected as five and ten years to allow for both shorter- and longer-term processes. We present both lag periods here as a means to show consistency of the results. It is possible to interpret the five- and ten-year lags as representing different mechanisms for the effects, but we choose not to do so here until the potentially different mechanisms can be theoretically specified.

[4] Equation 1, for a five-year lag, then, specifies that $P_t - P_{\Delta t}$ is primary enrollment in 1894– primary enrollment 1890, primary enrollment 1895– primary enrollment 1891, etc; and that $IMM_{\Delta t}$ is immigration in 1890, 1891, etc. No data overlaps the two time periods of 1890 to 1924 and 1925 to 1970.

Table 4. Public-School Enrollments in the United States Regressed on Immigration in Two Periods: 1890–1924, 1930–1970

	Independent Variables				
Dependent Variable	Growth Term[b]	Eligible-age Population[c]	No. of Immigrants[d]	R^2	Durbin-Watson
A. Primary Enrollment,[a] 1890–1924, 5-year lag	.4E-3(2.46)*	.86(3.7)	−.039(−2.04)	.52	1.79
B. Primary Enrollment, 1890–1924, 10-year lag	.14E-2(2.67)	1.05(3.77)	−.03(−.79)	.67	1.94
C. Secondary Enrollment, 1890–1924, 5-year lag	.1E-2(19.1)	−.3(−2.25)	−.016(−2.9)	.94	1.57
D. Secondary Enrollment, 1890–1924, 10-year lag	.3E-2(9.97)	−.5(−2.04)	−.006(−.34)	.92	.64
E. Primary enrollment, 1930–1970, 5-year lag	.8E-4(4.52)	.73(34.6)	.18(2.03)	.99	.92
F. Primary Enrollment, 1930–1970, 10-year lag	.2E-3(10.8)	.78(63.4)	.29(5.4)	.996	.95
G. Secondary Enrollment, 1930–1970, 5-year lag	.17E-3(1.13)	.9(9.37)	−.1(−.93)	.89	.52
H. Secondary Enrollment, 1930–1970, 10-year lag	.4E-3(1.16)	.68(15.2)	.45(5.25)	.96	.69

NOTES: * Table entries are slope, with T-statistics shown in parentheses.

[a] The enrollment variables are constructed as difference terms, 5 years apart in the 5-year lags and 10 years apart in the 10-year lags; e.g., enrollment $_{1896}$ − enrollment $_{1890}$.

[b] The growth term as discussed in the text is enrol$_{t-5}$ (eligible-age pop.$_{t-5}$ − enrol$_{t-5}$).

[c] The population variables are also constructed as difference terms at 5- and 10-year intervals.

[d] Immigration variable is the number of immigrants arriving each year, lagged 5 or 10 years.

lation are positive and significant. The number of immigrants has a negative effect for both lag periods between 1890 and 1924 in equations A and B, but the coefficient is not significant for the 10-year lag. In contrast, the number of immigrants has a significant positive effect in the period from 1925 to 1970 as shown in equations E and F. A similar pattern appears in the results for secondary-school enrollment. The growth term again is, as expected, positive in each equation for both time periods. The eligible age-group population is, however, negative in the earlier period (equations C and D); though its effects return to positive in the later period.[5] The pattern of effects of immigration on secondary enrollments is similar to its effects on primary enrollments. In the earlier period, the effects of immigration are negative (equations C and D), though the coefficient is not significant for the 10-year lag. In the later period, the effect of immigration is negative, but not significant for the 5-year lag; but this effect is positive and significant for the 10-year lag.[6]

The general conclusion from Table 4 is that immigration has had a negative effect on the rate of public primary- and secondary-school expansion between 1890 and 1924; but, after the enactment of the National Origins and Quota Acts, the effect of immigration has been positive. During the period 1890–1924, when there was widespread concern with the character of immigration, and nativist sentiment

---

[5] The negative effects of the eligible age-group population for secondary enrollment in the earlier period probably reflect the relatively low levels of secondary enrollments at the start of the period. The dominant demographic effect, then, is due not so much to increases in the eligible population but rather to the interaction of the eligible population and already enrolled individuals as embodied in the growth term.

[6] The Durbin-Watson statistics reported in these tables do not always reach significant levels, meaning that we cannot reject the hypothesis of first-order autoregressive serial correlation. The presence of such serial correlation may affect the significance of the coefficients, but the coefficients are still unbiased. We base our arguments in the text on the consistency of the signs of the coefficients for immigration on both public and private enrollments in the two time periods. When equations contain lagged endogenous variables as specified in Equation 1, the Durbin-Watson statistic is not always an appropriate test for first-order autoregressive serial correlation.

Table 5. Public-School Enrollments in the United States Regressed on Immigration in Two Periods with Unemployment Controlled: 1890–1924, 1930–1970

	Independent Variables					
Dependent Variable	Growth Term[b]	Eligible-age Population[c]	No. of Immigrants[d]	Unempl.[e]	R^2	Durbin-Watson
A. Primary Enrollment,[a] 1890–1924, 5-year lag	.4E-3(3.21)*	.82(4.05)	−.03(−1.81)	4.34(2.98)	.65	1.96
B. Primary Enrollment, 1890–1924, 10-year lag	.15(4,04)	1.1(5.54)	−.05(−1.63)	10.8(4.53)	.84	2.6
C. Secondary Enrollment, 1890–1924, 5-year lag	.12E-2(21.3)	−2.92(−2.26)	−.014(−2.82)	1.34(2.79)	.95	1.74
D. Secondary Enrollment, 1890–1924, 10–year lag	.29E-2(9.8)	−.49(−1.87)	−.006(−.34)	−.93(−.59)	.93	.72
E. Primary Enrollment, 1930–1970, 5-year lag	.7E-4(4.35)	.75(35.4)	.17(1.99)	.3(2.13)	.99	.78
F. Primary Enrollment, 1930–1970, 10-year lag	.17E-3(9.48)	.79(62.4)	.28(5.43)	.28(1.56)	.996	.81
G. Secondary Enrollment, 1930–1970, 5-year lag	.2E-3(1.23)	.89(9.23)	−.098(−.859)	−.13(−.548)	.89	.55
H. Secondary Enrollment, 1930–1970, 10-year lag	.4E-3(1.19)	.69(14.6)	.45(5.18)	−.08(−.33)	.96	.71

NOTES: * Table entries are slope, with T-statistics shown in parentheses.

[a] The enrollment variables are constructed as difference terms, 5 years apart in the 5-year lags and 10 years apart in the 10-year lags; e.g., enrollment $_{1895}$ − enrollment $_{1890}$.

[b] The growth term as discussed in the text is enrol_{t-5} (eligible-age $\text{pop.}_{t-5} - \text{enrol}_{t-5}$).

[c] The population variables are also constructed as difference terms at 5- and 10-year intervals.

[d] Immigration variable is the number of immigrants arriving each year, lagged 5 or 10 years.

[e] Unemployment variable is lagged 5 or 10 years.

was greatest, the effect of immigration on the growth of public schooling was negative. We see that immigration has had a positive effect on the expansion of public schooling only after legislation had altered the character of immigration in favor of those from northern and western Europe.

Before we accept the conclusions from Table 4, however, we need to take account of economic conditions. Variations in immigration have always been related to the business cycle; and school enrollments have often responded to cycles of the economy, with more people staying longer in school during bad economic times. Therefore, in analyzing the effects of immigration on school expansion, we need to control for the potentially confounding effects of economic conditions. Table 5 shows the consequences of immigration on schooling when the unemployment rate, our measure of economic conditions, is added to the equations in Table 4. The data on unemployment are from Lebergott (1964) and the United States Department of Labor (1972). Table 5 shows that the effect of unemployment is generally positive and significant, as was expected. Where it is not positive (equations D, G, and H), its effect is not significant. More importantly, the inclusion of unemployment does not alter the effects of immigration.[7]

These findings show that the effects of immigration on aggregate rates of the growth of public-school enrollment have varied as a consequence of the character of the immigrants themselves. The finding that immigration had a negative effect on the growth of public schooling during the period when the immigrants were predominantly from eastern, central and southern Europe, and that the effect of immigration was positive during the period when the immigrants were predominantly from northern and western Europe, leads toward explanations based on the societal reactions of political and economic elites and of nativist groups to the different types of immigrants in the two periods. It may be argued, however, that the negative effect of immigration between 1890 and 1924 was due to the presumed fact that the schooling capabilities and motivations of these immigrants were lower than those from northern and western Europe. As a consequence, these children may have been less likely to go to school at every grade or, if in school, their lower capabilities resulted in higher rates of failure. Both of these mechanisms could produce a negative effect of immigration on the growth of enrollments during this earlier period. Certainly the public controversy at this time presumed that these new immigrants lacked motivation and the same level of natural abilities as the older immigrant groups from northern and western Europe (Higham, 1974; Kamin, 1974). But the data presented in Tables 1 and 2 argue strongly against such an explanation for the negative effect of immigration on enrollment. For these tables reveal that, in the period when we would expect these differences in schooling experience to be most marked, the rates of school attendance and of retardation, or of flunking, were basically the same for the children of immigrants and of native-born fathers.

---

[7] Change in GNP was also used as a measure of economic business cycles in parallel analyses. The results of these equations were similar to those using unemployment.

But we can go further in our analyses. If these schooling experiences of the children of immigrants and of native-born fathers were similar, and there is a negative effect of immigration on enrollment growth of public schooling during this period, then we should expect to find a positive effect of immigration on the growth of private-school enrollments from 1890 to 1924. We turn now to an analysis of private schooling.

The Growth of Private School Enrollments

The growth of private education has often been interpreted as a consequence of the desire of minority groups to maintain their separate identities when they feel public schooling is imposing dominant cultural values on them (Katz, 1968; Meyer, 1970; Meyer and Roth, 1970). Parochial education exemplifies the desire and commitment to maintain and reinforce religious identities and values that differed from the Protestant-dominated public schools (Tyack, 1970). It is equally plausible that the children of immigrant groups attended private schools as a result of the inability or unwillingness of public schools to incorporate them, rather than as an assertion of their independent status (Ravitch, 1978:58). As with the analysis of public schooling, our data do not allow us to determine the exact reasons for any relationship between immigration and the growth of private schooling, but we can find if such a relationship does exist.

The enrollment data for private schooling does not appear separately for primary and secondary levels throughout our time periods. Table 6, then, reports the effects of immigration within the two time periods for combined primary and secondary private schooling, including equations with and without the inclusion of unemployment. As in Tables 4 and 5, the effect of the growth term is always positive and significant in all equations except equation C. As before, the eligible age-group population has negative effects in the earlier period and positive effects in the later period.

The effects of immigration on the growth of private schooling support our expectations. As shown in Equations A through D, the effect of immigration during the period 1890 to 1924 is always positive and significant. Unemployment, though positive, is not significant and it does not affect the relationship between immigration and enrollments. The effect of immigration in the later period, however, is consistently negative, though significant only in equation E. Unemployment has a negative but not significant coefficient, and its inclusion does not alter the relationship between immigration and enrollments.[8]

The findings in Table 6 stand in marked contrast to those in Tables 4 and 5. Those two tables showed that the effect of immigration had been to lower the rate of growth from 1925 to 1970. The effects of immigration on private school enrollments have been just the opposite. In the period 1890 to 1924, immigration increased the

---

[8] The consistent nonsignificant effects of unemployment on private enrollments can be interpreted to mean that the general tendency of people to stay in school during bad economic times holds true for public schooling but not for private.

Table 6. Public-School Enrollments in the United States Regressed on Immigration in Two Periods with Unemployment Controlled; 1890–1924, 1930–1970

	Independent Variables					
Dependent Variable	Growth Term[b]	Eligible-age Population[c]	No. of Immigrants[d]	Economic Control	R^2	Durbin-Watson
A. Private Enrollment,[a] 1890–1924, 5-year lag	.23E-3(2.19)*	−1.19(−.84)	.24(2.75)		.35	.71
B. Private Enrollment, 1890–1924, 10-year lag	.59E-3(3.96)	−5.97(−3.63)	.56(2.62)		.58	.61
C. Private Enrollment, 1890–1924, 5-year lag	.24E-3(1.82)	−1.28(−.83)	.24(2.70)	1.57(.152)[e]	.35	.70
D. Private Enrollment, 1890–1924, 10-year lag	.57E-3(3.78)	−5.95(−3.57)	.56(2.58)	9.78(.69)[e]	.59	.59
E. Private Enrollment, 1930–1970, 5-year lag	.97E-4(7.72)	2.2(18.6)	−1.18(−2.18)		.94	.52
F. Private Enrollment, 1930–1970, 10-year lag	.21E-3(12.2)	1.86(34.1)	−.77(−1.69)		.98	.43
G. Private Enrollment, 1930–1970, 5-year lag	.1E-3(7.88)	2.19(17.3)	−.99(−1.8)	−7.4(−1.34)[e]	.94	.54
H. Private Enrollment, 1930–1970, 10-year lag	.22E-3(9.91)	1.85(32.8)	−.56(−.90)	−6.9(−.49)[e]	.98	.45

NOTES: * Table entries are slope, with T-statistics shown in parentheses.

[a] The enrollment variables are constructed as difference terms, 5 years apart in the 5-year lags and 10 years apart in the 10-year lags; e.g., enrollment$_{1885}$ − enrollment$_{1890}$.

[b] The growth term as discussed in the text is enrol$_{t-5}$ (eligible-age pop.$_{t-5}$ − enrol$_{t-5}$).

[c] The population variables are also constructed as difference terms at 5- and 10-year intervals.

[d] Immigration variable is the number of immigrants arriving each year, lagged 5 or 10 years.

[e] Unemployment variable is lagged 5 or 10 years.

rate of growth of private schooling and decreased this rate from 1925 to 1970.[9]

Conclusion

The major conclusion from our empirical analyses is that the effect of immigration on the expansion of education has varied as a function of the character of the immigrants. From 1890 to 1924, during the period of the new immigration from eastern, central, and southern Europe, the effect of immigration was to decrease the rate of growth of public-school expansion and to increase the rate of growth of private-school expansion. After 1925, when the character of immigration shifted to favor the older immigrant groups from northern and western Europe, the effect of immigration was to increase the rate of growth of public-school expansion and to decrease the rate of growth of private-school expansion.

How do these findings contribute to the development of a theory of school expansion? Most importantly, they show that the growth of schooling responds to the character of social groups as this affects their interaction with other, more established, groups in society. The study by Meyer et al. (1979) showed that the early expansion of public primary schooling was a function of the cultural and organizational role of the nativist millenial-Protestant groups in the United States. Our findings complement and expand on this process by which educational expansion develops as a consequence of the social character of groups and their interaction with each other. The new immigrant groups which came to the United States between 1890 and 1924 were generally viewed as a cultural and political threat by many established groups. They were, in most respects, seen as carrying cultural and political traditions opposite to those of dominant elites and nativist groups and their presence generated a great deal of cultural and political conflict. Our findings, then, stand in a complementary relationship with those of Meyer et al. (1979). He showed that the presence of the nativist millenial-Protestant groups increased the aggregate rate of public school expansion. Our findings have shown that the influx of groups of opposite character decreased the aggregate rate of public-school expansion and increased the rate of private-school expansion. After the National Origins and Quota Acts of 1924 shifted the character of the immigrants, immigration increased the rate of growth of public schooling and decreased the rate of growth of private schooling.

---

[9] These results also clarify a finding in the analyses by Meyer et al. (1979). They found a negative effect of the proportion of the population that was Catholic and an insignificant negative effect of the proportion of the population that was immigrant on public primary enrollment. As they did not have enrollment for private schools, they could not readily interpret these effects and used them as control variables. Our tables show that the effect of immigration was different for public and private enrollments.

The argument of Meyer et al. (1979) emphasizes the cultural values and world view of the traditional, nativist groups as the cause for their constructing a widespread system of public schooling. They emphasize the role of Protestantism and independent entrepreneurship. The immigrants who came to the United States between 1890 and 1924 were predominately Catholic and working-class laborers. The evidence we have presented here, however, does not lead us to emphasize the cultural values and world views of these immigrants as an explanation for our findings. Rather, what seems to be important is that these groups were different and were perceived as oppositional to the established groups in the United States. The rates of schooling of the children of immigrants and of native-born fathers were similar; therefore, the consequences of immigration for variation in the aggregate rate of growth in public and private schools from 1890 to 1970 must be understood as the net result of the actions of a large number of groups, not just of the schooling behavior of the immigrants and their children.

Contemporary theories of schooling which emphasize the actions of a wide range of groups are often divided into functionalist, class-conflict, and status-conflict perspectives (Bowles and Gintis, 1976; Collins, 1971; Ravitch, 1978). Functionalist and class-conflict theories stress the role of public schools as agencies of imposition which properly socialize groups to the cultural and political system of the United States. Since the immigration from 1890 to 1924 was clearly seen as a cultural and political threat, such theories imply the expansion of public schooling during this period as cultural, economic, and political elites supported increased schooling as a mechanism of imposition. But our analyses are not consistent with such a process, for our findings have shown that, during the period of such immigration, immigration had a negative effect on the rate of public primary- and secondary-school expansion. When the character of the immigrants changed—that is, when their origins no longer represented a cultural and political threat—then immigration had a positive effect on the rate of growth of public schooling. A status-conflict process of schooling can better explain our findings. Under this perspective, schooling is seen as an asset for group mobility. A status-conflict view is consistent with our findings that the influx of immigration between 1890 and 1924 caused a decrease in the rate of growth of public schooling and an increase in the rate of growth of private schooling, as the threat to the dominant status group could cause a restriction in the growth of public schooling, causing immigrant status groups to concentrate in private schools.

However, our findings alone are not enough to substantiate these more general theories about growth rates in education. One reason is that these theories have not been specified enough to deduce the effects of different groups on educational expansion. A second reason is that it is not the findings about any one specific group's consequences for education that will differentiate such theories. Rather, in order to develop a general body of empirical findings that can be used in the development of a theory of educational expansion, we need to accumulate a series of such studies across groups which vary in their relationships to other groups in society.

REFERENCES

Ayres, Leonard
1909 Laggards in Our Schools. New York: Russell Sage.

Bowles, Samuel, and Herbert Gintis
1976 Schooling in Capitalist America. New York: Basic Books.

Butts, R. Freeman
1978 Public Education in the United States: From Revolution to Reform. New York: Holt, Rinehart, and Winston.

Collins, Randall
1971 "Functional and Conflict Theories of Educational Stratification." American Sociological Review 36:1002–18.
1976 Review of Schooling in Capitalist America: Educational Reform and the Contradictions of Economic Life. Harvard Educational Review 46:246–51.

Cubberly, E. P.
1909 Changing Concepts of Education. Boston: Houghton Mifflin.

Everhart, Robert B.
1977 "From Universalism to Usurpation: An Essay on the Antecedents to Compulsory School Attendance Legislation." Review of Educational Research 47:499–530.

Greer, Colin
1972 The Great School Legend: A Revisionist Interpretation of American Public Education. New York: Basic Books.

Higham, John
1974 Strangers in the Land: Patterns of American Nativism, 1860–1925. New York: Atheneum.

Kamin, Leon
1974 The Science and Politics of IQ. Potomac, Maryland: Erlbaum Associates.

Katz, Michael B.
1968 The Irony of Early School Reform. Cambridge: Harvard University Press.
1971 Class, Bureaucracy, and Schools. New York: Praeger.

Lebergott, Stanley
1964 Manpower and Economic Growth. New York: McGraw-Hill.

Meyer, John W.
1970 "The Charter: Conditions of Diffuse Socialization in Schools." Pp. 564–78 in R. W. Scott (ed.), Social Processes and Social Structures. New York: Holt, Rinehart, and Winston.

Meyer, John W. and James G. Roth
1970 "A Reinterpretation of American Status Politics." Pacific Sociological Review 13:95–102.

Meyer, John W., Francisco O. Ramirez, Richard Rubinson, and John Boli-Bennett
1977 "The World of Educational Revolution, 1950–1970." Sociology of Education 50:242–58.

Meyer, John, David Tyack, Joane Nagel, and Audri Gordon
1979 "Public Education as Nation-Building in America." American Journal of Sociology 85:591–613.

Nielsen, François and Michael T. Hannan
1977 "The Expansion of National Educational Systems: Tests of a Population Ecology Model." American Sociological Review 42:479–90.

Organization for Economic Cooperation and Development
1971 Policies for Educational Growth. Paris.

Ralph, John
1978 Determinants of Primary and Secondary Educational Expansion in the United States, 1870–1970. Ph.D. dissertation. Department of Social Relations, Johns Hopkins University.

Ravitch, Diane
1978 The Revisionists Revised. New York: Basic Books.

Roberts, Peter
1912 The New Immigration: A Study of the Industrial and Social Life of Southeastern Europeans in America. New York: Macmillan (Reprint Edition 1970 by Arno Press, Inc.)

Smith, Timothy L.
1967 "Protestant Schooling and American Nationality, 1800–1850." The Journal of American History 53:679–95.

Taeuber, Conrad and Irene B. Taeuber
1968 "Immigration to the United States." Charles B. Nam (ed.), 1968, Population and Society. Boston: Houghton Mifflin.

Tyack, David
1970 "Onward Christian Soldiers: Religion in the American Common School." In Paul Nash (ed.), History and Education. New York: Random House.
1974 The One Best System: A History of American Urban Education. Cambridge: Harvard University Press.

United States Bureau of the Census
1900 Twelfth Census of the United States. Washington, D.C.
1976 Historical Statistics of the U.S.—Colonial Times to 1970. Bicentennial Edition, Parts 1 and 2, Washington, D.C.
1974 Current Population Reports, Series P-25 No. 519.

U.S. Department of Labor
1972 Manpower Report of the President. Washington, D.C.

United States Immigration Commission
1911 The Children of Immigrants in Schools. Washington, D.C.: U.S. Government Printing Office.

United States Senate Documents
1911 61st Congress, 3rd Session, Immigration Commission Abstract, Volume 8. Washington, D.C.: U.S. Government Printing Office.

Weinstein, James
1968 The Corporate Ideal in the Liberal State, 1900–1918. Boston: Beacon Press.

THE MEEK SHALL NOT INHERIT THE EARTH: SELF-EVALUATION AND THE LEGITIMACY OF STRATIFICATION*

L. RICHARD DELLA FAVE
North Carolina State University

American Sociological Review 1980, Vol. 45 (December):955–971

Stratified social orders are maintained through a wide variety of mechanisms, one being broad-based legitimation of the notion of unequal distribution of primary resources. My attempt to develop a set of propositions provides at least a partial explanation of how such legitimation is generated and maintained. I argue that both conflict and functional theory point, at least implicitly, to the importance of the effect of unequal distribution of resources on the development of the self—constructing my argument through application of Mead's theory of the self to the case of stratification. This application is shown, in turn, to be compatible with several lines of theorizing in social psychology, including equity and status attribution theory. Once basic propositions are developed, I discuss ways in which major social institutions maintain legitimacy through their effect on the self and explore some possible sources of delegitimation.

One of the most persistent features of human societies has been the existence of some form of stratification. Theorists have tried to explain it: functionalists (Parsons, 1949; 1953; Davis and Moore, 1945; Davis, 1948; Baltzell, 1958; Keller, 1963) have stressed the unequal functional importance of occupational roles; elitist conflict theorists (Pareto, 1935; Mosca, 1939; Dahrendorf, 1959; Lenski, 1966) have emphasized the inherently unequal ability of people to pursue the struggle over scarce resources; and radical conflict theorists (Marx and Engels, 1959; Miliband, 1969; Habermas, 1973; Anderson, 1977; Harrington, 1976) have pointed to a succession of historically specific types of social systems whose very structures (principles of organization) insure continuing inequality.

Despite their considerable differences, each type of theorist (Strasser, 1976)[1] regards legitimation as an important mechanism through which stratification is perpetuated (Coser, 1956; Habermas, 1973). Legitimation refers to a belief on the part of a large majority of the populace that institutionalized inequality in the distribution of primary resources (Rawls, 1971)[2]—such as power, wealth, and prestige—is essentially right and reasonable (Feagin, 1972; Rainwater, 1974; Jasso and Rossi, 1977; Alves and Rossi, 1978). While the specific rationales used to justify inequality vary from one time and place to another (Bendix, 1956; Dumont, 1970; Wilson, 1973), the essence of legitimation is based in the existence of some set of such justifications which is widely believed—indeed, justifications sometimes so taken for granted as not even to

---

* Direct all correspondence to: L. Richard Della Fave; Department of Sociology/Anthropology; North Carolina State University; Box 5535; Raleigh, NC 27650.

This is a much-revised version of a paper presented at the meeting of the Society for the Study of Social Problems, September, 1978, San Francisco.

I would like to thank Robert L. Moxley and two anonymous reviewers for *ASR* for their most helpful comments on an earlier draft of this paper.

[1] In his analysis of the work of classical theorists, Strasser (1976) argues they can be classified along at least two dimensions: (1) in terms of their basic model of society (order or conflict) and (2) according to their fundamental political orientation (radical or conservative). For a thorough exposition of this classification scheme see Strasser (1976:19–27). Basing his analysis of contemporary stratification theory upon the Strasser typology, Kerbo (1975) has shown that for this group of theorists the radical-order cell is empty. Considering the macrotheorists to be discussed in this paper, Marx falls into the radical-conflict cell, Mosca into the conservative-conflict cell, and Parsons the conservative-order cell.

[2] It should be noted that the focus of this paper is social stratification which is ultimately concerned with *distribution* of resources. In contrast, the emphasis of Marxist class analysis lies in the sphere of *production*. While the relationship between these two spheres is enormously complex and will not be discussed here, it is important to realize that the attainment of socialism (Marx, 1959) or, more specifically, worker control of the means of production (Blumberg, 1968) is far from synonymous with the elimination and/or delegitimation of stratification itself.

be questioned (Shibutani and Kwan, 1965; Dumont, 1970; Moore, 1978).

Given the importance of legitimation in these writers' works, it is surprising that none, with the exception of Habermas (1973)[3], has attempted to develop a systematic theory of legitimation, one that would specify its causes, explain its strength under various conditions, and describe the process through which it ultimately becomes part of the individual's thought. The propositions I will develop should be a component of this type of theory.

In developing a theory of legitimation it is important to understand that legitimation is only one of a number of means of maintaining stratification, namely through gaining at least the tacit consent of the populace, especially those who are relatively disadvantaged. In fact, legitimation is only one of several consent-generating mechanisms. Others include hopes for future upward mobility for self or children (Chinoy, 1955; Lane, 1962; Lopreato and Hazelrigg, 1972; Form, 1976); fear of jeopardizing one's absolute standard of living (Mankoff, 1970); a feeling of powerlessness (Wright, 1976); fear that the freedom and popular influence existing under representative democracy could be lost in a revolution aimed at bringing about greater equality (Anderson, 1977); and the inability to imagine any egalitarian alternative model of society (Dahl, 1967; Mann, 1970; Sennett and Cobb, 1972; Sallach, 1974; Moore, 1978).

A comprehensive theory of stratification would need to consider all those mechanisms as well as all-important nonconsensual factors such as coercion (Lenski, 1966; Anderson, 1977), the influence of moneyed interests over government (Mills, 1956; Domhoff, 1967; 1971; Prewitt and Stone, 1973), and the structural imperatives of institutions in class-ridden societies (Miliband, 1969; Parkin, 1971, Harrington, 1976). While the development of such a comprehensive theory is far beyond my intended scope here, this work can be seen as one aspect of some such larger project to which a growing number of social scientists are contributing.

Legitimation, we should be careful to note, can apply to any one of at least four system levels: (1) the particular status occupied by a given individual or group, (2) the particular regime in power, (3) a particular system of political economy, and (4) stratification itself (Della Fave, 1974). The propositions to be developed here will focus primarily upon the fourth—the most general—level. However, the multileveled nature of legitimation must be kept in mind in order to understand how it is possible to make sense out of the seemingly paradoxical coexistence of interest group conflict, even to the point of revolution, and the continued legitimation of some form of stratification.

My objective is to develop propositions concerning legitimation through the use of the concept of self-evaluation.[4] Throughout, legitimation is treated as a process in which the structure of the larger society becomes incorporated within the inner consciousness of the individual (Mead,

---

[3] Habermas (1973) has approached the problem of legitimation by asking specifically, within the context of advanced capitalism, under what conditions the legitimacy of the existing inequalities of wealth—so integral to the system—will be seriously challenged. His answer is that this cannot happen until the economic crises that result from the working out of the internal contradictions of the system become displaced into the administrative (political) sphere. When this occurs, the contradictions remain, but their socially undesirable effects can no longer be attributed to the "nature-like" invisible hand of the impersonal market. Rather, they are traceable to the concrete decisions of identifiable government officials. This opens these decisions to public scrutiny and has the distinct potential for raising demands that violate the terms of the unequal status quo, what Habermas (pp. 68–9) calls the asymmetrical "class compromise" of advanced capitalism.

But here Habermas begs what I take to be the central question—namely, In the context of public discussion of this issue, under what conditions could the privileged members of a society mount effective, convincing arguments for their more privileged access to resources? As we shall see, the self-evaluation hypothesis addresses this question by specifying that successful arguments can be mounted only if the participants bring differential self-evaluations with them to such discussions.

[4] The concept of self-evaluation is not identical with that of self-esteem. The latter is far more general and refers to an individual's overall feeling about him/herself (Wells and Marwell, 1976; Coopersmith, 1967; Rosenberg, 1965). The concept of self-evaluation is specified later, herein.

1964:141). I will show the compatibility of this approach with three widely differing orientations—in macrosociology, in the microsociology of social behaviorism, and with empirical evidence derived from both survey and observational research dealing with a wide range of specific topics.

Then, first, I review relevant aspects of the work of Marx, Mosca, and Parsons (representing three major approaches in macrosociology—namely, radical conflict theory, elite conflict theory and functionalism), respectively (Strasser, 1976), pointing out, despite their differences, important similarities in their treatment of legitimation. Second, I turn to social psychology (namely, Mead's theory of the self, recent work on status attribution and equity theory, and Bem's critique of cognitive dissonance theory); I define the concept of self-evaluation, explicate its role in the legitimation process and, at that point, specify propositions. Third, I discuss the role of social institutions in the legitimation process and, four, conclude by exploring possible sources of delegitimation.

Macrosociology

The work of Marx, Mosca, and Parsons that I review is macrosociological in that its major focus is understanding why whole societies are organized as they are and why they change in particular ways. This does not mean that there is no concern with the social psychological processes that affect structure and change in societies—far from it. In fact, if these works lacked a social psychological side, it would have been far more difficult to identify relationships between them and the more explicitly social psychological work which I will also discuss.

Marx. Given Marx's view of capitalist society as rent by an irreconcilable conflict of interest between proletariat and bourgeoisie (Marx and Engels, 1959), he was faced with the task of explaining why this objectively defined conflict was not translated directly into class warfare over the very existence of the capitalist system itself. The answer, of course, lay in the concept of class consciousness. Workers display class consciousness in its rudimentary form as a result of their concrete struggles with employers over wages and working conditions (Marx, 1963:170–5; Marx and Engels, 1959: 19–20; Marx, 1906:396–7). Marx was confident that, after prolonged struggle and with the help of the radical party, workers would realize that only by overthrowing the capitalist system itself could they put an end to their own exploitation. He also believed they would come to see the pivotal role assigned to the working class by history—that of the liberator, rescuing mankind from the exploitation of class society (Marx and Engels, 1959:19)—by which action it would usher in a new era of equality and human freedom (Marx, 1959).

Marx (1959) was convinced, however, that even a successful revolutionary proletariat would still be under the sway of a capitalist-oriented ideology that justified more than trivial inequalities in the distribution of primary resources. Only after a period during which the revolutionary proletariat had actually been running the society would the working class arrive at a level of consciousness that would support the idea of equality, with exceptions based solely on the basis of need. Therefore, it is understandable how, in the absence of fully developed class consciousness, bitter conflict could coexist with the legitimation of stratification (a point to which we will return, later). While Marx fails to explain how the ideology of the ruling class became incorporated in the consciousness of the workers in the first place, he gives us some potentially fruitful insights into the legitimation process, insights that can best be understood if we look at the way stratification is supposedly delegitimated.

In Marx's schema, three things precede the delegitimation of stratification. First, the workers embrace an ideology that casts them in the role of the deliverers of mankind from oppression. Second, they are actually victorious in revolution. And third, they have the experience of successfully running an entire society by themselves, without the aid of their "betters." All three things, especially when taken together, are powerful sources of confidence in one's ability to control one's world, to act competently. Conversely, the experiences faced by workers (in their subordinated positions within the normal workings of capitalism) work to undermine that confidence—to make them feel dependent, powerless, and of little value.

In our subsequent discussions of the work of Mosca and Parsons we shall see that similar implications can be drawn, despite the sharp disagreements between each of these writers and Marx and, indeed, despite their differences with each other.

Mosca. Unlike Marx, Mosca saw no Utopian vision of equality nor freedom from the domination of some type of elite. In fact, central to his approach is the notion, often credited to Pareto (Meisel, 1965), that the few, everywhere and always, shall rule the many (Mosca, 1939).

While Mosca readily acknowledged that the dominance of elites rested heavily upon the use and threat of force, he was acutely aware of the importance of legitimation. Regardless of which elite happened to be in power, it promulgated a political formula that emphasized the allegedly superior qualities of its members (Mosca, 1939:82–92), by portraying them as being especially intelligent, moral, energetic, and/or strong. Most importantly, that political formula always portrays the ruling elite as embodying the society's most cherished values in greater measure than do other classes. Here, he introduces a theme that later recurs in the writing of Schumpeter (1951) as well as of functionalists such as Parsons (1949; 1953) and Davis and Moore (1945)—that is, the notion that members of the elite claim superior competence in precisely those activities (functions) that are most crucial to the particular society's survival and prosperity, those things such as military virtue, ritual purity, commercial acumen, or impartial justice.

Despite the variability in types of society and the qualities claimed by elites, his political formula is everywhere and always based upon the premise that superior people deserve to rule and enjoy the perquisites thereof. The successful promulgation of such a political formula cannot help but enhance, among members of the ruling elite, their feelings of competence and of being important and valued and yet undermine, among the masses, those same feelings.

Parsons. Turning now to the leading writer in the functionalist tradition, Talcott Parsons, we note a very different model of society than is characteristic of either Marx or Mosca. For Parsons (1949; 1951; 1953), a society is an organic whole in which the parts are differentiated, yet coordinated, to work toward the best interests of the whole. However, the functions (occupational roles) performed by the various parts are not of equal importance. In fact, there is a hierarchy of functional importance, with those roles most central to the survival and prosperity of society occupying the highest ranks (Parsons, 1953:102–3; 1949:83–4).

For Parsons there is well-nigh total consensus among all segments of the population concerning the functional importance of the various occupational roles, a consensus embodied in the very moral order of society (Parsons, 1949:69–70). Successful performance of these highly ranked roles represents the highest level of conformity to society's norms. And since all societies must reward conformity and punish deviance, the incumbents of the most functionally important roles receive the greatest amount of rewards in the form of primary resources. For it to be otherwise would seem incongruous and wrong to any properly socialized member of a society.[5]

Thus, the legitimacy accorded the unequal distribution of primary resources is based upon the deeply internalized norms of the society and is not subject to instrumental considerations of individual self-interest (Parsons, 1951:551).[6] This implies

---

[5] Parsons is certainly sophisticated enough to allow for the effect of factors other than functional importance in influencing the distribution of primary resources, factors such as the selfish use of power in garnering privilege (Parsons, 1953:95–6). However, the Parsonian model treats the effects of power as a form of disturbance, just as Manchester economics treated monopoly in its model premised upon "perfect competition." Nowhere do the effects of power become an integral part of the model, as they are for all varieties of conflict theory.

[6] Parsons is worth quoting here:

> Institutionalization of cultural patterns means, as we have often emphasized, in the integrated sense internalization of the same patterns in the person-

that the belief in the superiority of those in the most functionally important positions is nearly universal and deeply internalized as well.

Parsons's notion of an internalized moral order, with its hierarchy of roles based upon the idea of differential functional importance for society, elevates the sense of worth of the functionally most important, while that of most others is humbled (Parsons, 1949:73).

We have seen a common thread running through the work of writers as diverse as Marx, Mosca, and Parsons. In each case there is an ideology that justifies a highly stratified system of resource allocation. But this ideology also allocates feelings of potency, competence, and, above all, importance and self-worth in a manner congruent with that of primary resources. In order to better understand the mechanisms through which distributions of primary resources are reflected in and reproduce congruent distributions of feelings of self-worth, we now examine works in social psychology.

Social Psychology

Any theory of legitimation must ultimately answer the question of how legitimations of stratification become part of the consciousness of the individuals who make up society. By focusing, primarily, on the concept of self, I will describe the various phases of a process through which this might occur, beginning with the work of Mead.

Mead. For Mead (1934) the self was a crucial concept in the study of human behavior because only by having a self does the human organism become a conscious, reflective person. The self is truly a creature of society as it can develop only from the reactions of others. We become conscious of ourselves when we realize that our gestures, including verbal ones, call out the same response in others as they do in ourselves. Once we have established this, we can then take the role of the "other" and anticipate others' reactions to us or to a third party. Thus, we become aware of ourselves through the reactions of others. (Mead, 1934:141–9).

Because of the prolonged and acute dependency of the human infant we develop strong emotional bonds with those who satisfy our vital needs. They become our first significant others, and their reactions take on great importance. As we mature, our needs become more diversified and, as our circle of contacts broadens, we acquire a growing number of significant others.

As the development of the self continues, we become aware of certain similarities between what others expect of us and how they react to us. People, in general, see us as bright or dull, attractive or ugly, strong or weak, capable or incapable. This amalgam of perceived generalized expectations and reactions constitutes the generalized other. It is through the generalized other that we come to discover who we are and what is expected of us. Though our views (of who and what we are) vary not only over time but also from one situation to another, for most of us there remains a degree of consistency and stability commonly associated with maturity and mental health.

In an illustration of how the generalized other develops, Mead (1964) uses the example of the way in which we come to develop conceptions of the value of different goods in the marketplace—through observing how others react to the goods and, specifically, by noting for how much they offer them for sale and, in contrast, how much they are prepared to pay for them. Keeping in mind the ability of the self to view itself as object, we can see that selves are able to develop a conception of their own social value in very much the same way as they are able to understand the value of goods (Blau, 1964). This important idea will be developed further when we review some of the work in equity theory.

Recent work in social psychology, rele-

---

ality. Psychologically an internalized pattern is no longer an object of the situation. It is not possible to treat it as an instrumental means or condition. There is a specific model of cathectic integration of the actor's need dispositions with an internalized pattern. This fact has a fundamental methodological significance. It means that the orientation of "instrumental rationality" *cannot* be the attitude defining the actor's orientation to internalized patterns. (Italics in the original.)

vant to the problem at hand, has stemmed from two related lines of theoretical development—namely, equity theory and status attribution theory. Both have benefitted from extensive laboratory experimentation. Once they have been described, they will be linked to Mead's theory of the self through the work of Bem (1967).

Equity theory. Equity theory is based upon the notion of a balance—that is, individuals have a built-in tendency toward cognitive consistency (Heider, 1958; Festinger, 1957). Its main principle is that human beings carry around in their heads a notion that rewards should be proportional to investments, an idea Homans (1974; 1961) calls distributive justice. When the principle of distributive justice is violated, people become angry—morally outraged, one might even say. The individual who contributes more to the activities of a collectivity ought to receive a greater share of the rewards than one who contributes less.[7] Contributions can be in the form of expenditure of energy, lending of expertise, donation of money, acceptance of responsibility, etc. Evidence supportive of this approach for small groups is abundant (Leventhal, 1975; Homans, 1974; Ofshe and Ofshe, 1970).[8]

The relevance of equity theory for a theory of legitimation of inequality in larger societies has been pointed out by Lerner (1975) and Walster and Walster (1975). Those in power are assumed to be contributing more and, therefore, are seen as deserving of greater rewards. If ego is not among the major contributors to the group, even he/she comes to feel deserving of lesser rewards. Thus, inequality is made to seem legitimate. However, the unanswered question is, *Why* do people tend to perceive those in power as contributing more? For an answer we must turn to status attribution theory. Before doing this, however, it is important to keep in mind several relevant differences between small groups and whole societies—especially large, highly differentiated ones. In societies, inequality of resources is enormous, as compared with small groups. In most small groups (certainly those studied in the laboratory), coercion is not available to enforce inequality, as it is in societies. Also, the life of small groups is very short; in fact, they lack a history. Thus, inequality cannot become institutionalized over generations. Finally, small groups are, by definition, collectivities in which the members interact face to face. There is none of the remoteness as exists between leaders and populace in large societies. Among other things, this means that it is much more difficult to manufacture an impressive image of oneself, thereby to inflate one's importance, in a small group than in a large society.

Status attribution theory. The development of status attribution theory, like that of equity theory, is based upon the assumption of a strain toward cognitive consistency within the individual. The central problem lies in how an individual assesses the status of another when only one (or a few) status-relevant clues are known (Berger et al., 1972; Webster and Driskell, 1978). Normally, the individual generalizes from what is known and assumes that a person's unknown status-relevant characteristics are consistent with the known ones.

Since status attribution theory describes the process through which the overall picture of an individual's status is pieced together from fragmentary clues, we can see, once this picture is completed, how it is used to assess, retrospectively, the individual's "contributions."

The need to assess an individual's "contributions" in this manner is strongest when direct, face-to-face contact is at a minimum, which is the case with leaders in large, highly differentiated societies. Equity theory then describes how this, in turn, becomes the basis for an estimate of the level of reward deserved (Cook, 1975). Thus, equity theory and status attribution theory are complementary. What is crucial is that the entire process through which the appropriate level of reward is determined is *circular,* in that the very fact of being wealthy or powerful influences our assessment of "contributions" and, on the basis of such assessment, we judge that person worthy of such a high reward.

---

[7] This same idea is implicit in the functionalists' use of the concept of differential functional importance and even in Schumpeter's (1951) and Mosca's (1939) use of the notion of elites contributing disproportionately to a society's survival and prosperity.

[8] For example, laboratory subjects who are led to believe that individual A has contributed most to the group's success, individuals B and C have contributed moderately, and individual D very little tend to say that rewards should be divided in such a way so as to favor A most, B and C somewhere in between, and D least (Leventhal et al., 1972).

In large societies the degree of inequality of resources is most often very great (Lenski, 1966), making it likely that the disadvantaged will be awed by the size of the gap and attribute very large differences to "contributions."

Bem. As impressive and promising as status attribution theory and equity theory appear to be in developing propositions about legitimation, their primary assumption—that there exists a strain toward cognitive consistency within the individual psyche—has been challenged by Bem (1967). He argues that a whole range of heretofore inexplicable findings in laboratory research dealing with balance theory can be readily explained, were that assumption altered.

Bem (1967) suggests that we do not postulate any hypothetical strain toward cognitive consistency but, rather, we assume that an individual derives judgements as to what an "objective outside observer" would view as reasonable. This shift in assumptions solves an important problem. Social psychologists have long been plagued by the accusation that their findings reflect culture-bound thought patterns rather than universal human tendencies (Sampson, 1975). If the definition of what is reasonable (consistent) emanates not from the innate structure of mind but from an image of an external observer, it follows that the observer can vary in his/her judgements, depending upon the culture with which a researcher is dealing and upon the nature of the subject's individual experience.

But what external, objective observer is available and also, at the same time, likely to have his/her judgements internalized? Mead's generalized other has precisely these characteristics. As in his example of buying and selling goods, the way we develop an idea of what is a "reasonable" price or value of goods is what we see others regularly charging and/or paying for them. We have just seen that this is precisely the way in which Mead sees the generalized other developing.

This observation enables us to link equity and status attribution theory with Mead's theory of the self, for it is from the generalized other that individuals form an evaluation of self and, thus, of the worth of their "contributions." It is upon these evaluations, in turn, that judgements of equity are made in accordance with the principle of distributive justice.

So far we have achieved two major objectives. The first was to locate a common thread running through the macrosociology of writers as diverse as Marx, Mosca, and Parsons, with respect to their treatment of legitimation. This common thread is a relationship between legitimation and self-worth or, as we shall refer to it hereafter, self-evaluation. The second objective was to show that recent work in social psychology (specifically, equity theory and status attribution theory) is complementary, and that this work can be linked to Mead's theory of the self. Taken together, the social psychological works describe several aspects of the legitimation process. What remains now is to tie this in with macrosociology, in order to generate propositions about legitimation.

Before moving to the propositions themselves, the concept of self-evaluation must be explicated. The individual develops conceptions of self with respect to any number of possible characteristics. However, here we are concerned with those characteristics relevant to the individual's relationship with the institutions of the larger society. from which come rewards in the form of primary resources, specifically those characteristics enabling the individual to dominate or gain favor within these institutions.

The basic dependency of human beings makes impressive any ability to dominate the environment, either physically or socially. In the face-to-face context, leaders—those who are treated as most socially valuable and who are granted disproportionate rewards—are those who demonstrate their personal competence or, at least, those who do so before others can demonstrate theirs (Webster and Sobieszek, 1974). However, in large societies, the "demonstration" of the ability to dominate is mediated through a number of factors mentioned earlier in discussing equity theory. Dominance of the external world is most effective when it permits the formation of a *publicly defensible* positive evaluation of self—that is, an evaluation that is defensible before the widest possible audience. The extent of the audience is important as the apparent objectivity of societal reactions contributes so heavily to their power to convince. As objectivity is defined as intersubjective agreement, the wider the audience, then the more certain one can be of the objectivity of the assessment.[9] Thus, self-evaluation, as defined here, is the individual's perception of the social value that others attribute to him/her. Those who perceive that they are viewed as important by the widest audience, and particularly by others who are also viewed as important, have the highest self-evaluation. We are finally at the point where the diverse material presented so far can be pulled together and the propositions derived.

The Legitimation Process

The primary question to which this paper is addressed is, How do ideologies that justify structured inequality in the distribution of primary resources become part of the social consciousness of individuals? The main proposition of the theory states: *The level of primary resources that an individual sees as just for him/herself, relative to others, is directly proportional to his/her level of self-evaluation.* The individual's level of self-evaluation is developed through a process of comparison (Rosenberg and Pearlin, 1978). Thus, individuals with relatively high self-evaluations will see themselves as deserving of high levels of resources relative to others, whereas those with relatively low self-evaluations will see themselves as deserving of relatively little in comparison with others.

A corollary of this proposition allows us to deal with a related question—namely, Under what circumstances will the stratification system of any society enjoy relatively high or low levels of legitimacy? It states: *The strength of legitimacy of stratification in any society is directly proportional to the degree of congruence between the distribution of primary resources and the distribution of self-evaluations.* What follows is a discussion of the ways in which these propositions are derived.

Those possessing greater wealth and power in a society will tend to be perceived as having more of a whole range of other positive characteristics than those less fortunate or powerful. Through the status attribution process, those possessing greater amounts of primary resources come to be seen as actually being superior. At this point, the principle of equity takes over, and people believe that those who appear to be superior deserve to be more richly rewarded. This generalization, derived from previously reviewed laboratory studies, finds support in survey research as well (Fox et al., 1977; Lopreato and Hazelrigg, 1972).[10]

---

[9] Thus, for example, while the positive feedback of family and friends is affectively gratifying and is an important source of self-esteem, it would seem to lack the objective power of larger-scale reactions. Measures of self-esteem or self-concept used in sociological research and designed to test for a relationship between these variables and position in the stratification system (Rosenberg and Pearlin, 1978; Jacques and Chason, 1977) have used very general measures of self-concept (see footnote 4). Thus, their finding of a positive correlation leads me to believe that if a measure more specific to self-evaluation in terms of mastery of, and confidence in, abilities related to success in instrumental activity in social institutions were used the correlation would have been all the stronger.

[10] For example, both Fox et al. (1977) and Lopreato and Hazelrigg (1972) found that legitimacy and acquiescence to authority were *inversely* related to the authority level occupied by the subject. Thus, those closest to the top were *least* acquiescent. This is contrary to a simple conflict-oriented expectation that those who, themselves, had least authority would be least acquiescent. But these findings are compatible with the self-evaluation hypothesis as we would expect those occupying positions of relatively higher authority to have higher self-evaluations. Therefore, we would also expect them to be less

Of course, the attribution of superior characteristics to those with greater wealth and power implies that those lower in the stratification system will attribute to themselves relatively inferior characteristics, through comparison (Rosenberg and Pearlin, 1978; Jacques and Chason, 1977). Thus, if they are, in relevant ways, inferior, then they deserve to have lesser wealth and power. This is consistent with research on relative deprivation (Rushing, 1972; Runciman, 1966) in which members of different strata tend to compare themselves with others of their own stratum, rather than with those noticeably either richer or poorer than themselves. The latter type of comparison is meaningless precisely because those persons substantially higher or lower than oneself are obviously deserving of correspondingly greater or lesser rewards. The differences are simply taken for granted. It is the relatively small differences in rewards between oneself and those similar to self that excite feelings of deprivation.

Because of the circular nature of the process just described, a given distribution of primary resources tends to reproduce itself in a congruent distribution of self-evaluations which, in turn, reinforces the very status quo that generated it in the first place. While this describes the basic logic behind the main proposition, a closer study (of the mechanisms through which the process may be operating) is warranted.

Discussion of the status attribution process revealed that when ego meets alter and knows only one or two of alter's status-relevant characteristics, ego attributes other characteristics to alter—which, in the eyes of an imagined generalized other, are consistent with the ones ego knows. However, the work of Goffman (1959) strongly suggests that alter often behaves in ways that help the attribution process along. Highly placed individuals, by virtue of their greater wealth and power, are able to control or stage their encounters with subordinates in such ways as to impress them. For example, they may display their wealth in their personal appearance and in the very settings in which encounters take place. Also, the subordinate goes to his/her superior, not the reverse. Another staging device is the appearance of being calm and in control (Hall and Hewitt, 1970). The power to dictate explicitly, or better yet implicitly, where a meeting will be held is a convincing display of power. The subordinate's constant awareness of his/her asymmetrical dependence upon the superior serves this function (Emerson, 1962), as well as the superior's ability to make the subordinate wait (Schwartz, 1974).

Finally, the ability of superiors to stage their encounters allows them to shield their shortcomings from the view of subordinates. A good illustration of this is the fact that the deviance of the poor is more often performed in public places and is, thus, more visible than that of the rich (Johnson et al., 1977; Chambliss, 1973).

The process that generates differential self-evaluation does not operate in a vacuum but, rather, within the framework of social institutions—especially institutions such as the school and the workplace which are instrumental in determining one's place in the distribution of primary resources (Bowles and Gintis, 1976). Differential placement within these institutions, resulting from both competitive achievement and ascription, generates, within individuals, conceptions of where they stand in the larger society.[11]

---

likely to accept the legitimacy of the authority above them than would those with lower self-evaluations.

[11] In the case of schooling, Clark (1965) and Rosenthal and Jacobson (1968) have argued cogently that children who are treated as unworthy and incapable of learning are likely to internalize these judgements and fulfill the negative prophecies about themselves. Brookover and his associates (Brookover and Erickson, 1975; Brookover et al., 1964) have shown the strong impact that the school's evaluation of students' academic performance has upon their conceptions of their own ability, even when I.Q. is held constant. And Rosenbaum (1975) has shown how even the placement, itself, of high school students in "tracks," helps to determine I.Q.

In terms of the workplace, Kohn (1969) has shown how demands for conformity at work shape the individual's conception of his and his children's place in the realm of authority and obedience. We have seen how, from gradations of pay and the symbolic perquisites of office (Whyte, 1957) to the respect (or lack of it) with which one is treated on the job (Sennett and Cobb, 1972), the individual is confronted with seemingly objective and powerful messages about him/herself. At the negative extreme, the ef-

We have seen how the possession of greater primary resources allows dominants to control (through social institutions) the character of relationships between themselves and subordinates. One result of the establishment of these controlled relationships is the development of a generalized conception within the culture of the superiority of those who are highly rewarded and, by implication, the inferiority of others. This generalized conception develops out of communication among status-homogeneous groups. This is important because dominants maintain a certain degree of social distance between themselves and subordinates, which means that encounters with any one subordinate may not be frequent. Sharing of experience through communication creates a generalized set of expectations surrounding such relationships, expectations that become internalized through their embodiment in the generalized other. Through this process, the individual develops an evaluation of self with respect to the larger society. When the self-evaluations are congruent with actual command over primary resources, legitimacy emerges—as our corollary states. Through the resulting ideology, the circular logic of the legitimation process reinforces the stratified status quo.

If this description of the legitimation process is accurate, then the inverse of the corollary should also be true—namely: *The more incongruent the distribution of resources and self-evaluations, the more likely is the delegitimation of stratification.* This last point cannot be emphasized too strongly. Simply because I have referred to the legitimation process as self-reinforcing does not mean that "might becomes right," automatically. It is only when the distributions of resources and self-evaluations are congruent that it appears to do so. However, it is important to realize that, under most circumstances, the condition of congruence holds very well. This is precisely what accounts for the seemingly automatic character of the legitimation process, which may well have (mis)led functionalists into seeing it as universal. However, I maintain that incongruence between the two distributions can be produced by a number of forces—both internal and external to a society.

In order to round out the exposition of self-evaluation theory we must return to a question raised earlier—namely, How can the legitimation of stratification coexist with extensive intergroup conflict, and even revolution? The first step toward resolving this apparent paradox is to refer to our discussion of the four (system) levels at which legitimation can exist: (1) the position of the individual or group within a stratified order, (2) the particular regime in power, (3) the system of political economy, and (4) stratification itself. The specification of these levels shows how it is logically possible for legitimation to exist on some levels while conflict might prevail on others. This suggests that theories based upon conflict and those based upon consensus need not be incompatible but, rather, may simply be applicable to different system levels, but it fails to explain why this occurs or at which levels legitimation or conflict could be expected to be the dominant pattern.

Note also that these four levels fall along a continuum ranging from the more concrete to the more abstract. There appears to be a pattern in which conflict prevails at the more concrete levels while legitimation remains on the more abstract levels. For example, Marx and Engels (1959) note the tendency of workers to focus their discontent over poverty, harsh working conditions, and economic insecurity upon what appears to them to be its most immediate source—their employers or the machines being introduced into the factories. Similarly, Lenin (1963) observed that, left to their own devices, workers would not go beyond "trade union consciousness."

More contemporary writers have echoed that theme. Mills (1956) argued that people tend to be absorbed almost totally within their own limited milieux and are seldom able to connect private troubles with public issues. Both Mann (1970) and Parkin (1971) have pointed to the importance of a radical ideology which enables people to see the linkage between

---

fects of being locked into a cycle of menial jobs (Liebow, 1967) or, worse, prolonged unemployment (Bakke, 1940), can be nothing short of devastating.

everyday discontents and their systemic causes. Giddens (1973) has gone furthest in specifying a rather exacting set of conditions that must be fulfilled if workers are to proceed beyond a mere struggle with employers over economic issues (conflict consciousness) to the point where the legitimacy of the capitalist system is called into question (revolutionary consciousness).

Additional support for the existence of this pattern can be cited. Liebow's (1967) observations of unemployed blacks, and Sennett and Cobb's (1972) interviews with blue-collar workers both revealed, on the one hand, considerable resentment over material conditions and the low regard in which society holds them and, on the other hand, a virtually total failure to bring the legitimacy of the system itself into question. Needless to say, in no instance was the legitimacy of stratification itself ever challenged. Similar observations are reported by students of the Hindu caste system (Dumont, 1970; Mandelbaum, 1968).[12]

On the basis of this evidence I might frame a second proposition: *The probability that legitimation will prevail is directly related to the system level of abstraction.* That is, the higher the level of abstraction, the more likely a self-reinforcing legitimation process is to be found. At the very highest level—which is the focus of this paper—it has been nearly universal while, at the lowest level, conflict manifest in various forms has been the rule. Examples of those forces that might promote delegitimation and, therefore, signal the applicability of conflict theory at the highest level are now discussed.

Summary and Discussion

In recent years, conflict theory has gained an important place in American sociology and has shed much light upon the way in which stratification systems are initially established. However, conflict theories are much less effective in explaining exactly how stratification becomes legitimated; they offer concepts such as false consciousness and political formula but fail to describe the mechanisms through which the legitimation process operates (Marx and Engels, 1947).

Functional theory, which assumes the existence and internalization of a common value system among all strata of the population, has always purported to explain how an ongoing stratification system is legitimated. However, it has done so in a manner that is incompatible with the considerable achievements of conflict theory; for it has all but ignored the role of power and conflict. Moreover, it has assumed the inevitability of stratification and its legitimation and, therefore, precludes a priori the search for the conditions under which it might not exist (Gouldner, 1970).

The self-evaluation hypothesis is fully compatible with a conflict orientation in both its radical and elitist varieties. Like most conflict theories it, too, goes beyond the conception of stratified societies being held together exclusively by either force or the threat of force. However, it does more than simply allude to the role of ideology or ruling ideas in legitimating stratification. It describes legitimation as the outgrowth of social psychological processes operating within the confines of asymmetrical relationships embedded in major social institutions. A major strength of the self-evaluation hypothesis is that it can account for the coexistence of both legitimated inequality and a self-interested struggle over resources on the part of all strata in society.[13]

---

[12] In the Indian case, discontent most often took the form of a desire for upward mobility of the entire jati, not the lone individual or nuclear family. But acceptance of the caste system was in all cases implicit.

[13] This presents a grave problem for a theory such as Lenski's, for example. Lenski (1966:25–32) takes as one of his basic assumptions the idea that *man is essentially selfish*. However, given this assumption, he has difficulty explaining why most people who have been on the short end of grossly unequal social arrangements have, even so, accepted them. To solve this problem, Lenski advances the further assumption that man is a creature of habit and, for that reason, routinely accepts such unequal arrangements.

But he cannot have it both ways! *Which* individuals will behave selfishly or be slaves to habit at what points in time and under what conditions? In terms of

Unlike functionalism, the self-evaluation hypothesis does not *assume* the existence of a common value system. It achieves compatibility with functionalism by describing a process through which an ideology which justifies stratification comes to be commonly accepted by all strata—even the poor, in apparent contradiction to their self-interest. It also proposes an explanation of why this process *appears* to be inevitable and universal. In this sense, it takes seriously functionalism's insistence upon the existence of a common, objective standard of evaluation that is widely internalized (Kraus et al., 1978), in a way that conflict theory never really has been.

The self-evaluation hypothesis has also shown its ability to interpret a range of empirical data from both surveys and observational studies. While direct tests of the propositions stated here have not yet been made, their compatibility with so many theoretical orientations and existing findings suggests that they merit serious consideration.

One of their most promising features is their ability to explain why so many disadvantaged persons hold beliefs concerning stratification seemingly contrary to their "objective self-interest" while the views of the privileged seem to be more consistent with their interests (Cheal, 1979; Feagin, 1972).[14] Among the privileged, in terms of the propositions, self-evaluations coincide with self-interest. They tend to have high self-evaluations based upon their advantaged circumstances and success in dealing with the world. Such evaluations lead to their feelings that they *deserve* what they have. Among the disadvantaged, however, their self-evaluations are inconsistent with their interests. Having relatively low self-evaluations, they don't see themselves as *deserving* equality with those "better" than themselves (at least in terms of power and wealth). If the propositions are correct, the exceptions who are egalitarians ought to be precisely those individuals, among the disadvantaged, who have especially high self-evaluations (Feagin, 1972).

However, no claim is made that this is an exhaustive theory of mass support for inequality. As I pointed out from the start, factors other than self-evaluation are very likely to generate such support—even among people whose self-interest would seem to dictate support for equality. Only by taking all of these factors together can the maintenance of stratification in modern society be explained.

No discussion of legitimation would be complete without a consideration of the conditions under which stratification might be delegitimized.

Delegitimizing Stratification

It is important that we recognize the "dual nature" of the legitimation process.

---

legitimation, these two orientations are diametrically opposed.

The self-evaluation hypothesis resolves that problem by differentiating system levels at which legitimation (habit) and conflict (selfishness) orientation prevails. The theory states that: While low self-evaluation fosters acceptance of a lowly position (as incongruence between self-evaluation and self-interest leads to ambivalence) it also recognizes that people at all levels will react negatively to what they regard as threats to their self-interest.

[14] Cheal (1979) argues that Conservatives in Britain have a highly consistent ideology that favors the perpetuation of existing class relationships. Labourites, on the contrary, have a confused ideology that is in some ways opposed to existing arrangements but, also, in numerous ways supportive of them. *This,* Cheal (1979) argues, is a major reason why the disadvantaged have not been able to organize to alter, fundamentally, the existing system of class privilege (see also Schulman et al., 1976).

In terms of our hypothesis, the confusion in the ideology of the disadvantaged stems from the lack of coincidence between self-interest and self-evaluation among this segment of society.

It is Cheal's (1979) contention that the existence of a radical party is capable of reducing the confusion in the ideology of the disadvantaged. For him, as well as for others (Parkin, 1971; Mann, 1970), the radical party is able to provide cognitive structures that point out the linkages between the conditions of their lives and the principles upon which the present system is organized, linkages that provide people with a version of an alternative social order. While not taking issue with this approach, I suggest that the party provides another equally vital function—namely, to raise the self-evaluations of the disadvantaged. Marxist parties, for example, do this by promulgating the idea that the working class has a pivotal role in history, no less than that of ending oppression itself. The party can also make itself a powerful source of positive self-evaluation by actually being successful in wringing concessions from the system.

On the one hand, as I have argued repeatedly, it is self-reinforcing. Any given distribution of primary resources has a strong tendency to produce a congruent distribution of self-evaluations through a set of very basic social psychological mechanisms. On the other hand, the process is not totally automatic. In a thousand little ways it must be maintained and reconstructed, anew, on a daily basis (Berger and Luckmann, 1966).

Maintenance of legitimation involves the staging process in which social distance must be maintained and people must be duly impressed. The generalized conception (within the culture of elite superiority) must be reinforced directly through the mass media; control of subordinates and manipulation of their sense of self-worth must be continued within major social institutions through the exercise of hierarchical authority; channels of upward mobility must be kept open to the most talented members of subordinate strata.

While isolated cases of failure to maintain any of these facets of the system are unlikely to be fatal, a large-scale pattern of failure could initiate a chain of events which, under appropriate conditions, might undermine the legitimation of stratification. Since, theoretically, the delegitimation of stratification is possible, under what conditions might it occur?

I have hypothesized that delegitimation comes about when incongruence develops between the distribution of primary resources and that of self-evaluations. Though there are many logically possible ways in which incongruence may develop, I will focus on only a few here. In the first instance, the evaluation of the elite is lowered in the eyes of the populace so that, for most, the gap between the evaluation of self and that of the highly rewarded is narrowed, without any concomitant narrowing of the gap in terms of resources. In this case it is also possible that highly rewarded persons, reacting to the incongruity, would lower their evaluations of themselves to a point at which they feel overrewarded and experience a "loss of nerve."

One way in which such an eventuality might come about is through a failure of the dominant stratum to continue to keep the economy running smoothly and/or to fail to maintain order (Mankoff, 1970; Gurr, 1970). In either case, an objective failure on the part of the regime would weaken the ability of leaders to present themselves as possessing superior competence.[15]

In the second instance, the actual extent of inequality of resources is suddenly shown to be much greater than was previously suspected. This could occur either through changing ecological patterns that force rich and poor to observe at close hand the way each other lives, or through media exposure.

While both of these instances result in incongruity, it is of a negative sort—brought on by disenchantment with superiors. Elites, as leadership groups, are seen as those who are needed to perform society's most vital functions and make its most important decisions. Negative reactions to elites, with no concomitant rise in the self-evaluations of the masses, are very likely to simply initiate a search for more worthy elites to take the place of the fallen ones. Stratification still remains.

This leads to the third instance. Here, incongruity is generated through a rise in the self-evaluations of the disadvantaged. As a result, the privileged no longer seem especially worthy by comparison. In this instance, the disadvantaged come to feel more confident about their ability to shape their own destiny in the larger world.[16]

---

[15] None of this is meant to imply that a breakdown of the authority of elites in any society necessarily leads to radical change in that society's structure, let alone to an egalitarian society. Clearly it does not! On the contrary, it should be very clear at this point in our discussion that an egalitarian society might emerge only under a very carefully specified set of conditions that are extremely difficult to satisfy.

Sabotage and terrorism are tactics designed to precipitate such a breakdown. So, too, is the general strike. But for most people, disgruntled as they might be, the prospect of such drastic, dangerous, and illegal action inhibits them (Wilson, 1973). This, of course, makes reasonably easy the isolation of extremist groups who use such tactics, unless the state itself is already very weak or unless mass opposition is extremely strong, widespread, and well organized (Balbus, 1973). Another possibility would be a sustained exposure of elite misdeeds and/or incompetence through vigorous investigative reporting.

[16] There are a number of possibilities. First, there is the spread of literacy and, then, of higher educa-

While my main concern has been class stratification, one might argue that a hypothesis as general as the one advanced here should be applicable to other forms of stratification, as well, such as racial or sexual. But these forms of stratification involve only the first system level of abstraction, the placement of particular categories of individuals within a given stratification system. They can, theoretically, be eliminated while the society remains class-stratified—that is, members of previously subordinate racial groups, or women, can have an equal chance of high or low placement, with inequality itself being firmly entrenched. This would be the prototypical meritocracy. Thus, we should expect a much higher probability of conflict and a much lower level of consensus over the legitimacy of these forms of stratification than over the very idea of stratification itself. Yet it might be fruitful to explore the possible relevance of the relationship between the distribution of resources among racial groups and their self-evaluations, particularly the effect of suddenly elevated levels of minority self-evaluations or the high self-evaluations of members of minority elites whose opportunities for advancement are blocked. But these issues are much too extensive to be treated here and must be relegated to another study.

The self-evaluation hypothesis overcomes the static bias that afflicts not only functionalism but also the elitist types of conflict theory that treat stratification and its legitimation as being inevitable. By taking a dialectical approach (Ball, 1979) it opens up the very question of the conditions under which these two persistent characteristics of social life are likely to be present in varying degrees or, even, entirely absent.

REFERENCES

Alinsky, S.
1971 Rules for Radicals: A Practical Primer for Realistic Radicals. New York: Random House.
Alves, W. M., and P. H. Rossi
1978 "Who should get what? Fairness judgements of the distribution of earnings." American Journal of Sociology 84:541–63.
Anderson, P.
1977 "The antinomies of Antonio Gramsci." New Left Review 100:5–78.
Bakke, E. W.
1940 Citizens without Work. New Haven: Yale University Press.
Balbus, I.
1973 The Dialectics of Legal Repression. New York: Russell Sage.
Ball, R. A.
1979 "The dialectical method: its application to social theory." Social Forces 57:785–98.
Baltzell, E. D.
1958 Philadelphia Gentlemen: The Making of a National Upper Class. New York: Free Press.
Bem, D. J.
1967 "Self-perception: an alternative interpretation of cognitive dissonance phenomena." Psychological Review 74:183–200.

---

tion. When the spread of such education exceeds the availability of jobs characterized by suitable status, autonomy, intrinsic challenge, and money, incongruence can develop. Perhaps the increasingly pronounced stratification of higher education (Bowles and Gintis, 1976; Karabel, 1972), competency testing, and other efforts having the effect of reducing the extent of "status inflation" through education (Milner, 1972) may have the effect of heading off this potential threat. In fact, there is evidence that the college enrollment rates, at least for whites, are declining (U.S. Bureau of the Census, 1979; 1971), thereby reducing this pressure. Nevertheless, if the self-evaluation hypothesis is valid, one cannot neglect the role of the spread of literacy as a force favoring greater equality (Lenski, 1966). Neither can one neglect the importance of the extension of citizenship rights to the masses (Anderson, 1977; Marshall, 1964). By placing additional political resources, however formalistic, in the hands of the people, one is likely to raise their levels of self-evaluation. Given the importance of social institutions in developing people's self-evaluations, it would seem that any move toward the democratization of these institutions—be they schools, workplaces, or families—would tend to create greater equality of self-evaluations, as people would actually experience greater control over, and responsibility for, their own lives.

Still another possibility of raising the self-evaluations of the masses is through getting people involved in some sort of organized activity in which success can be demonstrably achieved (Alinsky, 1971; Freire, 1970). As students of revolution have long recognized, it is those groups actually making gains who are emboldened to press further demands (Brinton, 1938). My introductory review, herein, of Marx's work showed that he was keenly aware of the central role of struggle in raising consciousness. More recently, Anderson (1977) has argued that massive support for socialism in Western democracies can come about only through the actual successes of working-class-dominated parties within those democratic structures. In the absence of successful involvement, one is faced with the formidable task of convincing people of their own worth without having compelling, objective back-up evidence. This, indeed, is the key problem.

Bendix, R.
1956 Work and Authority in Industry: Ideologies of Management in the Course of Industrialization. New York: Wiley.

Berger, J., M. Zelditch, B. Anderson, and B. P. Cohen
1972 "Structural aspects of distributive justice: a status value formulation." In J. Berger, M. Zelditch, and B. Anderson (eds.) Sociological Theories in Progress, Vol. 2. Boston: Houghton Mifflin.

Berger, P. L., and T. Luckmann
1966 The Social Construction of Reality: A Treatise in the Sociology of Knowledge. Garden City: Doubleday.

Blau, P. M.
1964 Exchange and Power in Social Life. New York: Wiley.

Blumberg, P.
1968 Industrial Democracy: The Sociology of Participation. London: Constable.

Bowles, S., and H. Gintis
1976 Schooling in Capitalist America. New York: Basic Books.

Brinton, C.
1938 The Anatomy of Revolution. New York: Random House.

Brookover, W. B., and E. L. Erickson
1975 Sociology of Education (Rev. ed.). Homewood: Dorsey.

Brookover, W. B., T. Shailer, and A. Paterson
1964 "Self-concept of ability and school achievement." Sociology of Education 37:271–8.

Chambliss, W.
1973 "The saints and the roughnecks." Society 11:24–31.

Cheal, D. J.
1979 "Hegemony, ideology, and contradictory consciousness." Sociological Quarterly 20:109–17.

Chinoy, E.
1955 Automobile Workers and the American Dream. Boston: Beacon.

Clark, K. B.
1965 Dark Ghetto. New York: Harper and Row.

Cook, R.
1975 "Expectations, evaluations, and equity." American Sociological Review 40:372–88.

Coopersmith, S.
1967 The Antecedents of Self-esteem. San Francisco: Freeman.

Coser, L.
1956 The Functions of Social Conflict. New York: Free Press.

Dahrendorf, R.
1959 Class and Class Conflict in Industrial Society. Stanford: Stanford University Press.

Dahl, R. A.
1967 Pluralist Democracy in America. Chicago: Rand-McNally.

Davis, K.
1948 Human Society. New York: Macmillan.

Davis, K., and W. E. Moore
1945 "Some principles of stratification." American Sociological Review 10:242–9.

Della Fave, L. R.
1974 "On the structure of egalitarianism." Social Problems 22:199–213.

Domhoff, G. W.
1971 The Higher Circles. New York: Vintage.
1967 Who Rules America? Englewood Cliffs: Prentice-Hall.

Dumont, L.
1970 Homo Hierarchicus: An Essay on the Caste System. Chicago: University of Chicago Press.

Emerson, R.
1962 "Power-dependence relations." American Sociological Review 27:31–41.

Feagin, J.
1972 "When it comes to poverty, it's still, 'God helps those who help themselves.'" Psychology Today 6:101–29.

Festinger, L.
1957 A Theory of Cognitive Dissonance. Evanston: Row-Peterson.

Form, W. H.
1976 Blue Collar Stratification. Princeton: Princeton University Press.

Fox, W., D. Payne, T. Priest, and W. Philiber
1977 "Authority position, legitimacy of authority structure, and acquiescence to authority." Social Forces 55:966–73.

Freire, P.
1970 Pedagogy of the Oppressed. New York: Seabury.

Giddens, A.
1973 The Class Structure of the Advanced Societies. London: Hutchinson.

Goffman, E.
1959 The Presentation of Self in Everyday Life. Garden City: Doubleday.

Gouldner, A. W.
1970 The Coming Crisis of Western Sociology. New York: Avon.

Gurr, T. R.
1970 Why Men Rebel. Princeton: Princeton University Press.

Habermas, J.
1973 Legitimation Crisis. Boston: Beacon.

Hall, P. M., and J. P. Hewitt
1970 "The quasi theory of communication and the management of dissent." Social Problems 18:17–26.

Harrington, M.
1976 The Twilight of Capitalism. New York: Simon and Schuster.

Heider, F.
1958 The Psychology of Interpersonal Relations. New York: Wiley.

Homans, G. C.
1974 Social Behavior: Its Elementary Forms. (Rev. ed.) New York: Harcourt, Brace, Jovanovich.
1961 Social Behavior: Its Elementary Forms. New York: Harcourt, Brace, and World.

Jacques, J., and K. Chason
1977 "Self-esteem and low status groups." The Sociological Quarterly 18:399–412.

Jasso, G., and P H. Rossi
1977 "Distributive justice and earned income." American Sociological Review 42:639–51.

Johnson, W., R. Petersen, and E. Wells
1977 "Arrest probabilities for marijuana users as indicators of selective law enforcement." American Journal of Sociology 83:681–99.

Karabel, J.
1972 "Community colleges and social stratification." Harvard Educational Review 42:521–62.

Keller, S.
1963 Beyond the Ruling Class: Strategic Elites in Modern Society. New York: Random House.

Kerbo, H.
1975 Paradigms of Social Stratification: The Contemporary Power Elite Debate. Unpublished Ph.D. dissertation: Virginia Polytechnic Institute and State University.

Kohn, M. L.
1969 Class and Conformity. Homewood: Dorsey.

Kraus, V., E. Schild, and R. Hodge
1978 "Occupational prestige in the collective conscience." Social Forces 56:900–18.

Lane, R. E.
1962 Political Ideology: Why the American Common Man Believes As He Does. New York: Free Press.

Lenin, V. I.
1963 What Is to Be Done? New York: Oxford University Press.

Lenski, G.
1966 Power and Privilege. New York: McGraw-Hill.

Lerner, M. J.
1975 "The justice motive in social behavior: introduction." Journal of Social Issues 31:1–20.

Leventhal, G.
1975 "The distribution of rewards and resources in groups and organizations." In L. Berkowitz and E. Walster (ed.) Advances in Experimental Social Psychology (Vol. 9). New York: Academic Press.

Leventhal, G. S., J. W. Michaels, and C. Sanford
1972 "Inequity and interpersonal conflict: reward allocation and secrecy about rewards as methods of preventing conflict." Journal of Personality and Social Psychology 23:88–102.

Liebow, E.
1967 Tally's Corner. Boston: Little, Brown.

Lopreato, J., and L. Hazelrigg
1972 Class, Conflict, and Mobility. San Francisco: Chandler.

Mandelbaum, D. G.
1968 "Status seeking in Indian villages." Trans-Action 15:48–52.

Mankoff, M.
1970 "Power in advanced capitalist society: a review essay on recent elitist and Marxist criticism of pluralist theory." Social Problems 17:418–29.

Mann, M.
1970 "The social cohesion of liberal democracy." American Sociological Review 35:423–39.

Marshall, T. H.
1964 Citizenship and Social Class. Garden City: Doubleday.

Marx, K.
1963 The Poverty of Philosophy. New York: International Publishers.
1959 "Critique of the Gotha program." In L. S. Feuer (ed.) Marx and Engels: Basic Writings on Politics and Philosophy. Garden City: Doubleday.
1906 Capital (Vol. 1). New York: Modern Library.

Marx, K., and F. Engels
1959 "Manifesto of the Communist party." In L. S. Feuer (ed.) Marx and Engels: Basic Writings on Politics and Philosophy. Garden City: Doubleday.
1947 The German Ideology. New York: International Publishers.

Mead, G. H.
1964 G. H. Mead: Selected Writings. Andrew J. Reck (ed.) Indianapolis: Bobbs-Merrill.
1934 Mind, Self, and Society. Chicago: University of Chicago Press.

Meisel, J. H.
1965 Pareto and Mosca. Englewood Cliffs: Prentice-Hall.

Miliband, R.
1969 The State in Capitalist Society. New York: Basic Books.

Mills, C. W.
1956 The Power Elite. New York: Oxford University Press.

Milner, M.
1972 The Illusion of Equality. San Francisco: Jossey-Bass.

Moore, B.
1978 Injustice: The Social Bases of Obedience and Revolt. White Plains: Sharpe.

Mosca, G.
1939 The Ruling Class. New York: McGraw-Hill.

Ofshe, L. and R. Ofshe
1970 Utility and Choice in Social Interaction. Englewood Cliffs: Prentice-Hall.

Pareto, V.
1935 The Mind and Society: A Treatise on General Sociology. New York: Harcourt, Brace.

Parkin, F.
1971 Class Inequality and Political Order. New York: Praeger.

Parsons, T.
1953 "A revised analytical approach to the theory of social stratification." In R. Bendix and S. M. Lipset (eds.) Class, Status, and Power. (1st ed.) New York: Free Press.
1951 The Social System. New York: Free Press.
1949 "An Analytical approach to the theory of social stratification." In T. Parsons (ed.) Essays in Sociological Theory. New York: Free Press.

Prewitt, K. and A. Stone
1973 The Ruling Elites. New York: Harper and Row.

Rainwater, L.
1974 What Money Buys: Inequality and the So-

cial Meanings of Income. New York: Basic Books.

Rawls, J.
1971 A Theory of Justice. Cambridge: Harvard University Press.

Rosenbaum, J.
1975 "The stratification of socialization processes," American Sociological Review 40:48–55.

Rosenberg, M.
1965 Society and the Adolescent Self-Image. Princeton: Princeton University Press.

Rosenberg, M., and L. I. Pearlin
1978 "Social class and self-esteem among children and adults." American Journal of Sociology 84:53–77.

Rosenthal, R., and L. Jacobson
1968 Pygmalion in the Classroom: Teacher Expectation and Pupil's Intellectual Development. New York: Holt, Rinehart, and Winston.

Runciman, W. G.
1966 Relative Deprivation and Social Justice. Baltimore: Penguin Books.

Rushing, W.
1972 Class, Culture, and Alienation. Reading, Mass.: D. C. Heath.

Sallach, D.
1974 "Class domination and ideological hegemony." The Sociological Quarterly 15:39–50.

Sampson, E. E.
1975 "On justice as equality." Journal of Social Issues 31:45–64.

Schulman, M. D., F. H. Buttel, and W. L. Flinn
1976 "The social cohesion of liberal democracy revisited: class differences in value consensus and dissensus." Paper presented at the annual meeting of the Midwest Sociological Society, St. Louis.

Schumpeter, J.
1951 Imperialism and Social Classes. New York: Meridian.

Schwartz, B.
1974 "Waiting, exchange, and power: the distribution of time in social system." American Journal of Sociology 79:841–70.

Sennett, R., and J. Cobb
1972 The Hidden Injuries of Class. New York: Vintage.

Shibutani, T., and K. Kwan
1965 Ethnic Stratification. New York: Macmillan.

Strasser, H.
1976 The Normative Structure of Sociology. London: Routledge and Kegan Paul.

U.S. Bureau of Census
1979 Current Population Reports. Series P-20, No. 346.
1971 Current Population Reports. Series P-20, No. 236.

Walster, E., and G. W. Walster
1975 "Equity and social justice." Journal of Social Issues 31:21–44.

Webster, M. J., and J. E. Driskell
1978 "Status generalization: a review of some new data." American Sociological Review 43:220–36.

Webster, M., and B. Sobieszek
1974 Sources of Self-Evaluation: A Formal Theory of Significant Others and Social Influence. New York: Wiley.

Wells, L. E., and G. Marwell
1976 Self-Esteem: Its Conceptualization and Measurement. Beverly Hills: Sage.

Whyte, W. H.
1957 The Organization Man. Garden City: Doubleday.

Wilson, W. J.
1973 Power, Racism, and Privilege. New York: Macmillan.

Wright, J.
1976 The Dissent of the Governed: Alienation and Democracy in America. New York: Academic Press.

RESEARCH NOTES

SUBURBAN STATUS INSTABILITY*

HARVEY M. CHOLDIN, CLAUDINE HANSON, AND ROBERT BOHRER
University of Illinois, Urbana–Champaign

American Sociological Review 1980, Vol. 45 (December):972–983

Contrary to conventional urban theory, recent studies have found "suburban persistence" in social status, over time. This concept is challenged here, in an examination of change in relative status of places. Three simple indexes of relative status are introduced and analyzed over time in a cohort analysis. It is shown that status typically rises for about two decades after a suburb is built and then it declines. Thus, like city areas, suburbs follow the neighborhood life cycle. Earlier studies which found suburban persistence are criticized for using panel designs in combination with correlational statistics, which were unable to detect the kind of change occurring in metropolitan areas.

Two recent studies of the social status of suburban communities conclude that such places tend to be stable over time, that "suburban persistence" occurs in United States metropolitan areas. In the first study, Farley (1964:38) observed, "Particular suburbs retain their peculiar socioeconomic characteristics for long periods of time and even rapid population growth does not produce substantial changes in a suburb's socio-economic characteristics." Likewise, Guest (1978:262) concluded: "Amid the massive suburban growth of post-World War II America, most suburbs retained a very high stability of relative ranking in social status." These findings are inconsistent with previous theories of urban development, which state that residential areas tend to change and decline over time.[1]

A historical perspective on urban development, however, suggests that suburbs will change over time if they behave like previous types of high status residential areas. Certainly high-status places have changed in earlier periods. For example, a series of Chicago maps showing the locations of elite residential areas (Hoyt, 1939:83) showed that the upper class had shifted its location every 30 years. Indeed, once-elite areas later became slums. Why should sociologists presume that the current period is frozen and that local status in the metropolis is stable, knowing that status patterns have shifted and changed throughout United States urban history?

Unlike those who now see stability, previous urban theorists—viewing city neighborhoods rather than suburbs—emphasized evolution and change of residential areas. Perhaps the earliest sociological theory to this effect was "residential succession" (Burgess, 1925; 1928), which explained how a sequence of groups occupied a given city location. As a group rose in status it was likely to abandon its environment which would then be taken by new occupants of slightly lower status. "Filtering," the process by which residences are passed down to less prosperous occupants, is another concept of residential change (Lowry, 1960). A third expression of the general concept of

---

* Direct all correspondence to: Harvey M. Choldin; Department of Sociology; Lincoln Hall; University of Illinois; Urbana, IL 61801.

We are grateful to the Research Board and the Center for Advanced Study of the University of Illinois at Urbana-Champaign for generously supporting this research. Bohrer's participation in this project was supported by the National Science Foundation under grant MCS79-02581. Alan Booth, Joan Huber, James Klugel, and Daphne Spain helpfully criticized an earlier draft of this report.

[1] Other sociologists (Logan, 1976; 1978; Stahura, 1979a; 1979b; Collver and Semyonov, 1979; Marshall and Stahura, forthcoming) have recently studied suburban status with varying interpretations of change.

subcommunity change is the "neighborhood life cycle" (Hoover and Vernon, 1962:183–98), which posits a series of stages in which residential areas start as new housing developments, mature, and decline into old, run-down, undesirable environments. All three of these concepts—succession, filtering, and neighborhood life cycle—consistently portray downward change, not stability, among residential areas.

Recent suburban trends imply that local socioeconomic status may not be stable. The oldest suburbs, built in the first decades of the twentieth century, now have old housing, noncompetitive business districts, and concomitant local problems. The suburban zone is absorbing many central city functions such as apartment housing and various types of employment (Zimmer, 1975). These change processes call for an increased understanding of recent developments in social status among suburbs.

The question of suburban stability versus change and decline has ramifications in urban sociological theory as well as in public policy. If suburbs do not change while their central cities do, this shows that the metropolis is not simply an expansion of the large city as previous theories stated (Banfield, 1970). Thus, the older theories of community growth and change are undermined because suburban stability equals nonchange in a major segment of the metropolis. In the policy realm, stability implies that the suburban belt will not suffer the long-run problems attendant upon status decline. Presumably central cities could continue to age and deteriorate and, perhaps, to become slums, while suburban persistence would guarantee a rosy future outside the city.

We suspect that recent studies found status persistence among suburbs because they employed deficient research designs. The first two suburban stability studies were based on panels of suburbs which appeared in the 1920 census (Farley, 1964; Guest, 1978). Using regression and correlation, Farley examined the bivariate relationship between high school enrollment in 1920 and adult educational attainment in 1960. Guest developed multivariate regression equations to examine the relationship between 1920 high school enrollment with 1950 adult educational attainment and the relationship between the 1950 variable and its 1970 counterpart. In both studies the educational indicators were used to represent community social status. The test of stability in these studies was the magnitude of the correlation coefficient between the status measurements at any two time points. The authors interpreted high correlations between scores at different times as representing status stability. The panel design excluded from consideration the massive suburban growth of recent decades. The panel design could not incorporate the growing new suburbs, including many high-status ones. Neither could it reflect the differential growth of new and old suburbs. Possibly the entire panel of 1920 suburbs was declining gradually in status, leaving room for new suburbs in the upper echelons. Thus, a better research design would consider population growth of existing places and include new suburbs.

The use of correlation coefficients within a panel also had a distorting effect in the Farley and Guest studies. Correlation does not offer the most revealing way to study suburban status over time. In a panel design the correlation coefficient cannot reveal upward or downward changes of the entire distribution. A downward trend of all the suburbs would be the same as subtracting a constant, and if the ranks were unchanged the correlation over time would be high. Two recent studies (Collver and Semyonov, 1979; Stahura, 1979b) introduced more complex regression techniques but, staying within panel designs, were subject to the same constraints as the earlier investigations. Furthermore, since status changes among subcommunities are likely to be small and gradual, a more sensitive measurement technique is required to detect them.

Another methodological deficiency in the literature is that city areas and suburbs are almost never studied together, even though the metropolitan community as a whole is a single status field. The social status of suburbs is related to the status of places in their central cities as well as to that of other suburbs.

The study which follows was designed

to test the hypothesis that the neighborhood life-cycle principle applies to suburbs as well as to city neighborhoods. We examined change in social status among suburbs in metropolitan Chicago, from 1940 to 1970. We devised an index of relative status for each census year and applied it in a cohort analysis to compare young and old suburbs. The index reflects changing city status, differential suburban growth, and the addition of new suburbs. The simple index, which can be presented graphically, reveals status change for individual places within the metropolis. The results show that status levels among subcommunities change constantly, although not necessarily rapidly, and that suburbs do pass through the neighborhood life cycle.

METHODS

An Index of Relative Status

Because social status among residential areas is relative it is best measured relatively. Individual members of the population of a metropolitan area have statuses and their localities have statuses based on the characteristics of their residents. These locality statuses are recognized within the population at large, as demonstrated by Felson's (1978) survey which showed that most individuals are able to rate suburbs in their own metropolitan area. This aspect of the suburban zone has long been recognized in sociological writings, certainly as far back as Whyte (1956), who noted that the young corporate employees in Park Forest aspired to more prestigious suburbs as they rose in their organizations.

By measuring status relatively, the analyst can make valid comparisons over time. Even if a place rises absolutely, say in educational attainment, if the general level of educations rises more, that place will fall in relative status. If a place stays constant in absolute status, it also falls relatively. A relative ranking also makes it possible to define cutting points consistently for strata or classes. For example, one might define the middle stratum as the middle third or the central two quartiles at each point in time and then look at the locational pattern of that segment. It becomes possible in this way to compare the location of the middle stratum at two different points in time.

Furthermore, as the status of localities exists relative to the old places, it is also affected by the statuses of newly emerging suburbs and by the differential sizes of places. Thus, if several new high-status suburbs are built, this changes the rest of the distribution. Likewise, if new, large middle-range suburbs enter the system or grow substantially, this affects the status distribution. So, a good measure of the relative status of places should take into account all the localities in the metropolis, and their population sizes, at the time of measurement.

We have devised a relative status scoring technique which treats city and suburban residential areas on an equivalent basis and takes account of the growth of the entire metropolitan system. It measures change in relative socioeconomic status of individual subcommunities and reflects shifts in the social status profile of the entire metropolitan area. Being weighted by population, it takes account of the differing sizes of residential areas and yields a single percentile score for each place at each time. Finally, the technique takes account of the addition of new suburbs and the differential growth of subcommunities within the metropolitan community.

We introduce three measures of status rank for each place. They are based on educational attainment of the adult population, the percentage of white-collar workers, and the median income of families residing in each place. Educational attainment has been used widely in urban ecological studies as an indicator of subcommunity socioeconomic status (Schnore, 1964; Farley, 1964; Guest, 1978). Schnore (1964) consistently used educational attainment as a proxy for socioeconomic status in his major studies of city-suburban status differences. Family income and occupational level are also conventional indicators of subcommunity status (see, for example, studies by Anderson and Egeland, 1961; Guest, 1974; Hunter, 1974a; 1974b; Rees, 1970; Timms, 1971). In the table and graphs which follow, the score based on educational attainment is called EDRANK; the income score is called INRANK; and the occupational score is called OCRANK.

Scores are generated by a cumulative percentage distribution in which each subcommunity receives a number between 0 and 100 indicating the percentage of the metropolitan population living in subcommunities scoring no higher. The N subcommunities are sorted so that the place with the lowest absolute status score (educational attainment, percentage of white-collar workers, or median family income) appears first in a list. The place with the highest absolute status score ends the list. The rank assigned to the M^{th} lowest subcommunity is:

$$\text{rank}_M = 100 \times \frac{\sum_{i=1}^{M} \text{Population}_i}{\sum_{j=1}^{N} \text{Population}_j} \qquad (1)$$

where Population_i is the population of the i^{th} lowest subcommunity.[2] Because formula (1) is weighted by population, it generates a percentile score for each place representing the percentage of the metropolitan population living in areas of equal or lower status. The weighting of places by population is necessary in the study of suburban social status because of the widely unequal sizes of suburbs and the mathematical effects of these differences. In metropolitan Chicago, for example, there are several small elite suburbs. Obviously such a cluster of cases with extreme values, unweighted, would distort the calculation of a mean across all suburbs. Similarly, a group of outliers would distort the calculation of a correlation-regression statistic.

The intuitive clarity of a single percentile number to represent the social status of a place at a point in time may be shown in an example. In 1940, Oak Park had a median educational attainment of 12.0 years, giving it a relative status score of 89.6, meaning that 89.6% of the metropolitan population lived in subcommunities with lower scores. By 1970, its median educational attainment was 12.8 years, which, in a study of absolute status would have appeared as a rise. But, as the educational level of much of the metropolitan population was rising rapidly, and as there was differential gain in status among places, Oak Park's relative status score dropped to 84.5.

Relative status, as defined in formula (1), computed for each decade, has several advantages for measurement and conceptualization. The educational and income levels of the entire metropolis rose from 1940 to 1970. Thus, a neighborhood with an unchanging absolute level of educational attainment necessarily fell behind other places. Relative status also permits a clear, convenient way to view different levels within the overall metropolitan community. One may define strata in some consistent manner—by deciles, standard deviations from the mean, or whatever—and then monitor particular subcommunities or particular strata over time.

In sum, the entire metropolitan area (Urbanized Area) is treated as a single field over which social status is distributed. New suburbs are added to the data base and relative status scores are computed for all suburbs, each decade. Community areas[3] and suburbs in each census year are ranked from bottom to top, regardless of city or suburban location. Each subcommunity receives a unique score at each time, which, because of its construction, can be compared with other places or with itself at some other time.

Cohort Analysis

In addition to measuring status, it is necessary to incorporate *time* into the analysis in order to evaluate the neighborhood life cycle model. It is necessary to measure the statuses of subcommunities at different points in their own growth cycles. This is readily accomplished by examining status at each census year. But, in order to cover the life cycle completely, it would be necessary to follow the status of each place for the better part of a century after it was built. This task would be exceedingly difficult and so the researcher must employ a design which simulates this history. Cohort analysis of subcommunities facilitates the examination of groups of places at different points in their histories and permits comparisons of the changes occurring among young and old places.

---

[2] Actually, an approximation or rounding error is introduced implicitly here when we represent the entire subcommunity by its median score. This is eliminated in part by replacing Population_M by $\frac{1}{2}\text{Population}_M$ in the application of formula (1).

[3] A Community Area is a district within the City of Chicago which has defined boundaries and a name. During the 1920s, Burgess and his students at the University of Chicago examined the city looking for areas with social homogeneity, common history, and boundaries. They divided the city into 75 community areas; data published in the Local Area Fact Books conform to these areas.

Research Site

Viewing the metropolitan community as an ecological entity composed of central city plus suburbs (Schnore, 1967), we defined the scope of the study as the 1970 Chicago Urbanized Area. We treated city Community Areas and suburbs as equivalent units of analysis (subcommunities), following the procedure introduced by Rees (1970). The population of the Urbanized Area grew substantially during the three decades studied; and the number of suburbs increased, as well. Data for the first year, 1940, included all 75 community areas and 57 incorporated suburbs with populations of 2,500 or more. City subcommunities are Community Areas, as defined by University of Chicago sociologists in the 1920s (Wirth and Furez, 1938; Kitagawa and Taeuber, 1963; Hunter, 1974b). In 1970, the city is divided into 76 such areas[4] which, in turn, are composed of census tracts. While the number of community areas remained constant, the number of suburbs grew; we added smaller or younger suburbs at later points in time when they reached 2,500 population and appeared in the census. By 1970, the Urbanized Area included 147 suburbs, with an aggregate population[5] of 2,579,802.

The basic data source is the United States census which provides small area statistics at ten-year intervals. Equivalent status measurements for city tracts and suburban places are not available for periods before 1940, so the time frame for the study is 1940 to 1970. Demographic measurements—such as numbers of persons by age, sex, and race—are available for all periods since 1920 but socioeconomic status variables do not appear until the 1940 and 1950 censuses.

TWO REPLICATIONS

Correlation within a Panel

It should be noted that our data set and status indicator could have been used to make an argument for suburban stability, had we employed the same type of analysis as did Farley (1964) and Guest (1978). Among Chicago suburbs, considering only the 57 suburbs in the 1940 census, the Pearson's correlation between relative educational statuses in 1940 and 1950 is .95; between 1940 and 1960 is .90; and between 1940 and 1970 is .89. Within the same panel of suburbs, the correlations for occupational rank are .96, .92, and .85, respectively, and for income rank are .78 between 1950 and 1960, and .68 between 1950 and 1970. These coefficients are extraordinarily high and might easily lead to a stability interpretation. The coefficients are approximately equal to those reported by Farley and Guest for postwar 20-year intervals. Those sociologists interpreted the high correlations to mean that the statuses of suburbs are stable over time.

But the remainder of our analysis shows that this is not the correct interpretation because most places are declining most of the time and because there are several types of movement taking place.[6] The correlation method of the earlier studies showed only that, relative to each other, the suburbs in the panel—which had to be older ones—retained their relative rankings. Within the entire suburban system they were not necessarily stable. Their ranks were not compared to newer localities in those inquiries. Furthermore, the movement, such as peaking and falling, entailed in status careers, could not be detected by the correlational method.

---

[4] The 76th community area consists mostly of O'Hare International Airport. In 1970, there were less than 1,000 residents of this area. We have omitted it from our analysis.

[5] Aggregate population in 1970 consists of the total population of all 147 subcommunities in this study. It is, therefore, less than the total population of the Chicago SMSA or the Chicago Urbanized Area.

[6] By examining series of status measurements for individual suburbs over time, we detected six types of sequences. A place may be stable, rising, falling, peaking, recovering, or zigzagging. Peaking places rise initially, hit a high point, and fall thereafter. Recovering places fall initially, then begin to rise in status. Zigzagging places continually switch directions.

Decomposition within a Panel

Collver and Semyonov (1979) have suggested that suburban social status has three dimensions rather than just the one—education—analyzed by Farley (1964) and Guest (1978). Collver and Semyonov measured change in income and occupational level in addition to educational attainment and concluded that there are different patterns of stability and change for each. In particular, they emphasized differences between income changes and changes in the other two dimensions.

While we were able to reproduce their findings among Chicago suburbs using unranked data, we found that income conformed more closely to the general pattern than it did in Collver and Semyonov's (1979) analysis of 89 Long Island suburbs[7] (see Table 1). We surmise that income appeared to be an anomalous variable in Collver and Semyonov's study because their measure failed to accommodate the fact that generally rising income levels necessarily brought increased variance in income among places.

Table 1. Components of Status Changes among Chicago Suburbs, 1960–1970*

Components	Median Years of School	Percentage White Collar Workers	Median Family Income
Change of mean	.140	.018	.875
Change of dispersion	.014	.000	.104
Positional change	.010	.052	.155
Dispersion and positional change	−.016	−.001	.071
Total change	.237	.070	1.205
N=	121	120	121

* This table replicates Table 3 in Collver and Semyonov (1979:484).

A factor analysis of our three status indicators among the Chicago suburbs revealed a nearly perfect correspondence among them (see Table 2). Furthermore, as will be explained later, the patterns revealed in Figure 4 show that all three dimensions of status behave quite similarly over time by cohort. Like the New York calculations, these factor loadings do show a somewhat lesser connection of income to the main factor, but even income has a very high loading which increases during each decade and reaches .92 by 1970. Thus, we conclude that, even though there are slight differences among the status indicators, the coincidence among them is so great that they are interchangeable.

---

[7] More specifically, our use of the Collver and Semyonov (1979:480–6) equation was as follows. We calculated the 1960–1970 total change and its components for the 121 suburbs of Chicago (there were only 120 for percentage white-collar). The measures of status were: Median years of school completed for the adult population (25 years or older), the percentage of the male population greater than 14 years of age employed in white-collar occupations, and the median family income in 1959 and 1969, in constant dollars. Our analysis shows about half the amount of total change Collver and Semyonov found in 89 Long Island suburbs. The four components of change—which sum to the total change—are also about half for our measure of occupational distributions. Chicago suburbs show several important deviations from the Long Island pattern. Educational attainment showed greater change in dispersion in Chicago than on Long Island by a factor of about 20.

Change in the mean is by far the largest component of change in median family income for both Chicago and Long Island suburbs. However, in Chicago it constitutes a larger proportion of total change (73%, compared to 58% in Long Island). The change in dispersion is small (9%) for Chicago but sizable for Long Island (32%); and positional change is moderate in Long Island (12%) but small in Chicago (4%). This paints a picture of a greater trend toward shared property in Chicago's suburbs.

The greatest similarity between Long Island and Chicago suburbs can be seen in the distribution of education. Here, the Chicago data showed almost exactly the same pattern of components of change as Collver and Semyonov found, although the Chicago figures are about half as large as the Long Island calculations.

Table 2. Factor Analysis of Status Indicators for Chicago Suburbs for 1950, 1960, and 1970*

	Loadings on:		
	Status 1950	Status 1960	Status 1970
EDRANK	.953	.925	.969
OCRANK	.989	.974	.958
INRANK	.831	.896	.921

* There is no factor called "Status 1940" because we have only two rankings for that year. Data on family income was first available for 1950.

RESULTS

Relative Status

Figure 1 shows the distribution of the relative educational status scores of a sample of places in 1940. Each black dot represents a suburb and each dash a city district.

Figure 2 shows the relative status scores for all the localities in the Urbanized Area 30 years later. Note that the educational level of the entire population rose (the "curve" shifted right) and that 90 suburbs were added to the system. The score of one of the examples, Oak Lawn, rose by 10.9 points, while Elmhurst and Berwyn lost 2.1 and 22.5 points, respectively, by 1970.

A comparison between Figures 1 and 2 shows that city areas which were in the middle and upper ranks in 1940 had drifted downward by 1970. Although there were numerous middle-level and a few upper-level city places in 1940, they had all lost status by 1970. Almost all of the popula-

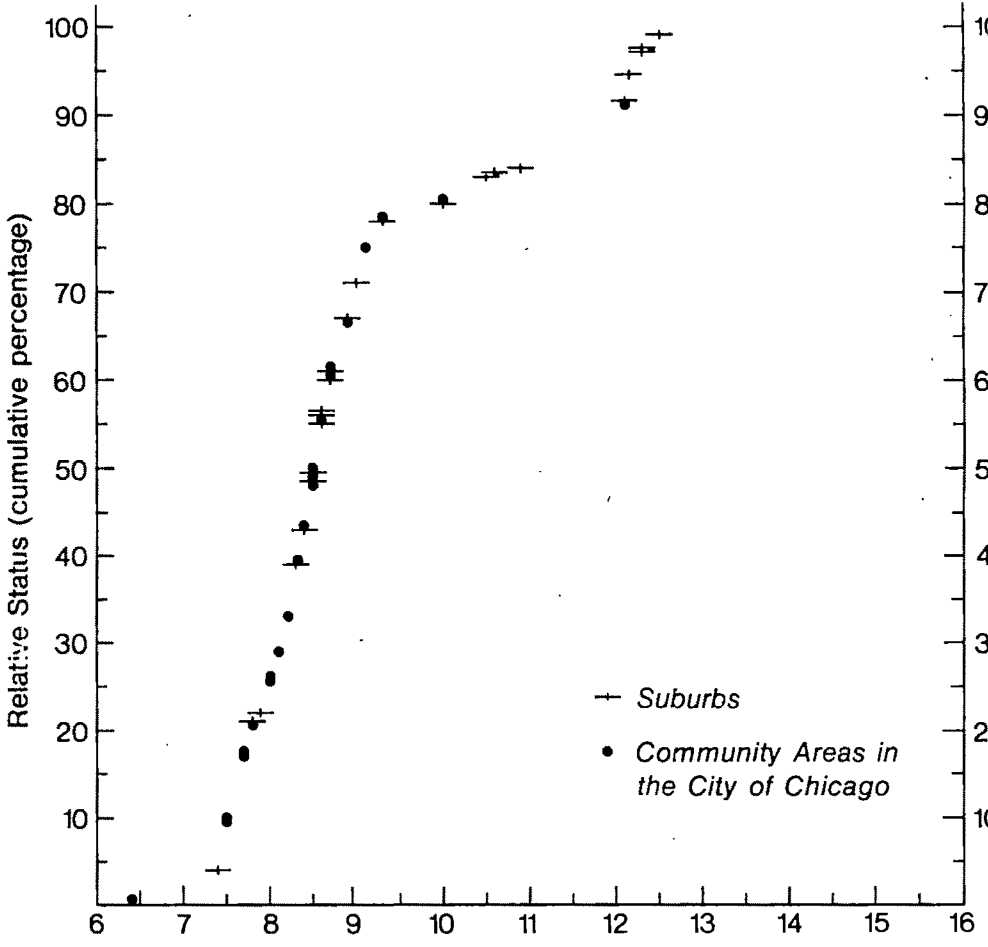


Figure 1. Educational Status Scores of a Sample of Chicago Area Subcommunities, 1940

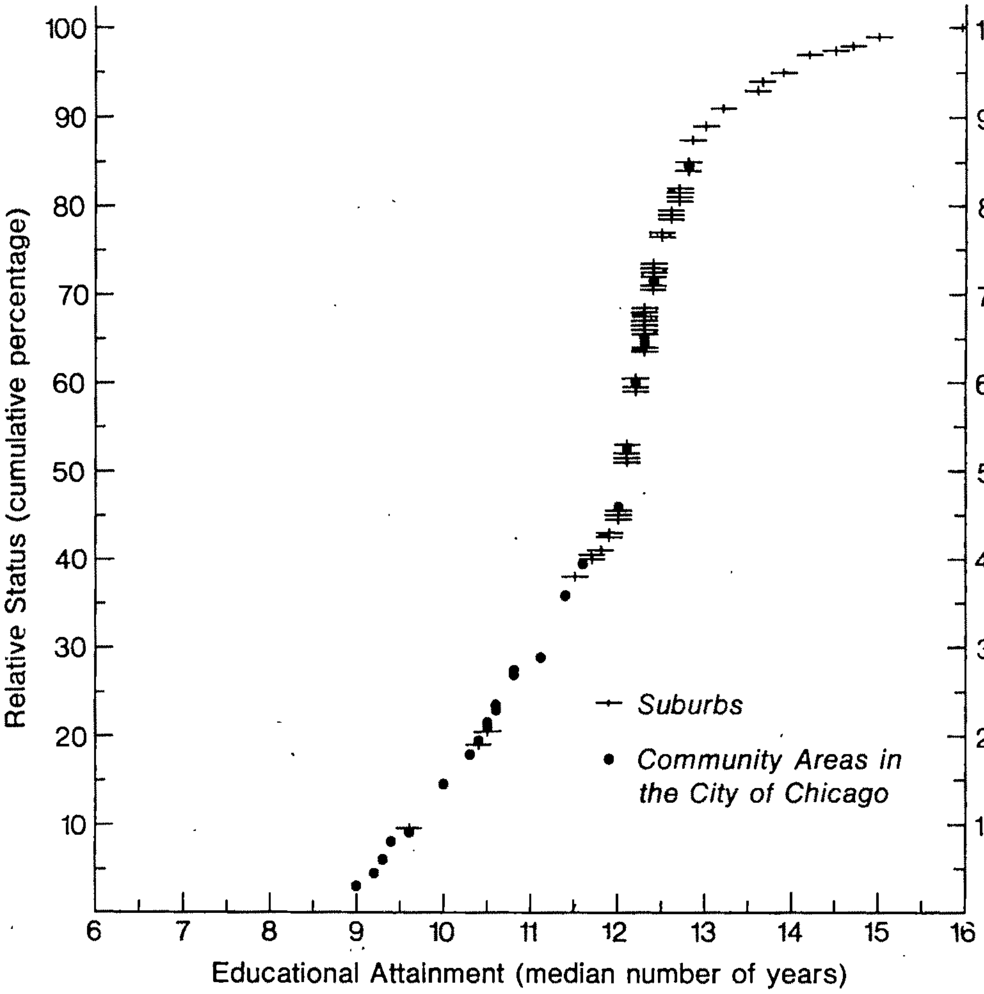


Figure 2. Educational Status Scores of a Sample of Chicago Area Subcommunities, 1970

tion of the upper strata and most of the middle strata lived in suburbs in 1970. In sum, Figure 2 shows that in 1970 most of the middle-strata places and all of the elite places in the Urbanized Area were suburbs.

Another way of seeing the status shift during this period is to compare the average scores for city areas and for suburbs in each census year. Table 3 shows that, in 1940, the average suburb had considerably higher status than the average city area. This was true for all status dimensions. This differential increased over the following decades as the city average dropped. The only exception was that suburban income status, already much higher than the city's, failed to rise in the 1960s.

Cohort Analysis

Placing the subcommunities in cohorts allows a different interpretation of the high status suburb and low status city area split. Sixty-three of the 75 city neighborhoods are in the cohorts born before 1940, while nearly all (48 of 57) of the suburbs were born after 1940. Plotting the mean weighted rank scores of the six cohorts of suburbs from 1940 to 1970 reveals the typical life cycle pattern of rise, peak, and decline (see Figure 3).

To conduct a cohort analysis, we first determined the time at which each subcommunity was built[8] and assigned each

---

[8] Duncan et al. (1962) and Guest (1973) use a criterion of two dwelling units per acre to indicate when a census tract was built. (This would have been a

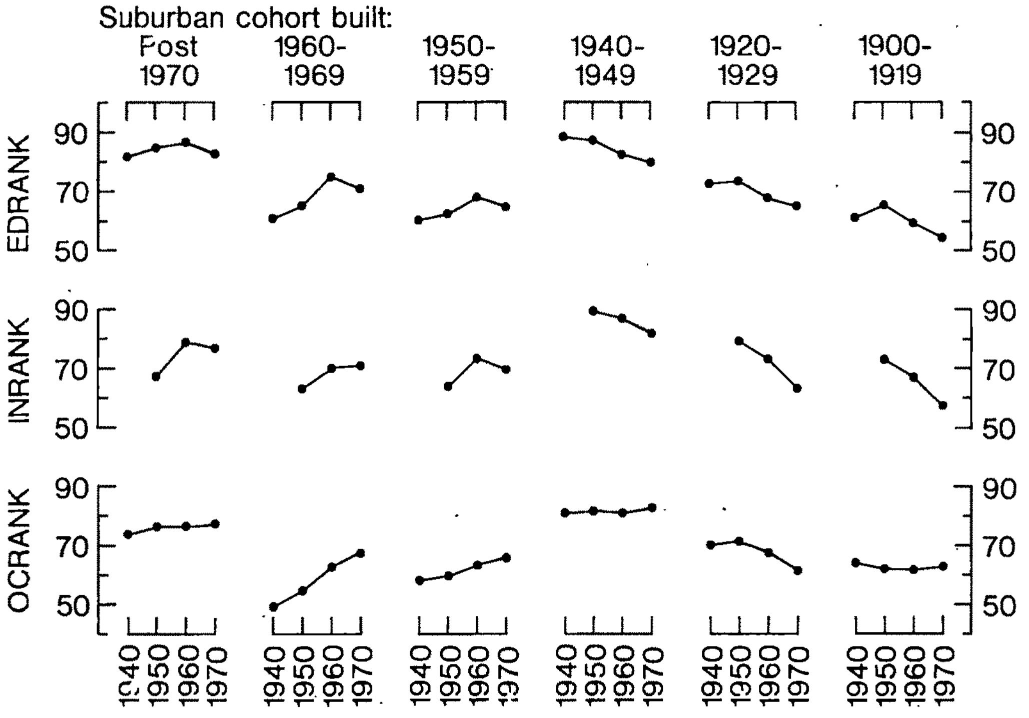


Figure 3. Mean Rank Scores among 6 Cohorts of Suburbs

place to a cohort, similar to a birth cohort in demographic analysis. We measured the relative status of each subcommunity at four census years and combined these scores into means for cohorts. We then followed the status changes of cohorts. This allowed us to compare the 30-year changes of old subcommunities with those of young subcommunities, to see both types behave as the life cycle model would predict. Our cohort analysis follows the procedure recommended by Guest (1974:230–1). There are six cohorts among the Chicago suburbs: those built before 1920, those built in the decades of the twenties, forties, fifties, sixties, and those which had not crossed the specified density threshold by 1970.

Table 3. Weighted Mean Rank Scores, Chicago Community Areas and Suburbs, 1940–1970

	EDRANK		OCRANK		INRANK	
	City Areas	Suburbs	City Areas	Suburbs	City Areas	Suburbs
1940	46.2	68.4	47.1	64.2	—	—
1950	44.6	70.6	46.0	65.3	44.3	70.2
1960	38.0	73.7	41.7	66.3	38.0	73.7
1970	33.9	70.8	35.7	68.9	32.7	72.4

Notes: Each entry represents the mean score for localities, either within the city or the suburban zone. The means were computed after the localities were weighted by their own populations. For example, the table shows that in 1960, the mean EDRANK for all city community areas was 38.0. The combined weighted mean rank score for city and suburban localities, pooled, is always 50.

There is no INRANK score 1940 because the census did not give family income then.

Suburbs of the two oldest cohorts, built prior to World War II, peaked in status in 1950 and declined gradually in each of the successive decades. They followed this pattern in each dimension of status: educational attainment, occupation, and income. Suburbs of the 1940s followed the same pattern as the prewar places except that their educational status peaked earlier.

---

dwelling-unit density of 1,280 per square mile.) We approximated their procedure with larger areal units, using a criterion of 1,200 dwelling units per square mile.

Cohort was coded by the last two digits of the year in which the place crossed the density threshold. For example, places built up between 1920 and 1930 are coded "20" and places born between 1950 and 1960 are coded "50." Areas built up in the nineteenth century are coded "01." This coding scheme means that young places have high scores and old places have low scores.

Table 4. Mean Weighted Rank Scores of Chicago Suburbs, 1940–1970

	Cohort built:						
	1900–1919	1920–1929	1930–1939	1940–1949	1950–1959	1960–1969	After 1970
EDRANK							
1940	61.7	72.3	—	88.3	60.3	61.6	81.5
1950	65.3	73.4	—	87.3	62.4	64.9	84.8
1960	59.7	67.8	—	82.2	67.9	75.2	86.7
1970	54.1	64.9	—	80.0	65.0	70.9	82.5
OCRANK							
1940	63.9	70.0	—	80.8	58.0	49.1	73.8
1950	62.0	70.8	—	81.9	59.8	54.4	76.2
1960	61.9	67.8	—	80.7	63.3	62.3	76.6
1970	62.3	61.6	—	82.6	65.8	67.5	77.4
INRANK							
1950	72.9	79.6	—	89.6	63.9	63.3	66.8
1960	66.9	73.1	—	86.7	73.4	70.3	79.2
1970	57.3	63.5	—	82.0	69.8	71.6	76.5
Number of suburbs:	2	7	0	4	15	14	15

Younger suburbs, on the other hand, rose in their early decades. Those built in the fifties and sixties tended to peak by 1960 and then to decline somewhat in educational and income status. They did, however, continue to rise in occupational status through the 1960s.

The overall pattern, over time, in each of the status dimensions, is that of the neighborhood life cycle. This can be seen by reading each of the horizontal panels in Figure 4 from left to right. Older cohorts of suburbs tend to decline in status while young cohorts of suburbs tend to rise.

DISCUSSION

We found that suburbs do change in status over time, typically moving gradually downward. This change is a function of the age of place. The general pattern of change resembles the neighborhood life cycle.

Methodologically, we have demonstrated a way to compute status scores for subcommunities, incorporating city districts and suburbs within a metropolitan area. These scores facilitate comparisons over time. We have used this technique to reveal changes in suburban status as the housing stock in the suburb ages.

Had we simply followed a panel of suburbs and correlated their absolute status scores over time as Farley and Guest did, we would have found high correlations, as they did. But the new scoring procedure clearly shows change in suburban status.

Thus, we reject the persistence-stability hypothesis in favor of the neighborhood life cycle. This conclusion is consistent with older sociological theories of subcommunity change. We conclude that the suburban analysis does not show status stability and that the great majority of suburbs is subject to the neighborhood life cycle. Suburban housing ages like city housing and, as this happens, the relative status of places declines. The only reason the life cycle has not become obvious outside the large cities is because most suburban environments are relatively young. Nonetheless, we are convinced that suburban communities are not immune to the ravages of time. Like other urban subcommunities, they must discover ways to renew themselves or give way to processes of decline. A casting-aside of the persistence-stability hypothesis allows us to look directly at the macroscopic urban community changes in progress and at their implications for urban theory.

As the metropolitan areas of the midwestern and northeastern United States enter a new era of nongrowth, the dominant models of urban growth and structure need major revisions, especially insofar as they attempt to explain change at the subcommunity level. For perhaps 20 years after World War II, urban population growth continued apace, as peripheral suburban growth overbalanced central-city losses. By 1970, though, many central cities had small net losses. Thus, the era of growth had ended for them. The succession and neighborhood life cycle models were predicated upon physical growth of the community so that as the occupants of a neighborhood rose in social status, they could move on to a newer, more desirable residential environment.

Now, however, there are developments which promise to curtail the construction of new peripheral residential areas. Proponents of the "no-growth" ideology often successfully prevented residential expansion (Frieden, 1975) and city planners generally oppose continued "suburban sprawl." The national constraints on energy consumption are also likely to prevent further suburbanization because of the high automobile usage in such areas. Therefore, metropolitan communities are likely to be spatially confined to their current areas.

If this occurs, the continued operation—on its old terms—of the neighborhood life cycle will be impossible and some alternative will emerge. This could take the form of a status freeze whereby areas are maintained at current levels, contrary to the historic pattern. The new model might include high-status redevelopment in old city centers, also contrary to the United States historic model. Or, it might result in some pattern which is simply not now anticipated.

REFERENCES

Anderson, Theodore R., and Janice Egeland
1961 "Spatial aspects of social area analysis." American Sociological Review 26:392–8.

Banfield, Edward C.
1970 The Unheavenly City. Boston: Little, Brown.

Burgess, Ernest W.
1925 "The growth of the city." Pp. 47–62 in R. E. Park, E. W. Burgess, and R. D. McKenzie (eds.), The City. Chicago: University of Chicago Press.
1928 "Residential segregation in American cities." Annals of the American Academy 140:105–15.

Collver, Andrew, and Moshe Semyonov
1979 "Suburban change and persistence." American Sociological Review 44:480–6.

Duncan, Beverly, Georges Sabagh, and Maurice D. VanArsdol
1962 "Patterns of city growth." American Journal of Sociology 68:418–29.

Farley, Reynolds
1964 "Suburban persistence." American Sociological Review 29:38–47.

Felson, Marcus
1978 "Invidious distinctions among cars, clothes and suburbs." Public Opinion Quarterly 42:49–58.

Frieden, Bernard
1975 The Environmental Protection Hustle, Cambridge: MIT Press.

Guest, A. M.
1973 "Urban growth and population densities." Demography 10:53–70.
1974 "Neighborhood life cycles and social status." Economic Geography 50:228–43.
1978 "Suburban social status: persistence or evolution?" American Sociological Review 43:251–64.

Hoover, E. M., and R. Vernon
1962 Anatomy of a Metropolis. Garden City: Anchor.

Hoyt, Homer
1939 The Structure and Growth of Residential Neighborhoods in American Cities. Washington, D.C.: Federal Housing Administration.

Hunter, Albert
1974a "Community change: a stochastic analysis of Chicago's local communities, 1930–60." American Journal of Sociology 79:923–47.
1974b Symbolic Communities: The Persistence and Change of Chicago's Local Communities. Chicago: University of Chicago Press.

Kitagawa, Evelyn M., and Karl E. Taeuber (eds.)
1963 Local Community Fact Book, Chicago Metropolitan Area, 1960. Chicago: University of Chicago, Chicago Community Inventory.

Logan, John
1976 "Industrialization and the stratification of cities in suburban regions." American Journal of Sociology 82:333–48.
1978 "Growth, politics, and the stratification of places." American Journal of Sociology 84:404–16.

Lowry, Ira
1960 "Filtering and housing standards: a conceptual analysis." Land Economics 36:362–79.

Marshall, Harvey, and John Stahura
Forthcoming "The impact of racial transition and composition on the status of American suburbs." Sociological Inquiry.

Rees, Philip H.
1970 "The factorial ecology of Chicago." Pp. 276–90 in Brian J. L. Berry and Frank E. Horton (eds.), Geographic Perspectives on Urban Systems. Englewood Cliffs: Prentice-Hall.

Schnore, Leo F.
1964 "Urban structure and suburban selectivity." Demography 1:164–76.
1967 "Community." Pp. 79–150 in Neil Smelser (ed.), Sociology. New York: Wiley.

Stahura, John
1979a "Structural determinants of suburban socioeconomic status." Sociology and Social Research 63:328–45.
1979b "Suburban status evolution/persistence: a

structural model." American Sociological Review 44:937–47.

Timms, Duncan
1971 The Urban Mosaic. London: Cambridge University Press.

U.S. Bureau of the Census
1942 Areas of the United States: 1940. Washington, D.C.: U.S. Government Printing Office.
1943a U.S. Census of Housing: 1940. Vol. 2. Washington, D.C.: U.S. Government Printing Office.
1943b U.S. Census of Population: 1940. Vol. 2. Washington, D.C.: U.S. Government Printing Office.
1953a U.S. Census of Housing: 1950. Vol. 1. Washington, D.C.: U.S. Government Printing Office.
1953b U.S. Census of Population: 1950. Vol. 2. Washington, D.C.: U.S. Government Printing Office.
1963a U.S. Census of Housing: 1960. Vol. 1. Washington, D.C.: U.S. Government Printing Office.
1963b U.S. Census of Population: 1960. Vol. 1. Washington, D.C.: U.S. Government Printing Office.
1972 U.S. Census of Housing: 1970. Vol. 1. Washington, D.C.: U.S. Government Printing Office.
1973 U.S. Census of Population: 1970. Vol. 1. Washington, D.C.: U.S. Government Printing Office.

Whyte, William H.
1956 The Organization Man. New York: Simon and Schuster.

Wirth, Louis, and Margaret Furez.
1938 Local Community Fact Book of Chicago. Chicago: University of Chicago Press, Chicago Community Inventory.

Zimmer, Basil
1975 "The urban centrifugal drift." Pp. 23–91 in A. Hawley and V. P. Rock (eds.), Metropolitan American in Contemporary Perspective. New York: Wiley.

GENERATING NEWSWORTHINESS: THE INTERPRETIVE CONSTRUCTION OF PUBLIC EVENTS*

MARILYN LESTER
University of Nevada

American Sociological Review 1980, Vol. 45 (December):984–994

Everyday life is not organized a priori as discrete public events which can be simply mirrored by newsworkers. It does not differentiate itself into newsworthy events for reporting and publication. News is a product of reality-making activities and not simply reality-describing ones. A critical sociological task is to examine newsworkers' transformations of the everyday world into published or broadcasted events-as-stories (Molotch and Lester, 1974; Tuchman, 1972; 1973a; 1973b; 1976; Glascow University News Group, 1972; Cohen, 1972; Cohen and Young, 1973). This paper details one core aspect of this process, the routines newsworkers use to identify and display the newsworthy character of occurrences and events.[1] Historically, newsworthiness has been viewed as a property of events in an external social order. Relying on primary and secondary data collected in several news organizations, an alternative conceptualization, consistent with basic tenets of ethnomethodology, is provided in this paper. Here, newsworthiness is an actively generated feature of events.

CASE MATERIALS

The principal data for this project were collected in 1972 while I was a participant observer at a large national and international newspaper, referred to as "*The Interpreter*." Its owner is an international organization, referred to as "Organization," which is based in a major metropolitan area.

The major components of *The Interpreter* are American News and International News. Each has an Editor and support staff in the newsroom while subordinate Bureau Chiefs and reporters are geographically dispersed throughout the world. The local news bureau, here called "Regional Bureau," is officially part of American News.[2] In conjunction with great stipulations, I was assigned to Regional Bureau as an intern reporter. Apart from the practicality of focusing data collection there, it was observationally strategic in that it was the only bureau with a full complement of staff in the newsroom. Additional sources of data at *The Interpreter* included: the American News desk where I spent two weeks; outside the newsroom, tracking camera crews and both City Hall and Suburbs reporters on their work routines; the City in general—covering stories, conversing with news sources and newsworkers from other media; and attending official meetings of the whole Organization staff.

While there was variation in the type of materials collected at *The Interpreter*, a single case is insufficient for developing durable theory and specifying the conditions under which various features of a process occur and/or assume different forms (Glaser and Strauss, 1967; Bigus et al., 1978). Therefore, in the summer of 1974, further observations were collected in seven media organizations in a midwestern city: the only daily newspaper,

---

* Direct all correspondence to: Marilyn Lester; Department of Sociology; University of Nevada; 4505 Maryland Parkway; Las Vegas, Nevada 89154.

The Russell Sage Foundation assisted in the support of this project and obtained my entry into a major newspaper. I wish to thank Carolyn Mullins for her editorial assistance, Allen Grimshaw, Stuart C Hadden, Candace West, and several anonymous reviewers who read earlier versions of this paper and gave helpful comments.

[1] Throughout this paper I adopt Molotch and Lester's (1974:101–3) conceptual distinctions between happenings, occurrences, and events.

[2] Officially the Regional Bureau Editor is delegated his authority by American News. In practice he operates semiautonomously. Regional Bureau has control over its own pages in the Regional Edition of the paper, although some local stories are selected for publication in other regional editions, in which case the American News Editor has control of the story.

the University News Bureau, an "alternative" weekly paper, two AM radio stations, one FM radio station, and a local television station.

A third source of data is used: other sociologists' ethnographic descriptions and analyses of newswork routines, particularly the work of Tuchman (1972; 1973a; 1973b; 1974; 1976).

TRADITIONS IN THE STUDY OF NEWSWORTHINESS

Newsworkers view their work in terms of *selecting* the most important events, and portraying those in an interesting and informative way. Journalists' conceptions are paralleled by the dominant sociological perspective—gatekeeping (cf. White, 1950; Gieber, 1956; Carter, 1959; Schramm, 1960; Buckalew, 1974; Harless, 1974; Janowitz, 1975; Whitlow, 1977)[3]—which suggests that events are successively filtered through a set of news gates. Official and unofficial (but, nonetheless, literal) values, norms, and rules constitute a template against which events sui generis and stories portraying those events are measured for their newsworthy character. A number of scholars (Halloran et al., 1970; Glasgow University News Group, 1972; Cohen and Young, 1973; Galtung, 1973; Hall, 1973a; 1973b; Phillips, 1973; Young, 1973) have modified this perspective by showing that political ideologies and historical circumstances form an inferential structure for selecting and interpreting events.

However, in both its limited and broader forms, gatekeeping is a substantive application of a paradigm which views social action as primarily deriving from actors' orientation to, internalization of, and enactment of values, norms, and rules (see Wilson, 1970; Wieder, 1974). To be more than merely sensitizing—that is, to be empirically accurate and logically adequate, the "news gates" must be clearly identifiable and available for *literal* description in the career of an event. Moreover, their meaning and applicability would have to be specifiable prior to use as a template for gauging newsworthiness.[4]

If these conditions are not met, a core variable other than gatekeeping is required to explain the process by which journalists identify the newsworthy character of occurrences, events, and stories. My research points to the availability of such an alternative: an ethnomethodological perspective on the process of identifying and displaying newsworthiness which I term "generating newsworthiness."

GENERATING NEWSWORTHINESS

Talk about newsworthiness is a commonplace newsroom activity. From newsworkers' and gatekeeping theorists' standpoints, such talk merely reports on or reflects social reality.

However, when the talk-world dichotomy is analytically suspended, an alternative perspective is made available to the sociologist.[5] Accounts of newsworthiness do not present reality; rather, they forge it. They are one crucial set of practices for *managing* newswork routines.

Articulating particular occurrences, events, and stories with accounts of newsworthiness is not a literal application of a priori rules or news norms to specific situations. Whatever the inherent structures of occurrences might be, what they will have come to as newsworthy events are socially and situationally organized. Generating newsworthiness is a *reflexive* process whereby: (1) accounts of newsworthiness organize and give meaning to occurrences as events and stories;

---

[3] In fact, the central concern of sociologists since World War II had not been with newswork processes at all but, rather, with either the content of media or the consequences thereof. It is quite possible that the fundamentally ambiguous—and even conflicting—findings about media bias and effects are due to a flawed (or at least highly oversimplified) view of news production (see Shibutani, 1966:130, 169). Essentially an input-output conception (cf. Lasswell, 1971; Wright, 1959) has operated in the majority of research.

[4] Wieder (1974) discusses similar analytic requirements for behavior in general to be viewed as rule-governed.

[5] For important discussions of the distinction between topic and resource, see Garfinkel (1967), Zimmerman and Pollner (1970), and Hadden and Lester (1976).

(2) accounts of occurrences, events, and stories elaborate the meaning of accounts of newsworthiness; and (3) the preceding items (1) and (2) are used to display newsworkers' professional and rational methods for doing their work.

Generating newsworthiness is always grounded in some particular news project. So, as practical projects change, so do the form, content, meaning, and practical consequences of accounts of newsworthiness. Moreover, when newsworkers generate competing accounts of newsworthiness, they are not, in fact, constructing competing accounts of the selfsame social object. The following excerpt from field notes (Lester, 1974) at *The Interpreter* is one example:

> (I) In the first week of my research, I was asked to cover a protest against the Vietnam War staged by school teachers at the site of local war industries. The story was written as an "interpretive" article where the particular occurrence illustrated a basic change from a student-centered national movement to local-centered protest by nonstudents. When the story was completed and scanned by the Editor, herein called "Rex," he took me and my story to the People Reporter (a news "beat" within Regional Bureau) who usually does most of the "protest reporting." She said *The Interpreter* had done several pieces on the war. What I should do, she said, was focus on the question, "What is business's response to war protestors?" Then she said there already were articles which reported change in the protest movement—its decline. I claimed we were talking about two different kinds of change. Rex told her that material about business belonged in a separate article, that this story deals with the meaning attached to the protest by the protestors themselves.

Rex's initial project, informing the People Reporter about a story he perceived to lie in her area, faded when seemingly incompatible accounts of newsworthiness were employed. In so doing, the newsworkers actually created multiple events[6] (for example, declining protests, transformation in protest form, the subjective meaning of protest).

Organizational Variation in Generating Newsworthiness

Accounts of newsworthiness constitute a dominant focus for newsroom interaction and are central to an explanation of newswork routines at *The Interpreter*. Yet, they are not detailed in other research on newsmaking processes. Instead of simply assuming oversight by others, I compared others' ethnographic descriptions of research sites and published reports with my own data to discern the organizational features and/or newsworker orientations which might constitute conditions under which generating newsworthiness is a central *newsroom routine*.

I found two patterns in the relationship between organizational features surrounding newswork and the general way in which generating newsworthiness occurs, displayed in Tables 1 and 2.

At issue in this comparison is not *whether* generating newsworthiness occurs but *where* it occurs as a routine interactional focus—that is, "on the beat" vs. "in the newsroom." For personnel working in media organizations oriented to break occurrences, accounts of newsworthiness are constructed routinely among those sharing a beat and in the newsroom only where *particular* cases are *typified* as problematic. At other times, with an orientation to "quickening urgence," workers identify their paper simply as "a newspaper," "a paper of record," "we cover important and interesting events"; or they categorize occurrences as certain types of events—for example, "hard," "soft," "developing" news, etc. (Tuchman, 1973a). When such categories

---

[6] That competing accounts proffer different social objects is not unique to newswork. Pollner (1970; 1974a), for example, examined how traffic court judges adjudicate accounts of District Attorneys, police, and defendants. The adjudication process constitutes the seen-but-unnoticed procedures through which mundane inquiry's commitment to "objective reality" is displayed and sustained. Similarly, Bittner (1967), Cicourel (1968), Zimmerman (1970a; 1970b) and Wieder (1974) point to a similar property of accounts in their respective analyses of how police on Skid Row, probation workers, welfare agency personnel, and residents and staff of a halfway house attend to, and make sense of, apparent contradictions in their rule use, and thereby simultaneously provide for their competent (for example, rule-governed) performances "for all practical purposes."

Table 1. News Media Organized to Cover Breaking Occurrences

Organizational Feature	News Orientation
1. Paper oriented to supplying news on breaking events (Tuchman, 1973a).	1. Principally *identifies itself* as supplier of metropolitan news. What counts as news is not made routinely problematic.
2. A majority of the paper is devoted to metropolitan news. Reporters write at least one, often several stories per day.	2. Reporters spend relatively little time on each story. "Quickening urgence" is the orientation to newswork (Tuchman, 1973a). Occurrences are typified on the basis of the need to manage the flow of work, in particular, to manage occurrences that are "specifically unforeseen." Newsworkers typify and distinguish "hard news," "spot news," "soft news," "continuing news," and "developing news."
3. Reporters have fixed "beats"—e.g., City Hall, Police. Some of these are subdivided into even more specific beats. Reporters spend most of their time at the location of their beats. Reporters from ostensibly competing media who share a beat spend both professional and informal time together.	3. The beat location is the primary site for generating accounts of newsworthiness. Accounts of newsworthiness are routinely produced as an *interorganizational* activity—i.e., among those sharing a beat. In the newsroom, generating newsworthiness occurs when a particular case is typified as problematic.

are assumed to have shared meaning, they are constantly available as a general gloss of newsworthiness to manage the flow of work. However, Garfinkel (1967), Zimmerman (1970a; 1970b), Cicourel (1974), Wieder (1974), and Hadden and Lester (1979) have shown that people in the world of daily life (including reporters and editors) perform cognitive and interactional work to sustain and display the sense, "I know what *that* (for example, an identification of newsworthiness) means" so that a specifically vague account is allowed to stand without notice or breaching while other practical projects are undertaken (for example, working on the beat, generating newsworthiness with reporters sharing a beat).

The orientation to quickening urgence is not the organizational basis for *Interpreter* newswork. Newsroom personnel did not use the same typifying scheme as did Tuchman's subjects for carving the world of occurrences into newsworthy and nonnewsworthy events, which contravenes any generic status for her (Tuchman, 1973a) conceptualization of newsworkers' typifications of news. Generating newsworthiness occurs routinely as an *intra*organizational activity. Glosses of newsworthiness are replaced by the *interactional* production and display of newsworthiness at multiple points in news production. Instead of hard vs. soft news, etc., the issues related to identifying newsworthy and nonnewsworthy occurrences-as-events and stories are of the sort: "What, in general, should we be looking at?" "Is it newsworthy?" "What kind of coverage should be given?" and "How can we make this story different from other medias' accounts and meaningful to the reader?"

One indicator of this variation is that several staff moving to *The Interpreter* from metropolitan media encountered difficulty in changing their work routines with regard to the specific organizational features described. A second source of verification of the different loci for generating newsworthiness comes from data (collected in other media organizations) which turn out to lie between the two ideal types depicted.

FRAMING THE ORGANIZATION, NEWS NORMS, AND STORIES

Generating newsworthiness can take three different forms: (1) Framing the

Table 2. The Interpreter: An Example of a Multifocused News Media

Organizational Feature	News Orientation
1. Stories on breaking occurrences typically appear at least one day later than in newspapers organized to cover breaking events.	1. No one definition of the newspaper's function pervades. What counts as news is made routinely problematic. The "real status" and "real meaning" of the paper, news, and newsworthiness are identified in different ways on different occasions.
2. A maximum of two pages of the Regional Edition of the paper is devoted to regional news, most of which is City-related. Reporters write no more than three stories per week.	2. Reporters spend a lot of time on each story—finding topics, discussing details with colleagues, etc. The newsroom has an ambience of casualness; "quickening urgence" is rarely an orientation. No one typifying scheme is used by workers to manage the flow of work.
3. "Beats" are generalized roles[7] (e.g., Environment, People, Suburbs) which change with *Interpreter* and Organization goals, personnel, and current news themes. The only conventional beat is City Hall, to which one reporter is assigned. Reporters spend much of their time in the newsroom. Due to the features outlined above, reporters have no counterpart in other media organizations and eschew such contact.	3. The newsroom is the primary site for generating accounts of newsworthiness. Generating newsworthiness occurs as an *intraorganizational* activity. *Interpreter* personnel are consistently available to each other for, and are oriented to, identifying and displaying newsworthiness as a routine feature of newswork.

Organization, (2) Framing News Norms, and (3) Framing Stories. Each form can be readily identified as an interactional focus in specific situations and each has distinct consequences for newswork.

Framing the Organization

Tables 1 and 2 show that organizational features and organizational context—as defined and accounted for by members of news organizations[8]—make a crucial difference in the news process. As such, persons' accounts of organization are an important component of generating newsworthiness.

These organizational accounts which I term "framing the organization" may, in a direct way, socially organize and give meaning to occurrences (for example, as newsworthy events or not) as well as assemble their specific features (for example, what will be taken as features of occurrences and their ordering into a coherent pattern). As such, this form of generating newsworthiness is integral to newswork—in fact, occurring within the stream of ongoing newswork activities. The following excerpt (Lester, 1972) illustrates this relationship between accounts of organization and newsworthy occurrences.

> (II) During a portion of weekly Regional Bureau staff meetings, methods for covering various occurrences are discussed. In one such session, the topic of water fluoridation arose which one reporter then abruptly closed: "The [Organization's] interest in fluoridation is declining. . . . The people behind antifluoridation are crazy. . . . I don't want to get involved. . . . The arguments are extreme."

Here, framing the organization is used as an embedded interpretive device for making sense of an occurrence. Reciprocally, the account of the occurrence elaborates the meaning of the organization as formulated.

---

[7] Hughes (1940) shows that specialized feature topics are also carved out as beats in metropolitan newspapers. The difference is that these were the primary type of role at *The Interpreter*.

[8] More generally, Bittner (1965) suggests that a person's "concept of organization" is used as an embedded organizational resource to manage work routines.

On other occasions the connection between framing the organization and the very practical routines for putting out the paper are more opaque. Accounts of organization often serve as a reminder to one's self about the status and meaning of otherwise "seen but unnoticed" newswork practices. Only upon such a reflective glance, entailing a shift of attention from the immediate practical task at hand, are social actions (for example, newswork strategies; either meaningful or discernible as a series of discrete activities (cf. Schutz, 1962). The next illustration (Lester, 1974) comes from the alternative newspaper in a midwestern city. In this case, a concept of organization in terms of intended readers is used to "make sense" of particular newswork strategies:

> (III) "We appeal to people in and around the University rather than to the general townspeople or to just students exclusively. . . . We appeal to students who aren't into all the campus activities. . . . The main thing that we do that is different is that we spend more time researching topics and more hours with the people we're writing about. Most papers don't have the time to get the real facts. . . . We usually do more than interview and try to follow a scene all the way through to find out what's actually happening."

Finally, framing the organization may occur outside any specific newswork situation and function to identify or formulate the organization per se. The illustration (IV), following, comes from field notes (Lester, 1972) taken at a formal meeting of all Organization workers (that is, *Interpreter* and all other Organization personnel).

> (IV) The Editor of *The Interpreter* spoke first. He said the paper as a whole including the editorial pages has a concern with healing: "If you could see a theme, it would be a responsiveness to good." Then he said the goal of [*The Interpreter*] was problem solving. He noted three phases in journalism: "(1) fact reporting, (2) interpretive reporting, and (3) what to do about the situation. And [*The Interpreter*] has led the last two phases of journalism." Another speaker was the Syndication Editor who selects *Interpreter* stories for sale to other subscribing media. He said the syndication activity is a "mission mandated by [the Founder of Organization] by virtue that [Founder] said. . . ."

Three distinct concepts of organization are provided: having a healing effect, leading the last two phases of Journalism, and syndication being an organizational mission. Each account offers a scheme of interpretation for understanding one's everyday activities and each elaborates the meaning of the others.

One additional feature of this form of generating newsworthiness is also frequently present, either explicitly or implicitly—subjective evaluation of the organization. The illustrations above could imply that framing the organization consists solely in accounts which proffer the "*good* sense of what we do." Though not occurring frequently in the data, there can, of course, be negative organizational accounts which have the same formal structure and practical consequences as other accounts, varying only in content.

Framing News Norms

Framing news norms is the situated identification and display of news criteria. Accounts of news norms are rules-in-use. They inform the organization of, and give meaning to, the situation at hand (for example, producing stories at some point or providing for the competent character of a particular newswork strategy, etc.). At the same time, the situation *as attended to* elaborates the news norm. An excerpt from field notes (Lester, 1972) taken at *The Interpreter* illustrates:

> (V) The Clerk for Regional Bureau typically assembles one part of the Regional Pages—a relatively large "box"[9] containing both original short articles and rewritten wire service material. While doing this one afternoon, she looked up and said, "You throw away car crashes, murders, throw away anything that has a really negative tone—like fires, accidents, things like that." At this point, Rex interrupted: "Some things can be newsworthy. *Keep the sensationally nega-*

---

[9] A "box" is a story set off on the page by enclosing the entire story or stories within 4 bordering lines.

> *tive*. When in doubt, you throw it to the Editor." While Rex went back to his work, the Clerk's conversation continued: "I throw the majority out. Certain times when I'm not sure, I save it and show it to the Editor."

In excerpt (V), the Clerk frames a news norm for doing one part of the local section of the paper—for example, throw away anything that has a negative tone. Neither she nor anyone else ever supplied a more specific set of criteria for gauging "negativism" (or any other news norm for that matter). Rather, we find her bringing substantive examples of the phenomenon ("fires, accidents, things like that")—to bear on the essence of the norm. Such examples do not merely illustrate a preestablished or clearly specified news norm. As a routine property of this form of generating newsworthiness, they are devices used by speaker and hearer to extract the situated essence—the substance, applicability and meaning—of the news norm in a particular situation. And, reflexively, the news norm as formulated identifies the situated essence of the examples—for example, fires are here assembled as instances of negativism. (Fires can also be attended to as instances of other kinds of occurrences.) The reflexive tie between accounts of news norms and examples provides persons with a resource to perform needed "ad hocing" work (Garfinkel, 1967:20–34), filling in unstated, but assumed, meaning that each can use to provide the sense, "we understand what newsworthiness is—for the time being."

A further analytic feature is illustrated in (V) above, the essential incompleteness of all news norm accounts. Rex apparently sensed the incompleteness and possible misunderstanding of the Clerk's first account of negativism, and he attempted to repair the situation ("*Keep the sensationally negative*.") However, this "exception" to the rule renders it no more a "news gate" than before, for it is itself specifically vague. As a repair, it sufficed only for the moment. Subsequently, the Clerk uses the Editor's "exception" as her resource to extend and, from her standpoint, to repair the previous formulation—for all practical purposes—for example, to display her competence as a newsworker, as well as to provide practical understanding for me. However, there can never be enough elaboration for the news norm to become a literal description of newsworthiness—that is, to clear up its indexical or context-sensitive nature (see Garfinkel, 1967:4–7). In none of these data is there a bottom-line, once-and-for-all framing of any news norm, only formulations that manage the practical circumstances at hand.

The reflexive pairing of news norm and examples is one method persons use to identify and display newsworthiness. A second method uses persons' accounts of the good and sensible "*reasons*" for a news norm. The following is illustrative:

> (VI) In a conversation (Lester, 1972) with Rex, he said reporters look for "an angle" or "a different context" for their reports.[10] That is, they have to "make the story relevant" because of the "time problem" (see Table 2: Organizational Feature, part 1). I asked him if he thought he missed important stories because of the time lapse between the occurrence and publication of stories. He said, "Yes," and immediately provided an example: "George Wallace was shot about 3:30 on a Monday afternoon. A small box was written by 4 p.m. The next day there appeared 'interpretive stories.' "

First, a general news norm is offered. Then, Rex details the reasonableness of the norm: structural contingencies inhibit timely coverage of breaking events. Generally, the data suggest accounts of structural contingencies to be a common type of "reason" offered for a news norm along with accounts of events in which values and personal tastes are imputed to both consumers and news organizations (in excerpt III, quoted earlier). It may appear that these reasons are merely justifying statements as described by Mills (1940) and Scott and Lyman (1968). While they may justify news norms, they are also important for newsworkers' identification and display of the essential meaning and practical uses of the news norm. When disparate accounts of "reasons" are provided, the content, meaning, and uses of a news norm are fundamentally different.

---

[10] "Making a story relevant" by "finding an angle" was an important news norm. In the absence of an alternative to focusing solely on hard news or "breaking events," a more serious trouble in self-identification as a news organization would result: old news is no news. Finding an angle is used as a practical, though fundamentally interpretive, solution to that quandary.

As an empirically and logically related matter, if the "same" occurrence is depicted through different news norms, multiple public events are thereby made available. For example, Rex did not frame the "negativism" in the assassination attempt on Mr. Wallace in case (VI) above. Rather, these occurrences were used to render the meaning of a different news norm (that is, "finding an angle"). As an observer, however, I saw potential contradiction with the "negativism rule" and questioned the Clerk (who had been present during the interaction in [VI] above:)

> (VII) He said, "[*The Interpreter*] doesn't cover negative things." I raised the point of the assassination attempt on Mr. Wallace. He said, "Yes, that got covered, but *in the context of gun control*" (emphasis added).

At this point, the Clerk totally reframes the occurrence and how it was covered—that is, the occurrence is now an event documenting the need for stricter gun control. In effect, his account neutralized a reasonable observation of contradiction and created consistency between the news norm (negativism) and a possible violation.

Framing news norms has analytic consequences, as well as practical effects, for any conception of newsmaking. A simple gatekeeping perspective requires, first, that rules be specifiable prior to their use as a template to gauge newsworthiness and, second, that rule application be fundamentally independent of the situations being attended to under the rubric of any particular rule. My data suggest that these requirements are not met empirically, nor can they be met. Occurrences are not prepackaged "out there" and then selected by a finite and literal set of news norms. Nor is framing news norms just an intermediary stage or "gate" in newswork: (1) here's the world of occurrences, (2) here's the news norms, and (3) here's the final events-for-reporting. Rather, this process essentially provides embedded instructions (see Wieder, 1974) for socially organizing and giving meaning to newswork strategies as well as to occurrences, events, and stories.

Framing Stories

The focus of framing the organization and framing news norms is the situated construction and use of schemes of interpretation for managing newswork. Complementing that focus, framing stories involves the use of accounts of newsworthiness to render *particular occurrences as public events*. As such, this form of the process has a thoroughly practical orientation.

Framing stories includes accounts which assemble the overall newsworthy character of particular occurrences. Or a newsworker may show how an occurrence is not a newsworthy event and will not result in a good story. These types of accounts are illustrated in excerpts (VIII) and (IX) (Lester, 1972):

> (VIII) The Clerk for Regional Bureau had been assigned to write a "wrap up" (summary) of college commencement exercises. He looked up at Rex and me at one point and initiated a conversation:
> Clerk: Commencements are so boring.
> Rex: Maybe work out a "box," what speakers are saying.
> Clerk: I don't think there's any common thing as far as I can see.

> (IX) The Suburbs Reporter came over to Rex's desk: "I don't think I have a story here. The City *always* rejects bonds for new schools."

Without knowing "the facts," Rex identified commencement exercises as one coherent event-for-reporting in (VIII) above. With an alternative account of newsworthiness, there may have been several separate stories. Or, without this interaction, there would have been no story since the Clerk identified commencements as "boring"—that is, nonnewsworthy. Each account frames "commencements" into a different patterned event-for-reporting—or nonpattern (that is, no "common thing") as in the Clerk's second account proffering the absence of a newsworthy story. However, when an account of some occurrence proffers too much typicality, it becomes nonnews. This occurs in excerpt (IX) where the rejection of a school bond initiative is seen as "just" an instance of a type which "everyone already knows" (see Schutz, 1962:3–34).

Framing stories is not a literal description of occurrences. Rather, it is an interpretive device for actually assembling the essence of the occurrence, as well as the written depictions thereof, as the next two excerpts (Lester, 1972) show.

> (X) Rex and a reporter had been talking about a state official vetoing a City redevelopment project. The reporter had talked about his plan to cover "both sides" (that is, State vs. City). Rex responded:
> Rex: I'm concerned with the whole philosophy of urban renewal, that what we're doing is leasing large parts of the city to large financiers.
> Reporter: Urban renewal is concerned with rebuilding the City. . . . This is the best Model Cities Program in the country. Maybe on a national level it should be looked at.
> Rex: When are you going to do it?

> (XI) During a Regional Bureau staff meeting several topics had been discussed. Then, Rex said he had two series he wanted done. One involved a five-part series on urban renewal. He said that [City] could be used as case-in-point and he expected American News to pick up the stories and carry them as well. He assigned various reporters to the five parts. (The discussion of the second series then commenced in a similar form, not shown here.)

In (X), the reporter begins by treating the State's rejection of a renewal project as a singular event. Rex then reframes the event: treat "this case" as part of a broad public issue ("I'm concerned with the whole philosophy of urban renewal. . . ."). The reporter offers an alternative account: City redevelopment is sound, but it may be a problem on a national level. Since the Editor did not deny the newsworthy character of the rejection of the City project, the alternative accounts were allowed to pass at this point. However, in (XI), Rex presents an elaboration of his previous event and story framing: City urban renewal typifies a national problem. He suggested a series with five major aspects to be covered. Formulating a series is a particular interpretive method by which otherwise discrete events (which could be reported as such) are transformed into a gestalt.[11] However, this particular account of newsworthiness is no more a literal or objective interpretation of "what's really happening" than other substantive organizing principles. In this case, (XI) for example, the American News Editor attended to the stories as isolated news reports *about* City rather than the City program as "a typical case." Consequently, the stories remained on the local pages of the paper's local edition.

Finally, generating newsworthiness often consists of retrospective interpretations of how an occurrence or written depiction was managed "after all" by reporters, editors, copyreaders, and other technical staff. What was "finally" done with a story is used as an interpretive scheme by newsworkers to identify their own competence, treatment of their work by other staff, etc. Here is one kind of example.

> (XII) A reporter had written a story about a program sponsored by the federal government in which people are trained to be bus drivers and bus boys, etc. His original lead to the story criticized the effects of the program—for example, so few got permanent jobs. The American News Editor (who selected the local story for national distribution) added a paragraph early in the story to the effect that there were "good things" about the program too. The reporter said that the American News Editor had misused his discretion by adding a paragraph "without the facts."

SUMMARY

This paper applied principal tenets of ethnomethodology to extend sociological understanding of a crucial part of the news process—that is, how occurrences become newsworthy events and stories. The ethnomethodological perspective is an alternative to the more structural explanations of the news process, especially as found in gatekeeping studies. Various facets of the news process are dependent on newsworker interaction to construct and interpret news criteria and the occurrences such criteria help to render.

---

[11] Framing series is not unique to *The Interpreter*. There is an analogue to this interpretive strategy. Fishman (1978) has done a thorough empirical study of crime waves as the news media's structuring of otherwise discrete occurrences. Here, media personnel not only treat particular occurrences as instances of a news theme, but proffering a theme is a scheme of interpretation for actively searching for instances—that is, occurrences as public events. The structuring and use of news themes is also discussed at length by Halloran et al. (1970), the Glasgow University News Group (1972), Cohen (1972) and Cohen and Young (1973).

Importance and interest are not endemic properties of occurrences, events, or even completed stories. The rules governing newswork are not simply given and available, but actually constructed, interpreted, and elaborated upon in the actual settings of newswork. Reflexively, through the process of generating newsworthiness, occurrences and events are framed in terms of their constituent features, meanings, and consequences. As well, accounts of newsworthiness render newsworkers' own work routines as rational, sensible, and competent. Finally, it has been shown that generating newsworthiness has a direct bearing on what appears or fails to appear in the newspaper.

REFERENCES

Bigus, Odis, Stuart C Hadden, and Barney Glaser
1978 "The study of basic social processes." In R. Smith and P. Manning (eds.), The Handbook of Social Science Methods: Qualitative Methods. New York: Irving.

Bittner, Egon
1965 "The concept of organization." Social Research 32:239–58.
1967 "The police on Skid Row." American Sociological Review 32:699–715.

Buckalew, James
1974 "The radio news gatekeeper and his sources." Journalism Quarterly 51:602–6.

Carter, Roy
1959 "Newspaper gatekeepers and the sources of news." Public Opinion Quarterly 22:133–44.

Cicourel, Aaron
1968 The Social Organization of Juvenile Justice. New York: John Wiley.
1974 Cognitive Sociology. London: Macmillan.

Cohen, Stanley
1972 Folk Devils and Moral Panics: The Creation of the Mods and Rockers. London: MacGibbon and Kee.

Cohen, Stanley, and Jock Young (eds.)
1973 The Manufacture of News. Beverly Hills: Sage.

Fishman, Mark
1978 "Crime waves as ideology." Social Problems 25:531–43.

Galtung, Johan, and Mari Ruge
1973 "Structuring and selecting news." Pp. 62–72 in Stanley Cohen and Jock Young (eds.), The Manufacture of News. Beverly Hills: Sage.

Garfinkel, Harold
1967 Studies in Ethnomethodology. Englewood Cliffs: Prentice-Hall.

Gieber, Walter
1956 "Across the desk: a study of 16 telegraph editors." Journalism Quarterly 33:423–32.

Glasgow University News Group
1972 Bad News. London: Routledge and Kegan Paul.

Glaser, Barney, and Anselm Strauss
1967 The Discovery of Grounded Theory. Chicago: Aldine.

Hadden, Stuart C, and Marilyn Lester
1976 "Looking at society's troubles: the sociology of social problems." Pp. 4–31 in D. Zimmerman and D. Wieder, Understanding Social Problems. New York: Praeger.
1979 "Talking identity: people's practices for producing 'self' in interaction." Human Studies.

Hall, Stuart
1973a "A world at one with itself." Pp. 85–94 in Stanley Cohen and Jock Young (eds.), The Manufacture of News. Beverly Hills: Sage.
1973b "The determination of news photographs." Pp. 176–90 in Stanley Cohen and Jock Young (eds.), The Manufacture of News. Beverly Hills: Sage.

Halloran, James, Phillip Elliot, and Graham Murdock
1970 Demonstrations and Communication: A Case Study. Middlesex, England: Penguin.

Harless, James
1974 "Mail call: a case study of a broadcast news gatekeeper." Journalism Quarterly 51:87–90.

Hughes, Helen
(1940) News and the Human Interest Story. New
1968 York: Greenwood Press.

Janowitz, Morris
1975 "Professional models in journalism: the gatekeeper and his advocate." Journalism Quarterly 52:618.

Lasswell, Harold
1971 "The structure and function of communication in society." Pp. 117–30 in W. Schramm (ed.), Mass Communication. Urbana: University of Illinois Press.

Lester, Marilyn
1972 From unpublished study notes taken during internship on major newspaper (herein called "The Interpreter").

Mills, C. Wright
1940 "Situated actions and vocabularies of motive." American Sociological Review 5:904–13.

Molotch, Harvey, and Marilyn Lester
1974 "News as purposive behavior: on the strategic use of routine events, accidents and scandals." American Sociological Review 39:101–12.

Phillips, Damien
1973 "The press and pop festivals: stereotypes of youthful leisure." Pp. 323–33 in Stanley Cohen and Jock Young (eds.), The Manufacture of News. Beverly Hills: Sage.

Pollner, Melvin
1970 "On the foundation of mundane reason." Unpublished Ph.D. dissertation, Department of Sociology, University of California, Santa Barbara.
1974 "Sociological and common sense models of the labeling process." Pp. 27–40 in R. Turner (ed.), Ethnomethodology. New York: Penguin.

Schramm, Wilbur
1960 "The gatekeeper: a memorandum." Pp. 175–8 in W. Schramm (ed), Mass Communication. Urbana: University of Illinois Press.

Schutz, Alfred
1962 Collected Papers I: The Problem of Social Reality. The Hague: Martinus Nijhoff.

Scott, Marvin, and Stanford Lyman
1968 "Accounts." American Sociological Review 33:46–62.

Shibutani, Tamotsu
1966 Improvised News. New York: Bobbs-Merrill.

Tuchman, Gaye
1972 "Objectivity as strategic ritual: an examination of newsmen's notions of objectivity." American Journal of Sociology 77: 660–79.
1973a "Making news by doing work: routinizing the unexpected." American Journal of Sociology 79:110–31.
1973b "The technology of objectivity: doing objective television news." Urban Life and Culture 2:3–26.
1974 "Assembling a network talk-show." Pp. 119–35 in G. Tuchman (ed.). The TV Establishment. Englewood Cliffs: Prentice-Hall.
1976 "Telling stories." Journal of Communication 24:93–7.

White, David
1950 "The 'gatekeeper': a case study in the selection of news." Journalism Quarterly 27:383–90.

Whitlow, S. Scott
1977 "How male and female gatekeepers respond to news stories of women." Journalism Quarterly 54:573–7.

Wieder, D. Lawrence
1974 Language and Social Reality. The Hague: Mouton.

Wilson, Thomas P.
1970 "Conceptions of interaction and forms of sociological explanation." American Sociological Review 35:697–709.

Wright, Charles
1959 Mass Communication: A Sociological Perspective. New York: Random House.

Young, Jock
1973 "The myth of the drug taker in the mass media." Pp. 314–22 in Stanley Cohen and Jock Young (eds.), The Manufacture of News, Beverly Hills: Sage.

Zimmerman, Don
1970a Record keeping and the intake process in a public welfare agency." Pp. 319–54 in S. Wheeler (ed.) On Record. New York: Basic Books.
1970b "The practicalities of rule use." Pp. 221–38 in J. Douglas (ed.), Understanding Everyday Life. Chicago: Aldine.

Zimmerman, Don, and Melvin Pollner
1970 "The everyday world as a phenomenon." Pp. 80–104 in J. Douglas (ed.), Understanding Everyday Life. Chicago: Aldine.

COMMENTS

THE RELAY ASSEMBLY TEST ROOM: AN ALTERNATIVE STATISTICAL INTERPRETATION*

(Comment on Franke and Kaul, *ASR*, October, 1978)

INTRODUCTION

In what they call the "first statistical interpretation" of the data from the first Relay experiment (the most prolonged of the Hawthorne experiments), Franke and Kaul (1978:623–43) show that well over 90% of the variation in the average productivity of the workers involved can be accounted for by just three explanatory variables: two dummies of their own construction, which they interpret as "managerial discipline" and "economic depression"; and one experimental variable, scheduled rest time. They conclude (pp. 636–7) that "the lack of substantial unexplained variance . . . indicates that the unmeasured supervisory and social interaction variables were not very important economically."

In what follows, I shall show that this conclusion is not supported by the data. Specifically, I shall show that the data agree very much better with models in which average productivity increases gradually, with time, over the first three years of the experiment than they do with the model presented by Franke and Kaul (1978) in which most of the improvement over the first three years occurs in two abrupt jumps. Whether the gradual increase in productivity with time is to be attributed to "unmeasured supervisory and social interaction variables" or to unmeasured variables of some other sort is a question of interpretation on which statistical analysis has nothing to say.

ERRORS IN THE FIRST STATISTICAL ANALYSIS

Franke and Kaul (1978) investigated the output of the workers in the first Relay experiment by their analysis of two families of additive models: Model 1, in which the dependent variable is output per hour, and Model 2, in which the dependent variable is output per week, with hours per week as an additional explanatory variable. Since a reasonable additive model of output per week would have to contain a number of "interaction" terms[1] not included in their Model 2, I shall reexamine only the simpler Model 1.

All results will be based on the data used by Franke and Kaul (1978:639–40) and published in their Appendices; the reader is reminded that the outputs in these data are averages of weekly outputs within each of 23 experimental "periods" varying in length from 2 to 31 weeks. For brevity, the "first Relay experiment" will usually be called simply "the experiment"; "scheduled rest time" will be called simply "rest time"; "productivity" will be used as a synonym for "output per hour." The reader is assumed to be familiar with the discussion by Franke and Kaul, but it may be useful to recall the definitions of the two dummy variables which play so large a part in their explanation of the phenomena. "Managerial discipline" has the value 0 up to the moment when workers 1A and 2A were replaced by workers 1 and 2, the value 1 thereafter. "Depression" has, in principle, the value 0 up to the stock-market crash on October 24, 1929, the value 1 thereafter; it is set to .74 in the period within which the crash occurred. For a reason which will appear in due course, this variable will hereafter be called "crash" rather than "depression."

Serial correlation and the "passage of time." Taking the average productivity of the workers in the experiment as the dependent variable, the stepwise regression program used by Franke and Kaul selected the explanatory variables and produced the regression coefficients and value of R^2 shown in column 1 of Table 1, together with the value 1.58 for the Durbin-Watson statistic. Observing that 1.58 is significant at the .05 level, Franke and Kaul (1978:631) inferred that "passage of time and unspecified historical factors appear to have influenced the residuals," and proceeded to make a partial correction for serial correlation which yielded the coefficients and R^2 in column 2.

Because a continuing increase of productivity over time which cannot be attributed to

---

* Direct all correspondence to: Robert Schlaifer; Graduate School of Business Administration; Harvard University; Soldiers Field; Boston, MA 02163.

[1] For example, consider the effect of managerial discipline in a model in which the dependent variable is weekly output. If discipline affects output in each hour worked, then the effect of discipline on weekly output will be not a constant, but proportional to the number of hours worked; and, therefore, the model must contain an interaction variable which is the product of the variables, discipline and hours worked.

any measured explanatory variables is the only way in which the effects of unmeasured variables could manifest their presence in the data available to Franke and Kaul (1978), one might have expected them to investigate the effect of the passage of time a little more fully.[2] It may be, however, that the R^2 they obtained after partial correction for serial correlation was still so large that it simply reinforced their belief that the effect of time was unimportant—if, in fact, it existed at all.

Franke and Kaul's (1978) discussion of the effect of time rests, however, on a misunderstanding of the theory underlying the kind of correction for serial correlation that they applied. Such a correction is not appropriate when the "errors" or "disturbances" (u in the successive values of the dependent variable) are correlated with time. It is appropriate when the errors are *not* correlated with time (or any other potential predictor variable) but are correlated with each other because they are generated by a process such that

$$u_t = \rho u_{t-1} + \epsilon_t,$$

where the ϵ's are assumed to be independent and ρ is a parameter to be estimated just as the coefficients of the explanatory variables are to be estimated.[3]

Serial correlation is, thus, only very indirectly relevant to the real problem at hand, but, for future reference, it is worth remarking that if Franke and Kaul (1978) had analyzed the autocorrelated-errors model by least squares, and if they had then correctly computed the fraction of the variance of average productivity that was explained by the model, they would have obtained the results in column 3 of Table 1.[4] As one would expect, the value of R^2 goes up rather than down when the additional parameter ρ is added to the model, but the important thing to observe is that the changes in the regression coefficients resulting from full allowance for autocorrelation are completely negligible.

Analysis of residuals. Because Franke and Kaul (1978) thought that the residuals may have been "influenced by the passage of time" but really did nothing to determine whether or not they were in fact so influenced, the question remains open. The usual first step in the investigation of a question of this sort is simply to plot the residuals against time; and this is done in Figure 1 for the residuals of the Franke and Kaul model as originally fitted to the data, without any correction for serial correlation. Time is measured in weeks from the start of the experiment; each residual is plotted at the temporal midpoint of the experimental period to which it belongs. The two dashed vertical lines in the figure divide time into three intervals delimited by the imposition of managerial discipline at $t = 39$ (end of period 7) and the crash at $t = 131$ (shortly after the beginning of period 15).

What immediately strikes one on inspecting the plot in Figure 1 is not that the residuals are serially correlated but that they are strongly correlated with time within each of the three intervals which correspond to the "treads" or "flats" in the step function defined by the dummy variables, "discipline" and "crash." The three sloping lines in the figure represent the within-interval regressions of the residuals on time; the fractions of the within-interval variances of the residuals that are explained by the regressions are .538, .264, and .271.[5]

Explicit allowance for passage of time. Given so clear a set of relations between the residuals and time, the natural next step is to look to see what happens if a similar set of relations between productivity and time is allowed to appear explicitly in the explanatory model. If time t is measured in weeks from the start of the experiment, this can be done by introducing three new explanatory variables:

$$\begin{aligned} T1 &= \min\{t, 39\}, \\ T2 &= \max\{0, \min\{t,131\}-39\}, \\ T3 &= \max\{0, t-131\}. \end{aligned}$$

Taken together, these variables allow the effect of time on productivity to appear as a

---

[2] Had Franke and Kaul (1978) added either a measure of time or the serial number of the experimental period to the explanatory variables from which they asked their stepwise regression program to choose, the program would have chosen that variable in lieu of crash. Had they added a variable equal to time up to the crash and constant thereafter, this variable would have been the first variable chosen by their program and would, by itself, have explained 89.2% of the variance of average productivity.

[3] Franke and Kaul (1978:628) cite two papers by Durbin and Watson (1950; 1951) as references for their assertion that the "influence of other (generalized) historical factors as well as the passage of time can be measured as serial correlation," but I can find no trace of such a notion in these papers.

[4] Franke and Kaul took account of serial correlation by use of the approximate transformation called T2 by Johnston (1972:261) instead of the exact transformation T1; and instead of finding the least-squares estimate of p, which is .168, they used the guessed value .25. Their R^2 is the explained fraction of the variance, not of the dependent variable y, but of $(y_t - .25y_{t-1})$.

[5] In contrast, only .030 of the variance of the residuals is explained by the serial correlation which produced the value 1.58 of the Durbin-Watson (1950; 1951) statistic.

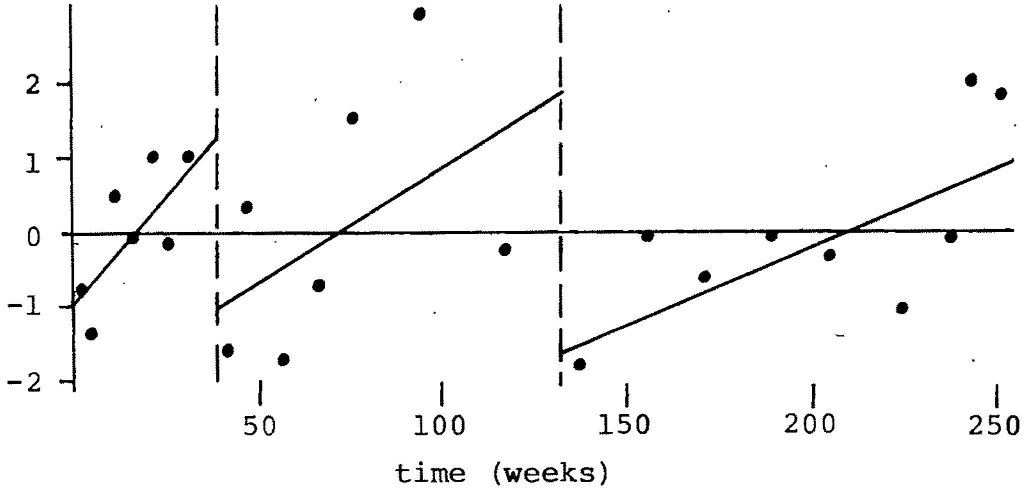


Figure 1. Residuals of Franke and Kaul Model

piecewise-linear continuous function with changes of slope at t = 39 and t = 131. The slope of each linear segment is equal to the regression coefficient of the corresponding T.

Regression of average productivity on these new variables together with the three original Franke and Kaul variables yields the coefficients and R^2 shown in column 4 of Table 1. Notice that, as suggested by Figure 1, the coefficient of T2 is less than that of T1 and the coefficient of T3 is less than that of T2. Productivity seems to increase with time—but at a decreasing rate.

Importance of explanatory variables and decomposition of R^2. Since the R^2 of the augmented model described in column 4 of Table 1 is only .012 greater than that of the original model described in column 1, Franke and Kaul would presumably argue that their conclusion has been confirmed. The effects of T1, T2, and T3 are so small that they are "not very important economically."

Table 1. All Workers; All Periods

	(1)	(2)	(3)	(4)	(5)
Discipline	8.64	8.26	8.50	5.64	5.60
Crash	6.18	6.34	6.27	3.07	.75
Rest time	.176	.164	.176	.113	.127
T1	—	—	—	.121	.103
T2	—	—	—	.026	.046
T3	—	—	—	.023	.029
R^2	.971	.945	.972	.983	.964

(1) Franke and Kaul (1978) Model 1.

(2) Attempted correction for serial correlation.

(3) Serial correlation estimated by least squares.

(4) Piecewise-linear trend added: changes in slope after discipline and crash.

(5) Observations weighted by lengths of periods.

It is interesting, however, to see what would have happened if the six variables in the augmented model had been selected—and their contributions to R^2 computed—by the stepwise regression program used by Franke and Kaul (1978). The variables would have been selected in the order T2, T1, T3, "crash," "rest time," and "discipline"; and the corresponding stepwise increments in R^2 would have been .788, .151, .020, .010, .009, and .005. The sum of the contributions of the three T's would, thus, have been .959; and, therefore, the kind of argument advanced by Franke and Kaul would have led to the conclusion that, because 95.9% of the variance of productivity can be explained by the passage of time, the three explanatory variables in the Franke and Kaul model were not very important economically.

The argument is fallacious, however, in either its revised or its original form. It is only when the explanatory variables are uncorrelated that R^2 or the variance of the dependent variable can be uniquely and, therefore, meaningfully decomposed into a sum of parts due to the several explanatory variables. When the explanatory variables are correlated ("multicollinear"), some quite different way of assessing relative importance must be found if relative importance is to be assessed at all. For example, one might compute that, in the model currently under discussion, passage of time over the 253 weeks of the experiment is responsible for 9.9 units/hour of additional productivity, discipline and the crash for 8.7 units/hour, and 25 minutes of scheduled rest time for 2.8 units/hour.

Heteroscedasticity; weighting of observations. One more assumption implicit in Franke and Kaul's (1978) analysis must be reexamined before trying to get a better understanding of the phenomena by means of least-squares regression. As is well known, fitting by (ordinary) least squares is not justified unless the variances of the random errors in the successive values of the dependent variable can be assumed to be at least roughly equal, but it is easy to see that the variances of the errors in the productivities analyzed by Franke and Kaul cannot all be of even the same order of magnitude.

If we had available the average productivity of the workers in each individual week of the experiment, the natural assumption would be that the variance of the unobservable errors in these weekly productivities was constant from week to week; and the fact that this assumption is, at least, a rough approximation to the truth is borne out by Whitehead's (1938) graph H42 of weekly average productivity. Then, because the productivities which are in fact available for analysis are averages of weekly productivities over periods varying in length from 2 to 31 weeks, it would require some very peculiar assumptions to avoid the conclusion that the variances of the errors in the available data range from about ½ to about 1/31 of the roughly constant variance of the weekly errors.

Unless proper account is taken of this extreme heteroscedasticity, estimates of regression coefficients will be subject to extremely large sampling errors; but, fortunately, it is easy to take the required account. All that is needed is to weight the squares and cross-products for each period by the number of weeks in the period, and this will be done in all regressions to be discussed from now on.[6] Coefficients derived by weighted regression for the six-variable model of Table 1 are shown in column 5 of that table, where one should notice particularly the effect of weighting on the coefficient of "crash" and the relative magnitudes of the coefficients of the T's. The weighted R^2 of the six-variable model is .964, as shown in Table 1; the weighted R^2 of the original Franke and Kaul (1978) model is .940.[7]

Serial correlation reexamined. That the weekly errors were not strictly independent is obvious from several of Whitehead's charts, and it follows that if the weekly data were available one would be obliged to investigate the error structure. The fact that the productivities actually available for analysis are averages over widely varying numbers of weeks means, however, that the model implicit in Franke and Kaul's (1978) correction for autocorrelated errors makes no sense in the present context. One cannot reasonably assume, for example, that the fraction of the mean of 31 weekly errors that is "due to" the mean of 4 preceding weekly errors is the same as the fraction of the mean of 2 weekly errors that is due to the mean of 4 preceding weekly errors. The same objection holds *mutatis mutandis* for any of the more complicated standard time-series error models, and, consequently, all that one can do is regret the fact that it is not possible to take proper account of interdependence of errors. One may, however, take some consolation from the facts that (1) coefficient estimates are unbiased even when no account is taken of autocorrelation of errors, and (2) correction for moderate amounts of autocorrelation often results in negligible changes in regression coeffficients, as is illustrated by the close agreement between columns 1 and 3 of Table 1.

DISCIPLINE AND DEPRESSION

Managerial discipline. As Franke and Kaul (1978:631) themselves recognize, the variable they call managerial discipline is a rather peculiar mixture which "includes better-performing replacement workers as well as the disciplinary example" resulting from the replacement of workers 1A and 2A at the end of period 7. The Hawthorne experiments were directed, however, not to the effects of what is ordinarily called personnel selection but to the factors affecting the productivity of a given worker or group of workers. What is needed for evaluation of these factors is an estimate of the effect of the "disciplinary example" on the average productivity of the workers who were *not* replaced at the end of period 7, namely workers 3, 4, and 5.

Even though worker 5 was temporarily replaced at the end of period 13, for a reason having nothing to do with discipline, we still have data available for the purpose at hand for workers 3, 4, and 5 from the start of the experiment, 39 weeks before discipline was exercised, to the end of period 13, 75 weeks after the exercise. Column 1 of Table 2 shows the results of weighted regression of the average productivity of these three workers on four of the six explanatory variables appearing in Table 1, the two omitted variables being irrelevant to the first 13 periods. The only striking difference between the regression coefficients in column 1 of Table 2 and those in column 5 of Table 1 is in the coefficient for managerial discipline, which was 5.60 for all workers (really, workers' "positions" or "slots") but becomes 1.76 for workers 3, 4, and 5 alone. This, of course, is just as one would expect, since the 5.60 includes the effect of the fact that workers 1 and 2 (particularly 2) were clearly faster than 1A and 2A.

---

[6] If the natural model is $y = \beta_0 + \beta_1 x_1 + \ldots + \beta_k x_k + \epsilon$, weighted analysis of the natural model is equivalent to unweighted analysis of the transformed model $y\sqrt{w} = (\beta_0 + \beta_1 x_1 + \ldots + \beta_k x_k)\, y\sqrt{w} + \epsilon^*$, where w is the number of weeks in the experimental period and the ϵ^*'s are assumed to be homoscedastic.

[7] How R^2 should be computed for a weighted regression—or, better, *what* R^2 should be computed—is a question without a unique answer. One may define R^2 to be the explained fraction of the variance of either $y\sqrt{w}$ (see previous note) or y; and in the latter case, one may compute the explained fraction of either the unweighted variance of y or the variance obtained when each y is weighted by the number of weeks it represents. The values of R^2 quoted in the present paper correspond to the last-named alternative.

If, however, one now examines the data on the productivities of workers 3, 4, and 5 in Franke and Kaul's (1978) Appendix 2—or, better, if one examines the graph of their weekly productivities in Roethlisberger and Dickson (1939:76)—it is hard to see any evidence of an abrupt increase in productivity at the end of period 7 which was maintained, unchanged, forever after. Rather, what one sees is an increase in period 8, when hours per day were reduced from 8¾ to 8¼; another increase in period 9, when hours were further reduced to 7¾; followed by a decrease in period 10, when hours were put back to 8¾. Inclusion of hours per day among the explanatory variables yields the regression coefficients in column 2 of Table 2, where R^2 has increased from .977 to .993 while the apparent effect of discipline has been reduced from 1.76 to .37 units/hour, or less than .7% of the average productivity of the concerned workers at the time the discipline was applied.

This result is not at all surprising. Workers 1A and 2A were not fired or punished in any ordinary sense of the word. They were merely taken out of the experiment and put back to work under ordinary conditions in "the department." What is more, they were not taken out of the experiment "because of their unsatisfactory attitudes in response to requests for greater diligence and more output," as asserted by Franke and Kaul (1978:627). They were removed because of behavior which risked disrupting the entire experiment, as evidenced by the fact that workers 3 and 4 refused to talk to them in period 7, and by the fact that both worker 4 and an auxiliary worker (called the "layout operator") "began to ask that either they or operators 1A and 2A be removed from the test room" (Roethlisberger and Dickson:55,169; Whitehead:117–9).

Table 2. Workers 3, 4, 5; Periods 1–13

	(1)	(2)	(3)
Discipline	1.76	.37	—
Rest time	.127	.114	.110
T1	.121	.134	.144
T2	.078	.102	.106
Hours/day	—	−3.33	−3.54
R^2	.977	.993	.993

One might well argue that the estimated effect of managerial discipline on workers 3, 4, and 5 is nothing but sampling error,[8] and, in any case, it is so small that elimination of discipline as an explanatory variable scarcely affects the coefficients of the other variables and leaves R^2 unchanged to 3 decimal places, as shown in column 3 of Table 2. Because attempts to construct an improved model of the effect of the passage of time would be seriously confused by inclusion in the list of explanatory variables of an adjustable step-function—which, at least for the moment, seems to explain almost nothing at all—discipline will be excluded from this list for the time being. Its effect will, however, be carefully reevaluated before any final conclusions are reached.

Dating of the depression. Column 5 of Table 1 implies that the crash on October 24, 1929, was immediately followed by an increase of .75 units/hour in the average hourly output of all workers, an increase which Franke and Kaul (1978) interpret as the result of job insecurity due to the depression. This result must be reexamined, however, because the crash in the stock market was not immediately and universally recognized as heralding a general economic depression. Exactly when the workers first began to fear loss of their jobs cannot be determined, but the first clear danger signal did not reach them until the start of period 17 on May 5, 1930, when both the length of the workday and the number of worksays per week were permanently cut back for economic—rather than for experimental—reasons.

The data uncontaminated by worker replacements which are available for reexamining the dating of the first effect of the depression consist of the productivities of workers 1, 2, 3, and 4 in periods 8 through 23. These data cover 92 weeks preceding the crash, 27 weeks from the crash to the cutback, and 95 weeks from the cutback to the end of the experiment. Data on worker 5 cannot be used because she was absent in periods 14–16, which include the crash and immediately precede the cutback.

---

[8] The stepwise regression program used by Franke and Kaul (1978) would reach this conclusion, since the (two-tail) P-level of the coefficient of managerial discipline is .144 in column 1 of Table 2 and becomes .634 when hours/day are introduced as an explanatory variable in column 2. The program would on the other hand accept hours/day as an explanatory variable because its two-tail P-level is .006.

The first two columns of Table 3 show the results of weighted regression of the average productivity of workers 1–4 in periods 8–23 on two very similar sets of explanatory variables. Both sets allow a linear trend in productivity up to the shock of the depression, a step at that point, and then another linear trend with altered slope. The only difference between the two sets is in the date of the step and the change in slope, which is October 24, 1979 ($t = 131$) in column 1, and the following May 5 ($t = 158$) in column 2. Formally, the three new variables in Table 3 are defined by

$$\begin{aligned} &T2^* = \max\{0, \min\{t,158\}-39\}, \\ &T3^* = \max\{0, t-158\}, \\ &\text{cutback} = 1 \text{ if } t > 158. \end{aligned}$$

The one noteworthy difference between the coefficients in columns 1 and 2 is in the coefficient of the step, which indicates an increase of .45 units/hour when dated to the crash, a decrease of 1.38 units/hour when dated to the cutback.

The difference between the .893 value of R^2 obtained with the step at the cutback and the .886 obtained with the step at the crash is much too small, by itself, to carry any weight—but there is very strong nonstatistical evidence pointing in the same direction. That the first effect of the depression should be dated to the cutback rather than to the crash is supported by the fact that no worker is reported by Whitehead (1938:148–9) as mentioning possible layoffs until immediately after the cutback. That the depression did not have a positive effect on productivity is supported by the fact that in all the subsequent conversations concerning layoffs reported by Whitehead (1938:149–51, 168–70, 178–85), there is no trace of belief by any worker that her job could be protected by increased output. That job insecurity actually had a negative effect on productivity is supported by evidence from outside the first Relay experiment. In the Mica experiment, the serious job insecurity which began to be felt in August of 1929, for a reason unconnected with the depression, was accompanied by a decrease in productivity, not by an increase (Roethlisberger and Dickson:153, 147).

Table 3. Workers 1, 2, 3, 4; Periods 8–23

	(1)	(2)	(3)
Rest time	.152	.149	.149
Hours/day	−.76	−1.02	−1.02
T2	.071	—	—
Crash	.45	—	—
T3	.005	—	—
T2*	—	.070	.070
Cutback	—	−1.38	−1.18
T3*	—	.004	—
R^2	.886	.893	.893

For all these reasons, the shock of the depression will be assumed in what follows to have really "hit" the workers in the Relay experiment with the cutback on May 5, 1930, at the start of period 17.

Changes in productivity after the cutback. The coefficient of T3* in column 2 of Table 3 implies that immediately after an abrupt drop due to the cutback, productivity increased very slowly—but continuously—up to the end of the experiment, 95 weeks later. It is impossible, however, to see evidence of such a trend in the data as presented either in Franke and Kaul's (1978) Appendices or in Whitehead's (1938) chart, H42. What inspection of the data does suggest is virtually constant productivity over the first 83 weeks (periods 17–21), followed by a sudden increase in the last 12 weeks (periods 22 and 23); and this visual impression is confirmed by the fact that regression of the average productivity of all workers on time during the first 83 weeks after the cutback yields a negative—rather than a positive—coefficient, −.009.

Although such a downtrend agrees with what was said earlier about the probable nature of the effect of the depression, its inclusion in the model would require the presence of not only the variable T3* but also a dummy with value 1 in periods 22 and 23 to prevent the downtrend from being converted into an uptrend. Since no causal interpretation of such a dummy has been offered, and since the total effect of the apparent downtrend amounts to only .009×83 = .75 units/hour over the entire 83 weeks that it may have persisted, the wiser course seems to be simply to drop the variable T3* from the model and leave all variation in productivity after the cutback "unexplained."

The effect of dropping T3* is shown in column 3 of Table 3, where there is a slight change in the coefficient of cutback but R^2 remains unchanged to 3 decimal places. The effect on the coefficients of the other variables is necessarily zero, since the presence in the model of the step-variable cutback, with a jump between periods 16 and 17, means that the coefficient of any variable other than cutback whose value is constant in periods 17–23 depends on neither the data for these periods nor the presence in the model of any variable other than cutback whose value is constant in periods 1–16.

MODELS FOR THE ENTIRE EXPERIMENT

Now that data on subsets of workers and periods have been used to decide some critical modeling questions, it is time to "put it all back together" by trying to explain the changing average productivity of all workers over the entire duration of the experiment. What makes the task "tricky" is the need to avoid confusing the changing productivity of workers with the effects of substitution of one worker for another.

Elimination of replacement effects. The only data covering the entire duration of the experiment that come to us uncontaminated by the effects of worker replacements are those for workers 3 and 4. Weighted regression of the average productivity of these two workers on the explanatory variables chosen for reasons already set forth yields the results in column 1 of Table 4.

To bring worker 5 into the analysis, one must replace the productivities of her very inferior substitute, 5A, in periods 14–16, by values which will let the contributions of these periods to the analysis depend on only the productivities of workers 1–4. Such values can be found by, first, regressing worker 5's productivity on the productivities of workers 1–4 in periods 8–13 and 17–23 and, then, computing regression "estimates" of the productivities of worker 5 in periods 14–16. The estimates thus computed are 58.8, 62.0, and 64.4; and, after the series for worker 5 has been patched with these values, regression analysis of the average productivity of workers 3, 4, and 5 over all periods yields the results in column 2 of Table 4.[9]

Finally, the data on workers 1A and 2A can be spliced with the data on 1 and 2 by multiplying the productivity of the former pair by a factor representing the inherent superiority of the latter pair.[10] The required factor can be estimated by, first, computing (1) the ratio of the total output of workers 1A and 2A to the total output of workers 3–5 in periods 1–7 and (2) the ratio of the total output of workers 1 and 2 to the total output of workers 3–5 in periods 8–23, and, then, dividing the results of (2) by those of (1). That result is 1.133; and, after the average productivity of workers 1A and 2A in each of the first 7 periods has been multiplied by this factor, regression analysis of the average productivity of all workers over all periods yields the results in column 3 of Table 4.

Table 4. All Periods; No Replacement Effects

	(1)	(2)	(3)
Hours/day	−1.92	−2.75	−2.45
Rest time	.106	.090	.108
T1	.234	.189	.167
T2*	.073	.074	.079
Cutback	−3.20	−3.08	−2.39
R^2	.915	.952	.972

(1) Workers 3,4; natural data.
(2) Workers 3,4,5; data for 5 adjusted.
(3) All but 5A; data for 1A, 2A, and 5 adjusted.

A smooth model of the effect of passage of time. Since replacement of workers 1A and 2A at the end of period 7 has been shown to have had little—if any—effect on the productivity of the other workers, there is no longer any justification for representing the effect of the passage of time over periods 1–16 as a piecewise-linear function with an abrupt change of slope at the end of period 7. The effect is more naturally represented as a smooth function which increases at a decreasing rate from the start of the experiment up to the first shock of the depression, at $t = 158$, and then remains constant to the end of the experiment, at $t = 253$. In order both to minimize the number of adjustable parameters in the model and to make a smooth transition between positive growth of productivity with time up to $t = 158$ and zero growth thereafter, the effect of time will be assumed to be proportional to a quadratic variable Q1 whose derivative with respect to time decreases gradually to 0 at $t = 158$ and remains 0 thereafter. Formally, this new variable is defined by

$$Q1 = x - x^2/316 \quad \text{where} \quad x = \min\{t, 158\}.$$

When Q1 is substituted for the T1 and T2* of Table 4, weighted analysis of the adjusted average productivity of all workers except 5A

---

[9] A case can be made for patching the series for worker 5 with estimates based on the regression of her productivity on the total productivity of workers 1–4 rather than their individual productivities, but the choice is unimportant because the estimates derived by this alternative procedure (59.4, 62.3 and 65.3) would lead to results equivalent for all practical purposes to those obtained using the estimates quoted in the text.

[10] The nonexistent productivities of workers 1 and 2 in periods 1 to 7 could of course be dummied-up by the same device used for worker 5 in periods 14 to 16. The reason for not so doing is simply that there is some hope of extracting some useful information from the actual performance of workers 1A and 2A in a crucial part of the experiment, whereas there seemed to be no hope of extracting anything useful from the actual performance of worker 5A in periods 14 to 16.

Table 5. All Workers Except 5A; Adjusted Data

	(1)	(2)	(3)	(4)
Hours/day	−2.72	−2.89	−2.82	−2.74
Rest time	.119	.137	.128	.130
Incentive	—	—	1.27	1.05
Defectv rm	—	—	—	−.77
Q1	.178	—	—	—
Q4	—	.167	.165	.173
Cutback	−.63	−.88	−.78	−1.16
R^2	.972	.972	.974	.975

yields the coefficients shown in column 1 of Table 5.[11] R^2 is as great for the new model as for the old even though the number of adjustable parameters has been reduced by one.

Incentive compensation. An improved, small-group wage incentive was introduced at the start of period 3, but, in trying to evaluate this change, the experimenters feared that its effect—if any—might be confused with "Hawthorne effect"; and, therefore, even before the first Relay experiment was concluded they conducted a brief, second Relay experiment in order to get independent evidence. The evidence thus obtained was hard to evaluate for a number of reasons (Roethlisberger and Dickson: chap. 6), but, such as it was, it "tended to substantiate the hypothesis that [the incentive] was an important factor" in productivity in the first Relay experiment (P. 133),[12] and it also gave some reason to think that the effect of the incentive was immediate (that is, abrupt) rather than gradual (p. 159).

If—contrary to this very weak evidence—the incentive *did* have any gradual effect, this effect must remain a statistically unidentifiable part of the whole set of effects of unmeasured variables which, together, make up the "effect of the passage of time." It is quite conceivable, for example, that the incentive acted as a multiplier of the effects of other, unmeasured variables.

The immediate effect of the incentive, on the other hand, may possibly be captured by Franke and Kaul's dummy variable which has the value 1 starting in period 3, but not if this variable is included in a model which also allows for a continuing increase of productivity with time, starting at $t = 0$. In such a model, the value and even the sign of the coefficient of incentive are extremely sensitive to very slight changes in the representation of the effect of time and to the presence or absence of other explanatory variables.

To avoid, insofar as possible, crediting time—that is, the unmeasured variables—with any part of the immediate effect of incentive, incentive will be introduced into a new model in which productivity is assumed not to increase with time at all until the start of period 4 at $t = 15$. The model is constructed by substituting for the variable Q1 a new variable Q4 defined by

$$Q4 = z - z^2/286 \quad \text{where} \quad z = \max\{0, \min\{t,158\}-15\}.$$

Lke the derivative of Q1, the derivative of Q4 with respect to time decreases smoothly to 0 at $t = 158$ and remains 0 thereafter.

Substitution of Q4 for Q1 without adding incentive to the model yields the results shown in column 2 of Table 5, where it should be observed that the coefficients are not very different from those based on Q1 in column 1 while R^2 is unchanged to 3 decimal places. Addition of incentive to this model then yields the results in column 3. The apparent immediate effect of the incentive is an increase in productivity of 1.27 units/hour.[13]

Defective raw materials. In periods 14 and 15, an unusually large number of defective component parts (called "raw materials" by Franke and Kaul [1978]) seem to have been supplied to the workers, and the Franke and Kaul data include a dummy variable which has the value 1 in these two periods. That this variable may have had some effect on productivity is indicated by the fact, pointed out by Franke and Kaul in their Model 3, that it is closely related to the amount of time the workers had to spend in repairing defective relays. Weighted regression of repair time on this variable alone explains 76% of the variance of repair time and indicates nearly 22 minutes per day of extra repair time during the affected periods.

---

[11] Strictly speaking, this analysis assumes that average productivity in any period is proportional to the value of Q1 at the midpoint of the period, not to the average value of Q1 in the period. Since any model of the effect of time that can be invented will be at best only a rough approximation to the truth, averaging would be a meaningless refinement.

[12] Franke and Kaul (1978:636) cite no source for their assertion that it was "contrary to the views of the Hawthorne researchers" that the incentive had a "positive influence" on productivity.

[13] A naive estimate of the immediate effect of the incentive may quite possibly be better than one based on a model involving other explanatory variables. By simply subtracting productivity in period 2 from productivity in period 3 one gets 51.0 − 49.1 = 1.9 units/hour or 3.9% as the immediate increase due to the incentive. No method of estimation has been found which yields anything like the 12.6% increase which immediately followed the introduction of the incentive in the second Relay experiment (Roethlisberger and Dickson, 1939:132), and this may well have been one of the reasons why Roethlisberger and Dickson concluded (pp. 133–4, 158, 577) that the 12.6% increase was in part a result of factors other than the incentive. Carey (1967:409), who violently attacks Roethlisberger and Dickson for this conclusion, gives no hint at his own explanation of the apparently much smaller immediate effect of the incentive in the first Relay experiment.

Addition of "defective raw materials" to the explanatory variables evaluated in column 3 of Table 5 yields the results in column 4. The indicated effect of the defective parts is a reduction in productivity of .77 units/hour, substantially less than implied by the 22 minutes per day of extra repair time.

Days per week. One more explanatory variable in the Franke and Kaul (1978) data, days/week, would seem promising but will remain unused. This variable enters virtually any model of the entire experiment with the wrong sign +, essentially because of the inexplicably low productivity in period 14, when days/week were experimentally reduced from 5-½ to 5. Separate analysis of the data from periods 17 to 23 only, where most of the variation in days/week took place, indicates a negligible increase of .33 units/hour for a reduction of half a day.

Rest Time. For a reason of the same kind as the one which prevents proper evaluation od days/week in a model of the entire experiment, the one which prevents proper evaluation of fect of rest time is, in fact, not so well founded as it seems. It depends too heavily on the low productivity in the single period 12 when rest stops were temporarily eliminated. If this one period is eliminated from the data, the coefficient .110 in column 3 of Table 2 is reduced to .044, the coefficient .130 in column 4 of Table 5 is reduced to .077, and so forth. In other words, elimination of rest stops seems to have reduced productivity much more than their introduction increased it.

IMPORTANCE OF THE PASSAGE OF TIME

Implications of the quadratic model. Since the model evaluated in column 4 of Table 5 includes all variables which seem to have had any material influence on productivity, it can be used as a basis for a rough evaluation of the importance of the passage of time relative to the experimental variables under managerial control, namely hours/day, rest time, and incentive. For comparison with other models to be introduced later, the description of this quadratic model is reproduced in column 1 of Table 6, where the numbers in parentheses will be discussed later on.

Statistically, time is by far the most important explanatory variable in the quadratic model, judging by the only method of evaluation that makes sense when the explanatory variables are highly correlated. Elimination of Q4 from the model reduces R^2 from .975 to .763, whereas elimination of the variable which is next most important in this sense—rest time—reduces R^2 only to .950.

And, what is even more relevant, Q4 is by far the most important variable economically. Its coefficient .173 implies that over the 143 weeks from the start of period 4 at t = 15 to the cutback at t = 158, passage of time was responsible for a total increase in productivity of $.173\times(143/2) = 12.4$ units/hour. This is to be compared with the 1.05 units/hour attributed by the model to incentive, 2.74 units/hour attributed to a reduction of one hour in the length of the workday, and $.130\times25 = 3.25$ units/hour attributed to 25 minutes of scheduled rest time, all of which taken together add up to less than 7.1 units/hour—or to less than 57% of the effect of the passage of time.

Alternative models. To test the sensitivity of these results to the choice of the variable Q4 in modeling the effect of time, two alternative models are described in columns 2 and 3 of Table 6. In column 2, the effect of time is assumed proportional to the square root of t-15 from t = 15 to t = 158 and constant thereafter. In column 3, it is assumed linear in t-15 from t = 15 to t = 158 and constant thereafter. Two facts are to be observed:

(1) In regard to the total effect on productivity of the passage of time, the square-root and linear models imply a growth from t = 15 to t = 158 of 15.1 and 15.6 units/hour, respectively—or substantially more than is implied by the quadratic model.

(2) The linear model attributes considerably more importance to the effects of the other variables than is attributed by the quadratic model, but the total effect attributed by even the linear model to incentive, a one-hour reduction in the workday, and 25 minutes of rest time is only 8.6 units/hour, or 55% of the 15.6 units/hour it attributes to the passage of time.

Statistical significance. Since explanatory variables have not been admitted to any model unless they entered with a prespecified sign, one-tail tests of significance are presumably called for, and, therefore, one-tail P-levels are shown in parentheses in Table 6. Two remarks seem called for:

(1) In the linear model described in column 3, the coefficients of all variables except incentive are significant at the .01 level, and the coefficient of incentive is almost significant at

Table 6.

	(1)	(2)	(3)	(4)
Hours/day	−2.74 (.036)	−2.18 (.086)	−3.29 (.007)	−3.70 (.022)
Rest time	.130 (.001)	.096 (.009)	.134 (.000)	.121 (.001)
Incentive	1.05 (.234)	.08 (.479)	1.93 (.056)	1.02 (.231)
Defectv rm	−.77 (.156)	−.73 (.187)	−2.70 (.000)	−2.82 (.002)
Q4	.173 (.000)	—	—	.196 (.000)
Square root	—	1.265 (.000)	—	—
Linear	—	—	.109 (.000)	—
Cutback	−1.16 (.178)	−1.64 (.120)	−5.72 (.000)	−2.14 (.110)
Discipline	—	—	—	1.49 (.089)
R^2	.975	.971	.983	.984

(1–3) All workers except 5A; adjusted data.
(4) All workers; unadjusted data.

the .05 level. I would argue, nevertheless, that this model is to be rejected out of hand because of the large value (−5.72) of the coefficient of cutback. Inspection of the data makes it obvious that productivity did not, in fact, drop by anything like this amount at the end of period 16. What is really certain is that by the end of period 16 the linear trend has seriously overshot the mark.

(2) In the quadratic model of column 1, the coefficients of incentive, defective raw materials, and cutback are nowhere near significant, evan at the .05 level. I would argue that the estimated coefficients of these variables are, nevertheless, the best available indications of what the data have to say; but it should also be noted that, if the offending variables are eliminated from the quadratic model, the implied total effect of time becomes 11.7 units/hour while the total effect of a one-hour reduction in the workday and 25 minutes of rest time becomes only 5.5 units/hour, or 47% of the effect of time.[14]

DISCIPLINE REEXAMINED

Early on, Franke and Kaul's (1978) "managerial discipline" was tentatively rejected as an explanatory variable with a promise that another look would be taken before any final conclusions were reached. As the first of two final checks, the *unadjusted* average productivity of all workers—exactly as it appears in the Franke and Kaul data—was regressed on the explanatory variables of the quadratic model plus "discipline." The results of weighted regression are shown in column 4 of Table 6.[15] The coefficient of "discipline" implies an increase in total output of only 5×1.49 = 7.45 units/hour, or less than the 12 or so units/hour one might have expected, as the direct effect of the replacement of workers 1A and 2A by the more rapid workers 1 and 2.[16] Of an effect of the "disciplinary example" there is no trace.

As a second check on discipline as an explanatory variable, the results of fitting the quadratic model to the unadjusted individual productivities of the workers are shown in Table 7. In columns 1 and 2 of this table, the pairs 1A,1 and 2a,2 are treated as single workers; in column 5, the results are based on the data for periods 1 to 13 and 17 to 23 only, excluding the periods during which worker 5 was absent all (or a large part) of the time. The direct effect of the replacement of workers 1A and 2A would appear to have been an increase of only 5.41 + 3.65 = 9.06 units/hour in total output—again, less than the 12 or so units/hour one might have expected as a result of the inherent superiority of workers 1 and 2. In regard to the other workers, the effect of the "disciplinary example" would appear to have been an increase of 1.86 units/hour in the output of worker 3, and decreases of .99 and 3.07

---

[14] When the three variables are dropped from the model, the coefficients of hours/day, rest time, and Q4 become respectively −.192, .143, and .163. R^2 is .971.

[15] In comparing the coefficients in column 4 with those in column 1, one must remember that in column 4 "defective raw materials" is picking up not only the effect of the defective parts but also the clearly inferior performance of worker 5A in periods 14 and 15.

[16] In discussing "Elimination of Replacement Effects," it was shown that, relative to workers 3–5, the total productivity of workers 1 and 2 was, on the average, 11.3% greater than that of workers 1A and 2A. Multiplying this percentage into the 107.9 units/hour produced by 1A and 2A in period 7 yields 12.19 units/hour as the increase in total units/hour one might have expected as the result of the replacement.

Table 7. Individual Productivities of Workers 1–5

	(1)	(2)	(3)	(4)	(5)
Hours/day	−2.59	−4.84	−1.73	−3.61	−6.20
Rest time	.135	.123	.144	.145	.052
Incentive	.07	−.03	3.42	.30	1.46
Defectv rm	−1.88	−1.08	−2.21	−.36	—
Q4	.177	.275	.106	.254	.187
Cutback	−.49	−2.58	−2.26	−3.73	−2.34
Discipline	5.41	3.65	1.86	−.99	−3.07
R^2	.951	.984	.924	.910	.939

units/hour in the outputs of workers 4 and 5, respectively, adding up to a net *decrease* of 2.18 units/hour.

It would, thus, seem to be established—about as clearly as such things can be—that managerial discipline, as distinct from personnel selection, was not a material factor in the history of the Relay Assembly Test Room. If one asks how a model in which time is replaced by discipline could yield a very high R^2 when fitted to the data, one need not seek far to find the answer. The coefficient of correlation between discipline and the smooth trend variable Q4 is .842. As Franke and Kaul included no smooth function of time among the predictor variables from which they asked their stepwise regression program to choose, the program took discipline as an excellent proxy and produced an excellent example of spurious correlation.

CONCLUSIONS

Despite uncertainty about the exact form of the relation between productivity and time, and despite very great uncertainty about the magnitudes of the effects of some of the other measured variables, there are two qualitative conclusions concerning the first Relay experiment that can be drawn, with considerable certainty, not from the models in Table 6 alone but also from the entire investigation which led to these models:

(1) The data agree very much better with models which include a smooth function of time as an explanatory variable than they do with models which do not. The exact form of the function is not at all critical. All that is required is that the function be capable of representing increase at a rate which becomes nearly (if not quite) zero, some time before the middle of the year 1930.

(2) Economically, the unmeasured variables for which time is a proxy will be, by far, the most important explanatory variables in virtually any model which allows their net effect to manifest itself in the way just described.

These two conclusions are, of course, nothing but restatements (in statistical jargon) of observations made by the original Hawthorne researchers more than 40 years ago. Digital computers and regression programs are not really needed to demonstrate the importance of "the effect of the passage of time," because mere inspection of Whitehead's (1939) chart H42 shows that the uniquely remarkable fact about the first Relay experiment is the almost continuous, "objectively" inexplicable growth of productivity with time over the first three years of the experiment. With the interpretation of this remarkable growth that was offered by the original researchers, other scholars are, of course, free to disagree; but they are not likely to find any plausible way of manipulating the data that allows them to deny its existence or its importance. The data, as such, are in all respects consistent with the conclusions reached by the original researchers.

Robert Schlaifer
Harvard University

REFERENCES

Carey, A.
1967 "The Hawthorne Studies: a Radical Criticism." American Sociological Review 32:403–16.

Durbin, J., and G. S. Watson
1950 "Testing for serial correlation in least squares regression, I." Biometrika 37:409–28.
1951 "Testing for serial correlation in least squares regression, II." Biometrika 38:159–78.

Franke, H. F., and J. D. Kaul
1978 "The Hawthorne experiments: first statistical interpretation." American Sociological Review 43:623–43.

Johnston, J.
1972 Econometric Methods (2nd ed.). New York: McGraw-Hill.

Roethlisberger, F. J., and W. J. Dickson
1939 Management and the Worker. Cambridge: Harvard University Press.

Whitehead, T. N.
1938 The Industrial Worker. Cambridge: Harvard University Press.

WORKER PRODUCTIVITY AT HAWTHORNE*

(Reply to Schlaifer)

A remarkable industrial experiment involving workers and managers of a major manufacturing company was begun in April 1927 and was continued until May 1933. The interpretation that increased productivity resulted from improved social relations, presented in influential reports published between 1933 and 1939, has provided a foundation to the present social science of work. Insofar as worker productivity is a matter for concern today,[1] it is more than an exercise in the history of science to reappraise the evidence that led to the early "human relations" conclusions.

The experiment provided for changes in working conditions and for extended close observation of assembly workers in the large plant of the Western Electric Company at Hawthorne Station near Chicago. It began in the regular assembly room to provide base-line data and, during the third week, was moved to the partially isolated "first relay assembly test room." The Western Electric experimenters studied only five direct production workers at a time, but they collected weekly (and daily) records that provide hundreds (thousands) of measures of performance and attitude. Within each of 23 periods that ranged from 2.3 weeks (11.6 workdays) up to 31 weeks (166 workdays), and for a final but disrupted 24th period that lasted 60 weeks, experimental conditions were held as nearly constant as may be possible in a complex field setting. Experimental variables were aspects of physical work environment and work requirements, changes in management and supervision, and changes in social relations of workers. Also occurring were some unintended alterations in working conditions which could be measured or otherwise attributed to certain portions of the six years of the experiment.

In our (cf. Franke and Kaul, 1978:639,640) report two years ago, which was the first systematic statistical analysis of worker productivity in the Hawthorne experiments, we made the period-by-period data from the first relay experiment accessible so that others could carry our work further. It is gratifying that, in addition to the critical comments of Wardwell (1979), an alternative statistical interpretation has now been offered by Robert Schlaifer (1980). Both these reviews have contributed to the present response which contains further and necessarily more detailed analysis. To encourage additional investigations, the weekly experimental data and a guide to records by the actual experimenters are provided here in Appendices 1 and 2.[2]

In his Comment, preceding, Schlaifer (1980) develops a number of models to explain the among-periods variance in hourly output rate of workers in the first relay experiment. He argues that his models, all of which include one or more independent variables that contain functions of time, are improvements in that "the data agree very much better" with them than do the data with the model presented by us. Our (Franke and Kaul, 1978:631, 633) Model 1 sought, instead, to explain group and individual productivity exclusively by measured experimental variables, both advertent and inadvertent. Model 1 uses but 3 variables—managerial discipline, the economic depression, and scheduled rest time, all of which had positive effects—to explain 97.1% of the variance in hourly output among periods. Schlaifer's best models in terms of data agreement (variance explanation) for all

---

* Direct all communications to: Richard H. Franke, Department of Management, Worcester Polytechnic Institute, Worcester, MA 01609.

I am grateful for the comments by Ralph Alexander, Gerald Barrett, Arthur Gerstenfeld, Leonard Goodwin, and Leon Graubard and, especially, for the detailed suggestions by Elke Franke and Douglas Woods.

This work has been supported in part by National Science Foundation Grant No. IST-7916483 for the study of the critical review process.

[1] Edward Denison (1979) has noted a lack of growth in U.S. labor productivity since 1973. However, long-term stagnation in capital (or managerial) productivity seems a more basic problem than is the still substantial long-term growth of labor productivity in manufacturing (cf. American Productivity Center, 1980; Franke, 1979a; Nystrom and Franke, 1977). Beyond economics, Albee (1977) and Dooley and Catalano (1980) summarize psychological and social effects of economic decline and other change.

[2] In further research on the Hawthorne experiments, the extensive quantitative and verbal information available in the Western Electric records should be consulted, at least for clarification of such information as has been published. The analyses and interpretations by the actual experimenters in the first relay assembly test room are worthy of study in the original. The microfilm copies of most of these records are deposited at the Hawthorne Plant, University of Wisconsin–Milwaukee, Worcester Polytechnic Institute, and Harvard University. Additional copies for research may be obtained from William Roselle, Director of the Golda Meir Library, University of Wisconsin—Milwaukee. The original records have been on deposit in the Archives of the Baker Library at Harvard since 1977. (Cf. also Franke, 1979b:fn. 3.)

periods are equation (4) in his Table 1 and equation (4) in his Table 6, the latter of which is repeated in his Table 7 for individual workers. His equations (here labeled Model T-1(4) and Model T-6(4)) use 6 and 7 variables to explain 98.3% and 98.4% of the variance in average hourly output rate of the group. Since these models employ the same unadjusted data base as Franke and Kaul's (1978) Model 1, direct comparison is possible.

In addition to attempting to demonstrate that his models are better, Schlaifer (1980) expresses concern with several statistical attributes of our (1978) Model 1:[3]

> (a) possible high interrelationships among independent variables used, or (roughly) *multicollinearity;*
> (b) likely unequal variance of regression equation residuals over periods that differ greatly in length, or *heteroscedasticity;* and
> (c) difficulty of testing for a time pattern in regression residuals that might show lack of serial independence, or *serial correlation (autocorrelation),* given the presumed condition of heteroscedasticity.

The following response asserts to the contrary that our (1978) Model 1 is not much worse in its agreement with the data than are Models T-1(4) and T-6(4) and that, from several points of view, Model 1 is to be preferred. It is relatively parsimonious and conveys simple meanings. The Model 1 elements:

> (a) do not show multicollinearity,
> (b) are supported in circumstances without presumed heteroscedasticity (in calculations that follow), and
> (c) are shown significant after correction for serial correlation.

In fact, Schlaifer's models include composite variables that ultimately confound rather than explain the behavior in the experiment. While in some cases he has dealt indirectly with the issue of heteroscedasticity, his models T-1(4) and T-6(4) are highly multicollinear, further confounding interpretation. They are also serially correlated, so that the tests for significance attempted in his Tables 6 and 7 cannot be performed.

The question as to whether Schlaifer's variables—and, thus, his equations—are meaningful is treated in the Method section where it will be demonstrated that further effort to adjust for technical problems in the alternative models is unwarranted. Means for dealing with multicollinearity, heteroscedasticity, and serial correlation are described prior to applying them in the quantitative analyses of the Results section.

METHOD

Data for More Precise Analysis

Weekly productivity data for individual operators and for the group were obtained from the original Hawthorne records, and these are presented in Appendix 1. The data are also summarized in Table 1, with results for some periods being different from those in Appendices 1 and 2 of Franke and Kaul (1978). The differences are slight (as shown by the .998 correlation between the earlier and the present group means) and probably result from the "estimates" for holidays or absences that had been included in the calculations of the Hawthorne experimenters and, thus, in our earlier periodic data.

Also in Table 1 are slope coefficients from regression of productivity of the group upon time for the weeks in each experimental period. These slopes indicate the extent of within-period rise or decline per week in group productivity. There were 15 periods with rises in productivity and 9 periods with declines. Five of the rises and four of the declines are or appear to be statistically significant. In order to summarize the productivity data both among and within periods, the regression lines of productivity against time are plotted for each period in Figure 1. Those lines that appear significantly different from horizontal are heavily shaded. Figure 1 illuminates those "within-period changes . . totally obscured by a statistical technique based on mean outputs for each period" (Wardwell, 1979:860), as do the statistical analyses of the weekly data for all periods (see Results section, herein). Examination of Figure 1 also allows initial evaluation of Schlaifer's (1980) statements that there was "gradual increase in productivity with time," . . . "an almost continuous, 'objectively' inexplicable growth of productivity with time over the first three years of the experiment." For the moment, it is left to the reader to determine from Figure 1 whether changes in productivity were gradual or sometimes abrupt and whether growth almost continuously occurred, or was interspersed with decline, from period 1 to period 16 (over the first 3 years).

Development of Composite Variables for Alternative Models

In constructing alternative statistical models, Schlaifer analyzed the residuals from

---

[3] Model 1 (Franke and Kaul, 1978) showed possible serial correlation, as measured by the Durbin and Watson (1951) coefficient and their Table 4. The model is presented in original and variously corrected forms in Schlaifer's. Table 1, columns (1) through (3).

Table 1. Productivity Means, Standard Deviations, and Regression Slopes within Periods

	Means and Standard Deviations for Individuals and Group[a]												Regression Slope Coef. for Group[b]
Operator:	1A+1		2A+2		3		4		5+5A		Group		
Period													
1	50.2	(1.0)	49.2	(2.4)	50.2	(0.2)	49.1	(1.1)	48.5	(0.3)	49.5	(0.5)	−0.70
2	48.0	(1.3)	48.9	(3.4)	49.5	(3.4)	51.3	(2.7)	49.0	(1.4)	49.3	(2.1)	−0.66
3	48.4	(2.5)	50.4	(2.5)	53.7	(1.2)	52.2	(1.5)	50.6	(1.1)	51.0	(1.6)	0.46[c]
4	51.5	(1.2)	50.7	(1.3)	53.6	(1.8)	53.5	(1.2)	50.8	(0.6)	52.1	(0.8)	0.15
5	54.1	(1.3)	55.4	(1.7)	57.0	(1.2)	56.0	(1.2)	53.0	(1.0)	55.1	(0.2)	0.10
6	54.1	(0.8)	53.6	(2.0)	55.7	(1.9)	55.7	(1.4)	53.5	(1.2)	54.5	(0.6)	0.25
7	54.1	(1.8)	53.9	(2.2)	58.7	(2.0)	58.2	(2.5)	54.2	(1.5)	55.8	(1.2)	0.14
8	62.8	(1.3)	64.5	(2.2)	62.2	(1.1)	63.1	(0.8)	56.8	(1.0)	61.9	(0.9)	0.34[c]
9	65.5	(1.3)	68.0	(0.7)	63.0	(0.9)	63.6	(1.5)	59.5	(1.5)	63.9	(0.4)	−0.17
10	63.9	(2.2)	64.9	(1.9)	62.1	(1.7)	62.8	(1.8)	55.3	(1.8)	61.8	(1.1)	0.05
11	65.6	(1.4)	66.5	(1.6)	63.9	(1.3)	62.9	(0.6)	55.0	(2.8)	62.8	(1.0)	0.13
12	62.4	(0.9)	63.8	(0.9)	59.7	(1.5)	61.3	(0.9)	56.1	(1.0)	60.7	(0.5)	−0.05
13	67.5	(2.2)	72.1	(3.3)	64.4	(2.5)	69.1	(3.3)	59.8	(2.1)	66.6	(2.2)	0.19[c]
14	66.1	(1.5)	71.8	(1.5)	63.0	(2.4)	70.2	(1.2)	51.3	(6.4)	64.7	(1.3)	−0.27
15	70.1	(2.7)	75.1	(2.2)	65.0	(2.1)	71.9	(2.4)	54.0	(4.0)	67.3	(1.9)	0.10[c]
16	73.7	(4.8)	77.8	(3.9)	68.4	(1.8)	73.5	(3.8)	60.6	(1.1)	70.8	(2.8)	−2.06[d]
17	70.6	(3.2)	76.4	(3.5)	66.4	(2.6)	74.2	(4.2)	62.1	(3.2)	69.9	(2.9)	0.29[d]
18	69.8	(3.4)	77.3	(2.5)	65.7	(2.3)	73.2	(4.4)	65.6	(1.6)	70.3	(2.2)	−0.26[d]
19	74.1	(2.5)	75.9	(2.3)	65.1	(2.5)	70.3	(3.2)	66.2	(1.6)	70.3	(1.5)	−0.23[d]
20	73.4	(3.2)	76.1	(2.3)	64.5	(1.8)	68.2	(3.9)	65.3	(1.6)	69.5	(1.7)	0.16
21	70.3	(1.6)	72.5	(2.0)	67.0	(1.4)	68.9	(2.8)	67.7	(0.3)	69.2	(1.5)	0.80
22	74.8	(2.6)	75.9	(2.7)	68.6	(3.6)	72.9	(5.3)	65.9	(2.0)	71.6	(2.6)	0.57
23	77.2	(1.9)	78.0	(—[e])	66.9	(1.7)	73.1	(1.2)	67.1	(2.3)	71.5	(1.3)	0.05
24[f]	71.4	(3.6)	73.0	(3.5)	64.9	(2.7)	69.1	(2.8)	62.0	(2.0)	68.1	(1.3)	−0.19[c]

[a] Period 1 (11.6 days) and period 2 (24.9 days) means were calculated for the days actually in the periods. For other periods, the means were calculated using weekly data only.

[b] Regression slopes are the units change from week to week in standard relays produced per operator per hour. For period 1, the first 1.6 days in week 3 (prior to the move from the regular relay assembly room to the experimental room) were counted as part of week 2. For period 2, which is after the move to the first relay assembly test room, only the last 3.9 days were counted here as week 3. However, the data reported in Appendix 1 are for the weeks as they occurred, without this adjustment.

[c] The relationship of productivity with time (weeks) within the period is significant ($p<.05$).

[d] The relationship of productivity with time (weeks) within the period appears significant ($p<.05$), but significance cannot be tested without correcting for the possible serial correlation indicated by Durbin-Watson test ($p<.05$).

[e] Operator 2 worked only one week during period 23, so the standard deviation among weeks could not be calculated.

[f] Data are only for the first 17 weeks of period 24 (weeks 254 and 270 of the experiment), before the first replacement of an experienced operator by an inexperienced one.

Franke and Kaul's (1978) Model 1 to arrive at variables that allow for the passage of time and explain some of the residual. As only 3% of the periodic variance in productivity was unexplained by Model 1, this analysis of the residuals could hardly, in itself, lead to inclusion of additional variables in models that improve much on Model 1's agreement with the data. Indeed, the residual analysis as carried out does not provide further explanation but, instead, incorporates most of Franke and Kaul's model into the new variables of the alternative models.

What is striking about Schlaifer's (1980) residual Figure 1 and his interpretation of it is that he has not chosen the boundaries and explanations shown by the residual plot but, rather, those suggested by Franke and Kaul's (1978) Model 1. The figure and his new variables, T1, T2, and T3, are apportioned into (1) the span until just before the imposition of managerial discipline, (2) the span between this and the onset of economic depression, and (3) the span after the beginning of the depression. Schlaifer finds that regressions of residuals on time (mid-period week) explain, respectively, 53.8%, 26.4%, and 27.1% of the residual variance within each of these spans. But during span (1), the small-group incentives that were introduced in period 3 could explain a higher 67.4% of the difference in residuals without consideration of time as such. For span (2), examination of Schlaifer's Figure 1 suggests that a much better plot against time would be

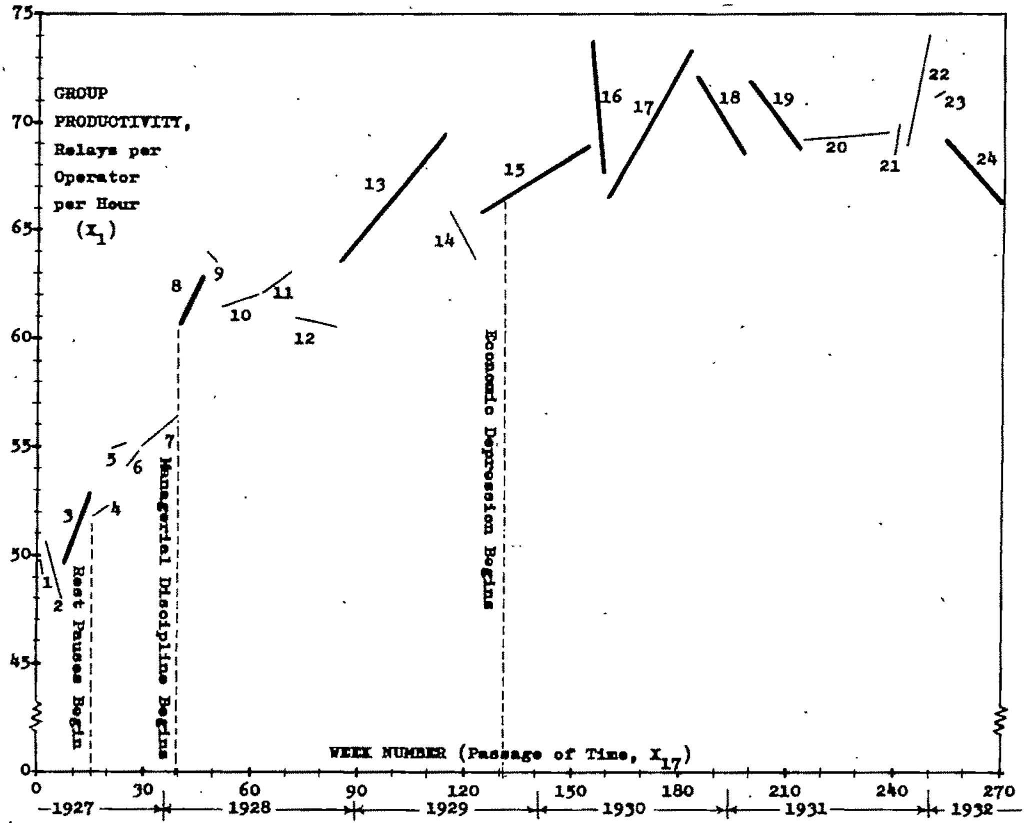


Figure 1. Worker Productivity Trends in Periods 1 to 24 of the First Relay Experiment at Hawthorne

Note: Within-period regression slopes of productivity on time are those presented in Table 1. Heavily shaded regression lines are or appear to be significantly different from horizontal.

from period 8 to period 13, which yields an r^2 of 71.2% as opposed to his 26.4%. (Schlaifer actually calculated his r^2 using periods 8 to 14; for period 8 through the first 26% of period 15—i.e., up to the depression—an r^2 of 42.5% would have been obtained.) For span (3) (apparently calculated by Schlaifer for periods 16 to 23, with a slight error) not much can be done, even if it is enlarged to include periods 14 and 15 and/or period 24. There simply is not much relationship of the residual with time (r^2 = 3.6% from period 14 to 24, 16.6% from period 15—last 74%—to 24, or 28.6% from period 16 to 23).

Instead of contributing much to explanation of productivity differences (and, after all, there was only 3% left to explain), the residual analysis, through its peculiar choice of boundaries, incorporates Franke and Kaul's (1978) managerial discipline and economic depression. This is shown in Figure 2, herein,[4] where variable T1 is composed not only of the passage of time but also of Franke and Kaul's (1978) first variable, as its rise halts abruptly with the imposition of managerial discipline and correlates .84 with it.[5] Even T1's time-increase segment may be part of a Franke and Kaul variable, as much of it parallels the experimentally introduced rise in rest time during span (1) (r = .68). Span (2)'s composite variable T2 also contains the passage of time, but it clearly begins with managerial discipline and ends with the start of the economic depression (r's with these are .72 and .81). In span (3), T3 begins with the depression and is correlated .77 with that categoric variable of Franke and Kaul. Finally (skipping over five of his intermediate and derivative variables), Schlaifer's most encompassing measure of "time," Q4, begins with the introduction of Franke and

---

[4] Figure 2 may be examined vis-à-vis Figure 1's productivity data for initial judgment whether growth in productivity was really "inexplicable," as concluded by Schlaifer (1980).

[5] Cf. Table 3, herein. The intercorrelation measures of multicollinearity presented are over all 257 weeks of work for the group. For those individual operators who worked some weeks fewer, the correlation coefficients may be slightly different.

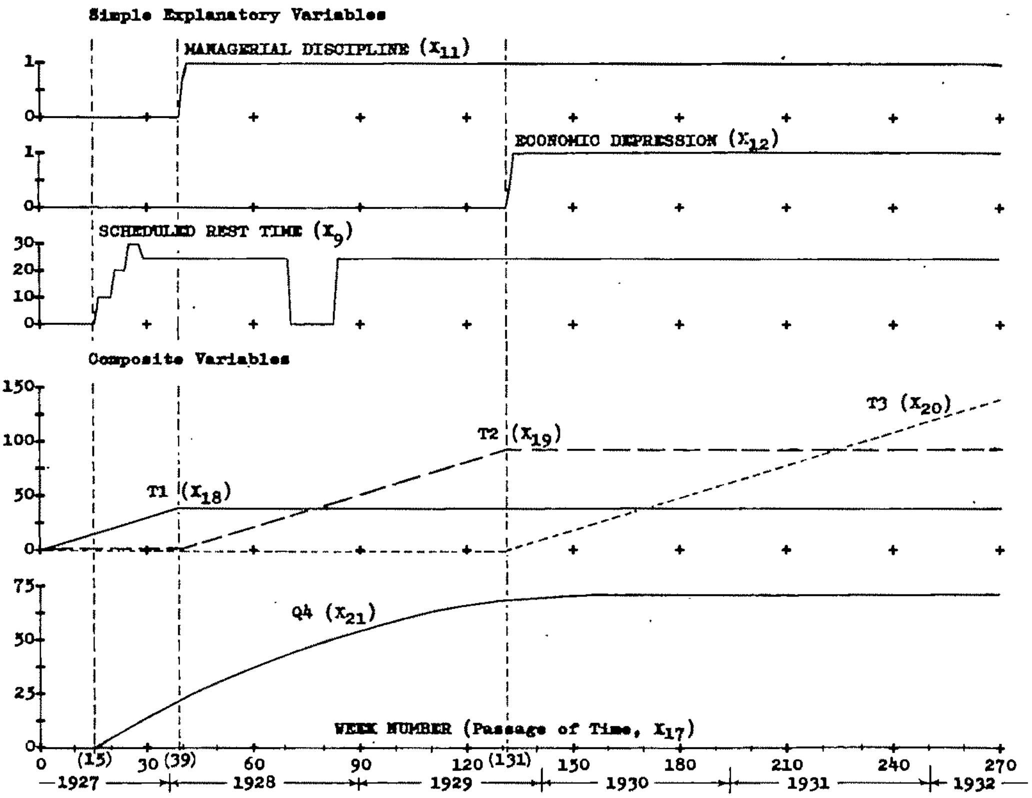


Figure 2. Relationships of Composite Variables to Simple Explanatory Variables

Kaul's (1978) third variable, rest time (r = .54), is shown to rise substantially following managerial discipline (r = .85), and comes nearly to completion (value of 69 vs. the maximum of 71.5) with the economic depression (r = .72). Because the three variables of our (1978) Model 1 intercorrelate from .38 to .46, the high correspondence of Q4 with all three variables is noteworthy.

As is shown statistically and (in Figure 2) graphically, the composite variables introduced seem to yield little additional information beyond that offered by the three variables of Model 1 (Franke and Kaul, 1978). Therefore, further consideration of these Schlaifer variables does not seem necessary. Nevertheless, the "passage of time," which Schlaifer's variables were intended to convey, might have effects which should be investigated further (cf. Schlaifer, 1980:fn.2). In addition, the logarithm of time suggested by learning theory[6] will also be added to Model 1 (Franke and Kaul, 1978) to ensure that the concerns of Schlaifer for the gradually decreasing influence of added time are not neglected.

Analytical Procedures for Time-Series Regression

There are at least three areas of technical concern in the use of statistics for the present endeavor: heteroscedasticity, multicollinearity, and serial correlation. Schlaifer (1980) adjusts data to reduce heteroscadasticity by weighting with the number of weeks in each experimental period, but he does not test any of his models for possible multicollinearity or serial correlation. He observes that the data would not show heteroscedasticity "if we had available the average productivity of the workers in each individual week of the experiment" and that "if the weekly data were available one would be obliged to investigate the error structure". Apparently unbeknown to Schlaifer, the weekly data are available in the original records. Use of them, however, requires some effort due to bulk (there are 313 weeks in all) and due to the need to calculate many of the weekly figures from daily records.[7] In the analyses that follow, only weekly data are used from the beginning of the first period of the first relay experiment in April 1927 through the 17th week of the 24th period in June 1932, after which the experienced operators (first 5A; then 1 and 3; 4; and, finally, 2) were laid off and replaced by inexperienced but more senior operators.[8]

---

[6] Cf. the standard-learning equation applied by Pitcher (1981) to the first relay data.

[7] Similar calculation was required to obtain productivity data for periods 21 to 23 in the study by Franke and Kaul (1978). The averages for periods 1

Clearly, heteroscedasticity can be avoided through assembly of data for about-equal time periods. Similarly, strategies can be used to avoid multicollinearity.[9] In stepwise multiple regression with forward inclusion, the first independent variable chosen is that with the highest t value for its slope coefficient of the dependent variable on it. Variables continue to be added so long as there is a t value (or partial correlation coefficient) that attains the significance level chosen. (In this study, a liberal t of 1.65 is used as lower limit, for $p < .10$, 2-tailed.) The addition of each variable can be restricted to a "tolerance"—the fraction of the variance of a potential variable that is not explained by other independent variables already entered. Tolerance may be set at or above .36 to avoid extreme multicollinearity (in the case of two independent variables, to maintain intercorrelation below .8), or at .75 to avoid moderate multicollinearity (for only two independent variables, to maintain intercorrelation below .5). In the following regression analyses, Model 1 (Franke and Kaul, 1978) is replicated using weekly data (Table 4), and then all meaningful variables including time are allowed to enter without controlling for multicollinearity (Table 5). Finally, meaningful variables are allowed to enter stepwise multiple regressions for the group and for individual productivity, but with control for multicollinearity at a tolerance level of .75 (Table 6). By means of comparison of the results in the final two tables—one without and the other with control for multicollinearity—we may check to see if the major findings are limited to either imperfect approach to the problem of interrelated explanatory variables.

The problem of serial correlation in time-series studies is not that bias is introduced into least squares regression equations, but that tests for significance of regression coefficients, such as those carried out by Schlaifer in his Tables 6 and 7, are inaccurate.[10] Thus, all

---

to 20 were available in the Western Electric progress reports. Daily and weekly data for period 24 are available in the original log books (cf. Franke, 1979b, fn. 3). Weekly data for periods 2 through 13 (to week 24) are provided by the progress reports, but other weekly data for periods 1 and 13 to 23 had to be calculated from daily data in those reports. The 270 weeks of the experiment solely with experienced relay assemblers include 257 weeks of work, 8 weeks of vacation for all operators, and 5 weeks of "days off schedule" due to lack of work. There were 251 weeks during which all five operators were at work.

[8] In his study of period 24 of the first relay experiment, Andrew Spaulding (1980) shows the importance of both learning and managerial discipline (a second episode) to the productivity of the initially inexperienced operators.

[9] Franke and Kaul (1978:629–30) noted, regarding multicollinearity, that "most intercorrelations of these independent variables are smaller than their correlations with the dependent variable." In the AAAS version of the report, they also provided an intercorrelation matrix of independent variables, allowing some further appraisal of multicollinearity (cf. Table 3, herein). In such matrices, intercorrelations above .8 (in absolute terms) are sometimes taken as evidence of extreme multicollinearity, while correlations below .5 indicate the lack of (or low) multicollinearity. Moderate or extreme multicollinearity can lead to unreliable regression coefficients which may change radically upon addition or deletion of a few observations (Elliott, 1973). Perhaps the most commonly employed remedy for multicollinearity is to drop one or more of the offending variables (cf. Dhrymes, 1978). Although this rule of thumb and corrective procedure are faulted (cf. Dhrymes, 1978; Farrar and Glauber, 1967), there seems to be lack of agreement as to more definitive measures and procedures (cf. Hanushek and Jackson, 1977; Kumar, 1975; O'Hagan and McCabe, 1975). Perhaps a better measure of absence of multicollinearity is "tolerance," the fraction of the variance of an explanatory variable that is not explained by prior explanatory variables in the regression equation. Tolerance can be used conveniently in regression with the Statistical Package for the Social Sciences to avoid multicollinearity beyond any set limit by restricting entry of additional independent variables (cf. Kim and Kohout, 1975:346).

[10] A widely used test for serial correlation is that of Durbin and Watson (1950). An equation is deemed possibly serially correlated and thus unworthy of testing for significance if the Durbin-Watson coefficient (DW) is below the upper limit provided by Durbin and Watson (1951:173) at $p < .05$. Then, the continuous dependent variable is adjusted by a factor roughly equal to $1 - DW/2$ (cf. Theil and Nagar, 1961; Wallis, 1972:94). In the experiment, as viewed weekly, none of the independent variables moves continuously; so there seems little point to adjusting them. (Trial correction of the independent variables as well as of the dependent variable shows little difference from correction of only the dependent variable.) Only the 251 work weeks for which all operators were present were used in correction. The process of correction is iterative, with successive estimation of correction factors by which (multiplied times values in time t-1) values in time t are reduced, until the corrected data yield an equation with nonsignificant Durbin-Watson coefficient. In response to Schlaifer's (1980:fn.3) concern over the meaning of serial correlation, note Durbin and Watson's (1950:410–1) focus on the violation of the least

Table 2. Experimental Correlations—Group and Individual Productivity[a]

	Productivity, units/worker/hr.					
Variables Operator:	Group	1A+1	2A+2	3	4	5+5A
Dependent Variables						
(1-G) Productivity, group						
(1-(1A+1)) Productivity, Oper. 1A+1	.96					
(1-(2A+2)) Productivity, Oper. 2A+2	.97	.95				
(1-3) Productivity, Oper. 3	.91	.84	.84			
(1-4) Productivity, Oper. 4	.93	.84	.90	.88		
(1-(5+5A)) Productivity, Oper. 5+5A	.76	.71	.65	.60	.56	
Independent Variables						
Work Schedule						
(4) Hours per day	−.55	−.50	−.48	−.41	−.40	−.72
(5) days per week	−.35	−.33	−.29	−.23	−.25	−.49
(6) Net hours per week[b]	−.52	−.48	−.45	−.38	−.39	−.66
Rest Pauses						
(8) Scheduled Rest Stops, no./day	.30	.28	.27	.32	.28	.22
(9) Scheduled Rest Time, min./day	.63	.59	.59	.64	.59	.41
Categoric Changes in Working Conditions[c]						
(11) Managerial Discipline	.83	.83	.85	.76	.75	.51
(12) Economic Depression	.71	.70	.68	.55	.61	.66
(12L (12-Lagged)) Cutback	.62	.58	.55	.45	.47	.78
(13) Defective Raw Materials	.11	.14	.22	.13	.26	−.40
(14) Temp. Replacement of Oper. 5[d]	.18	.23	.26	.22	.29	−.28
(15) Small-Group Incentive	.42	.39	.40	.49	.39	.29
(16) Operators Change Seats[e]	.35	.22	.35	.30	.41	.32
Simple Measure of Time						
(17) Passage of Time	.83	.83	.80	.68	.70	.76
Composite Measures of Time and Categoric Changes (see Figure 2)						
(18) T1	.78	.76	.75	.78	.71	.50
(19) T2	.88	.86	.90	.73	.84	.61
(20) T3	.58	.60	.50	.43	.39	.74
(21) Q4	.93	.91	.94	.81	.87	.63

[a] These Pearson product-moment correlations are for all weeks common to each pair of variables indicated (257 weeks for the group and for operators 1A+1, 254 for operators 2A+2, and 256 weeks for operators 3, 4, and 5+5A). Two independent variables treated in the earlier paper (Franke and Kaul, 1978) are excluded here: variable (7), weeks per period, and variable (10), voluntary rest time. (At least some of the information in weeks per period is conveyed through use here of weekly data. Voluntary rest time may be interpreted as a dependent variable which resulted from fatigue or perhaps from dissatisfaction. For the group over 23 periods, it is correlated −.84 with scheduled rest time.)

[b] Hours per day times days per week, minus scheduled rest time.

[c] Change indicated = 1, otherwise = 0.

[d] Operator 5 was replaced voluntarily by operator 5A from week 6 of period 14 to week 2 of period 17, and from week 2 of period 24 until operator 5A was laid off in week 18 of period 24. Operator 5 did not return again to the experiment.

[e] During experimental periods 16, 17, and 18, the operators' order of seating was changed from the usual 1-2-3-4-5 to 4-2-5-3-1. Judging from the operators' comments in the Western Electric records (cf. Franke, 1979b, fn. 3) and from the selections chosen by Whitehead (1938:I), this change substantially affected social relations and satisfaction. This variable was not included in the analysis of Franke and Kaul (1978).

equations to be presented in the three regression tables that follow are those uncorrected for serial correlation. But significance tests are performed on corrected equations, for whose regression coefficients t-tests and significance levels are reported. Variables are not entered in equations if they showed multicollinearity in the basic equations (when the tolerance re-

---

squares assumption that "successive errors are distributed independently of one another," and their question as to why they usually are not distributed independently using time series data. Clearly, the answer often is that there is some lack in model specification. Pindyck and Rubinfeld (1976:107) point out that positive serial correlation frequently occurs "because of the high degrees of correlation over time present in the cumulative effects of the omitted variables in the regression model." In response to Schlaifer's footnote 4, it may be noted that the approximate transformation called T_2 by Johnston (1972:261) provides "generally a close approximation" to the exact transformation, T_1, and is useful in an iterative process such as that described herein or by Johnston (1972:262–3).

Table 3. Independent Variable Correlations over the 257 Weeks of Work[a]

Variable Number:	(4)	(5)	(6)	(8)	(9)	(11)	(12)	(12L)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
(5)	.39															
(6)	.65	.94														
(8)	−.15	−.09	−.21													
(9)	−.30	−.21	−.38	.81												
(11)	−.34	−.22	−.33	.11	.46[b]											
(12)	−.63	−.51	−.64	.19	.38[b]	.43[b]										
(12L)	−.82[c]	−.65	−.80	.15	.30	.35	.81									
(13)	.33	.18	.23	.08	.15	.18	.09	−.34								
(14)	.37	−.02	.07	.09	.19	.21	.27	−.16	.75							
(15)	−.13	−.06	−.14	.39	.47	.39	.17	.14	.07	.08						
(16)	−.46	−.25	−.36	.08	.16	.18	.44	.45	−.18	−.08	.07					
(17)	−.60	−.59	−.71	.21	.46	.62	.87	.85	.01	.21	.28	.25				
(18)	−.29	−.19	−.32	.37	.68[d]	.84[d]	.36	.29	.15	.18	.66	.16	.55			
(19)	−.46	−.43	−.53	.19	.47	.72[d]	.82[d]	.66	.30	.39	.28	.35	.90	.60[d]		
(20)	−.62	−.65	−.75	.14	.29	.33	.77[d]	.89	−.26	.02	.13	.12	.90	.28	.63[d]	
(21)	−.43	−.39	−.50	.22	.54[c]	.85[c]	.72	.59[c]	.26	.33	.38	.32	.86	.77	.97	.56

[a] During the 270 weeks from April 26, 1927, to June 17, 1932, the group worked 257 weeks. Numbers are Pearson product-moment correlation coefficients for these 257 weeks.

[b] Triangle: Interrelationships of independent variables in Model 1, now using weekly data.

[c] Apparent multicollinearity in Models T-6(4) and T-7(1-5).

[d] Apparent multicollinearity in Model T-1(4).

striction is used); also, variables are not entered if they are nonsignificant (p > .10, 2-tailed) after correction for serial correlation.

I summarize—if I may—the methodological approach described here: Weekly data from the first relay experiment have been introduced in Table 1, Figure 1, and Appendix 1; the roots of Schlaifer's composite variables have been shown to lie within the experimental variables delineated by Franke and Kaul (1978); means of avoiding or correcting for the statistical problems of heteroscedasticity, multicollinearity, and serial correlation have been introduced for use in the analyses that follow. The stages of analysis that can now be undertaken will be those of the study by Franke and Kaul (1978, AAAS and American Sociological Review versions): Zero-order correlation of dependent variables with independent variables (Table 2); zero-order intercorrelation of independent variables (Table 3); and regression models that first replicate our Model 1 (Table 4), then allow entry of the "passage of time" and all other meaningful independent variables without controlling for multicollinearity (Table 5) and, finally, control for multicollinearity and attempt to maximize variance explanation using the weekly data (Table 6).

RESULTS

Zero-Order Relationships

The productivity measures over the 257 work weeks of the first relay experiment for the group and the five individual operators (with replacements) are correlated in Table 2 with each other and with all independent variables. Substantial (over 25%) variance explanations by single independent variables are provided by managerial discipline, the depression, and (except for operators 5+5A) by scheduled rest time. The depression lag of just over six months—named "cutback" as there was a reduction in working hours then to 40 hours per week—related better than the depression itself to the productivity of operators 5+5A, but less well for other operators. The simple passage of time and three of the four composite variables also related substantially to productivity.

The interrelationships of independent variables in Table 3 indicate little likelihood of multicollinearity for the independent variables of Franke and Kaul's (1978) Model 1 (variables 9, 11, and 12), but there are eleven instances of high intercorrelation for the two alternative models (involving, primarily, variables 18 through 21).

Replication of Model 1 with Weekly Data

Replication of Franke and Kaul's (1978) Model 1 period-by-period equations in Table 4 using the weekly productivity data yields slightly lower variance explanation—ranging from 71% for operators 3 and 4 up to 86% or 87% for the group and for operators 1A+1 and 2A+2 (for whom operator replacement in period 8 contributed to the strength of the "managerial discipline" effect). Serial correlation could be corrected for all individual operators and for the group, and effects of managerial discipline and the depression were significant. Regression coefficients for managerial discipline were considerably higher when weekly data were used than when periodic data were used, while effects of the depression were lower. Coefficients for the effects of rest time were generally not much different whether using data for weeks or for periods, but this variable was not significant for operators 5+5A. For the group as a whole, managerial discipline appears to have contributed about 10 additional units produced per worker per hour, the depression about 5 units, and rest time of 25 minutes per day about 4½ units, for a total of about 19 additional units per hour per worker. This accounts for 86% of the rise in group productivity from base period 1 to peak period 22 (see Table 1).

Addition of Time and Other Variables without Control for Multicollinearity

As shown in the intercorrelation matrix of Table 3, variable 17 (the passage of time, measured in chronological weeks of the experiment) is highly collinear with the depression (variable 12) and moderately so with managerial discipline (variable 11). Thus, in the expanded regressions of Table 5, which include time as well as variables 4 to 6 and 8 to 16 (see Table 2) without controlling for multicollinearity, time enters first in the equation for the group. For individual operators, however, other variables enter prior to time—managerial discipline is first in four cases and second in the fifth.

Through inclusion of further variables, variance explanation is increased for the group from 87% (Table 4) to 92% (Table 5), and increases for individuals are similar or greater. In all cases, the effect of time is significant, but the effects of managerial discipline (with one exception) and rest time remain, as well. Models were also attempted in which the logarithm of time was substituted for time, approximating the shape of learning curves in industrial situations.[11] For the group, and for each operator, the resulting variance explanation was lower than when the straight-line passage of time was included. (The model for the group only is presented in fn. b to Table 5.)

The high multicollinearity in each model of Table 5 makes interpretation ambiguous. Most models show sign reversal and diminishing of coefficients of early-entered variables as multicollinear later variables enter. Some control of multicollinearity would be useful in sorting out the statistics of the first relay experiment. However, there is no sure way to exert this control. Therefore, the following results are presented as a possible approach, for juxtaposition with the multicollinear results of Table 5.

Comprehensive Models without Multicollinearity

In these regressions, all work-schedule, rest-pause, and categoric-change variables are free to enter, subject to the limitation that they show a .75 tolerance and significance at $p < .10$ after correction for serial correlation. Time and, then, the logarithm of time were added to the potential explanatory variables in separate sets of runs. The resulting comprehensive models for the group and for individual operators are shown in Table 6. In no case did a model that included a measure of the passage of time provide the highest variance explanation.

For operators 2A+2 and 3, Franke and Kaul's (1978) Model 1 gave the greatest explanation. For operators 5+5A, an addition to Model 1 is variable 14: the replacement of operator 5 by the initially less-experienced operator 5A led to a drop of some 8 units per hour from the productivity rate of operator 5, and was reflected by a drop of 1-1/3 units in the average group rate. For operators 1 and 4, variable 16 is added to Model 1: the movement of operator 1's seat away from operator 2 (the group leader) seems to have resulted in a decrease of 2 units in operator 1's productivity, while the move placing operator 4 next to operator 2 led to an increase of over 4 units for operator 4. (This operator, who usually produced the lowest quantity of defective products, seems to have been stimulated even by the challenge of coping with poor quality raw materials, as shown by the positive entry of variable 13.)

DISCUSSION

In these correlations and regressions, the specific factors of Model 1 (Franke and Kaul, 1978) provided the most powerful and consistent explanations of productivity differences for the experiment with experienced assembly workers in the first relay test room. In other experimental situations at Hawthorne, similar specific factors could be identified as significant to productivity.[12] The demonstration of

---

[11] Cf. Baloff (1966); Pitcher (1981).

[12] Beyond the effects (i) in the first 23 periods of the first relay experiment—from managerial control, economic pressure, and fatigue reduction, positive productivity effects have been found:

Table 4. Replication of Periodic Model 1 Using Weekly Data—Group and Individual Productivity (X_1)[a]

X_1 for Oper.	Intercept	Managerial Discipline	Economic Depression	Scheduled Rest Time	Variance Explanation	Durbin-Watson Coef.
Group	50.56	$+ 9.74 X_{11}^{(1)}$	$+ 4.77 X_{12}^{(2)}$	$+0.18 X_9^{(3)}$	87.19%	0.72[b]
		$t = 12.68$	$t = 9.26$	$t = 5.69$	(70.97%)	(1.84)[c]
		$(p < .001)$	$(p < .001)$	$(p < .001)$	Factor = 0.5	
1A+1	49.26	$+ 12.59 X_{11}^{(1)}$	$+ 5.78 X_{12}^{(2)}$	$+ 0.18 X_9^{(3)}$	85.86%	1.04
		$t = 13.73$	$t = 9.14$	$t = 4.55$	(71.61%)	(1.98)
		$(p < .001)$	$(p < .001)$	$(p < .001)$	Factor = 0.4	
2A+2	49.20	$+ 15.32 X_{11}^{(1)}$	$+ 6.15 X_{12}^{(2)}$	$+ 0.21 X_9^{(3)}$	86.82%	0.80
		$t = 13.78$	$t = 7.92$	$t = 4.39$	(69.75%)	(1.90)
		$(p < .001)$	$(p < .001)$	$(p < .001)$	Factor = 0.5	
3[d]	52.38	$+ 6.68 X_{11}^{(1)}$	$+ 1.85 X_{12}^{(3)}$	$+ 0.19 X_9^{(2)}$	71.29%	0.95
		$t = 6.97$	$t = 3.04$	$t = 5.19$	(42.92%)	(1.91)
		$(p < .001)$	$(p < .01)$	$(p < .001)$	Factor = 0.5	
4	51.39	$+ 10.03 X_{11}^{(1)}$	$+ 4.23 X_{12}^{(2)}$	$+ 0.23 X_9^{(3)}$	70.61%	0.78
		$t = 7.36$	$t = 4.61$	$t = 4.17$	(45.60%)	(2.01)
		$(p < .001)$	$(p < .001)$	$(p < .001)$	Factor = 0.5	
5+5A[e]	51.89	$+ 4.56 X_{11}^{(2)}$	$+ 6.36 X_{12}^{(1)}$	—	50.36%	0.30
		$t = 2.49$	$t = 6.18$		(21.77%)	(1.96)
		$(p < .02)$	$(p < .001)$		Factor = 0.7	

[a] These regression equations replicate week by week the period-by-period analyses of Franke and Kaul (1978:Tables 2 and 3). Superscripts in parentheses in the equations indicate order of entry of variables in stepwise multiple regression.

[b] All regression equations presented in this report use the time-series productivity data in Appendix 2 without correction, and all show significant serial correlation so that significance tests cannot be performed. The t values and significance tests shown are for the equations corrected using the factor indicated (cf. Franke and Kaul, 1978:628).

[c] After correction for serial correlation, this and all other Durbin-Watson coefficients presented show nonsignificant serial correlation. The equation after correction for serial correlation is used only to determine significance levels, which is possible because the data have been made serially independent. The corrected equations are not reported here, but their variance explanations and Durbin-Watson coefficients are shown in parentheses.

[d] Replication with weekly data of the independent multiple regression for Operator 3 in Table 3 of Franke and Kaul (1978) reverts to Model 1 after correction for multicollinearity.

[e] After correction for serial correlation, X_9 is nonsignificant. The replication with weekly data of the independent multiple regression for Operators 5+5A in Table 3 of Franke and Kaul (1978) yields:

		Defective Raw Mtls.	Economic Depression	Net Hours per Week	Managerial Discipline
$X_{1\text{-}(5+5A)}$	$= 57.29$	$- 7.75 X_{13}^{(2)}$	$+ 5.59 X_{12}^{(1)}$	$- 0.12 X_6^{(4)}$	$+ 5.71 X_{11}^{(3)}$
		$t = 9.80$	$t = 7.44$	$t = 1.71$	$t = 5.93$
		$(p < .001)$	$(p < .001)$	$(p < .10)$	$(p < .001)$

Variance explanation (R^2) = 76.21%, Durbin-Watson coef. (DW) = 0.70 (after correction for serial correlation using factor of 0.5: R^2 = 57.73%, DW = 1.81)

Note that the order of variables presented above is that for the equation using periodic data (from Franke and Kaul, 1978:Table 3), and is different from the order here using weekly data, indicated by superscripts in parentheses. (There is moderate multicollinearity—indicated by tolerance of 0.49—at the entry of the fourth variable, net hours per week. The variance explanation before that entry is 75.50%.)

(ii) in the illumination experiments—from illumination increases and from "social hysteresis," or reluctance to retreat from productivity levels once attained (Wiljanen, 1979);

(iii) in the second relay and mica splitting experiments—from small group incentives and from fatigue reduction (Franke and Kaul, 1978:fn.9); and

(iv) in the 24th period of the first relay experiment (with inexperienced operators who clearly had something to learn)—from the logarithm of learning time and from managerial control (Spaulding, 1980).

these tangible causes of worker productivity contradicts any suggestion that resorts to the possible effects of "human relations" or placebo mechanisms upon productivity.

A much more detailed set of data has been used here than was used in the first attempt to test statistically the effects of the major Hawthorne experiment upon worker productivity. As it is difficult to maintain as high a variance explanation for 257 weeks as for 23 periods,[13] it

---

[13] Upon consideration of the still unaccounted-for effects of short-term changes in personal affairs, at-

Table 5. Comprehensive but Multicollinear Productivity Models—Group and Individuals (X_1)[a]

	Intercept	Time	Mgrl. Disc.	Scheduled Rest Time	Scheduled Rest Stops	Seat Chg.	Days per Week	Temp. Repl. of Op.	Ec. Dep.	Cut-back	Hours per Day	Other	Var. Expl.	Durbin-Watson Coef.
Model Reg. Coefs. for:		X_{17}	X_{11}	X_9	X_8	X_{16}	X_5	X_{14}	X_{12}	X_{12L}	X_4		R^2	DW
Group:[b]														
Entry:		(1)	(2)	(3)		(4)	(5)	(6)		(7)	(8)			
Model:	62.46	+0.079	+5.11	+0.14	—	+3.66	+0.95	−2.28	—	−6.75	−2.06	—	92.04%	1.18
Tolerance:		—	.61	.74		.93	.60	.91		.07	.21		(82.25%)	(1.97)
t =		8.25	6.11	5.66		5.70	3.31	3.20		3.95	2.22		Factor = 0.4	
p <		.001	.001	.001		.001	.01	.01		.001	.05			
Operators 1A + 1:[c]														
Entry:		(2)	(1)	(3)			(4)		(5)					
Model:	39.44	+0.045	+9.85	+0.15	—	—	+1.74	—	+1.83	—	—	—	88.74%	1.28
Tolerance:		.61	—	.74			.61		.23				(80.30%)	(1.96)
t =		5.87	11.36	4.96			4.67		2.17				Factor = 0.3	
p <		.001	.001	.001			.001		.05					
Operators 2A + 2:[d]														
Entry:		(4)	(1)	(3)		(6)	(5)	(8)	(2)	(9)				
Model:	39.54	+0.085	+9.99	+0.16	—	+5.32	+1.63	−3.61	+0.76	−5.37	—	—	91.15%	1.23
Tolerance:		.17	—	.76		.68	.62	.39	.82	.06			(80.65%)	(2.02)
t =		5.60	8.38	4.33		5.25	3.79	2.86	0.43	2.15			Factor = 0.4	
p <		.001	.001	.001		.001	.001	.01	NS	.05				
Operator 3[e]												X_{15}	(Small-Group Incent.)	
Entry:		(3)	(1)	(2)	(7)	(5)	(6)					(4)		
Model:	43.27	+0.020	+4.09	+0.26	−1.10	+1.61	+1.23	—	—	—	—	+3.79	76.48%	1.15
Tolerance:		.58	—	.79	.25	.93	.59					.73	(56.35%)	(1.87)
t =		4.71	3.92	4.53	2.34	2.58	2.98					2.57	Factor = 0.4	
p <		.001	.001	.001	.05	.02	.01					.02		
Operator 4:[f]												X_{13}	(Defect. Raw Mtls.)	
Entry:		(6)	(1)	(3)	(8)	(4)	(7)		(2)			(5)		
Model:	41.20	0.058	+5.22	+0.27	−0.90	+6.71	+1.88	—	−3.32	—	—	+4.52	80.36%	1.18
Tolerance:		.13	—	.76	.26	.81	.57		.81			.90	(69.46%)	(1.87)
t =		5.79	4.11	3.94	1.70	7.61	3.97		2.57			5.23	Factor = 0.3	
p <		.001	.001	.001	.10	.001	.001		.02			.001		
Operators 5 + 5A:[g]												X_{13}	(Defect. Raw Mtls.)	
Entry:		(4)	(2)	(7)				(3)	(5)	(1)	(8)	(6)		
Model:	69.33	+0.065	+1.21	+0.08	—	—	—	−6.00	+4.80	−7.52	−2.27	−5.44	86.44%	1.13
Tolerance:		.09	.88	.73				.89	.17	—	.17	.34	(73.88%)	(1.97)
t =		6.30	0.99	2.72				5.82	4.93	4.71	2.17	5.48	Factor = 0.4	
p <		.001	NS	.01				.001	.001	.001	.05	.001		

[a] For the group and each (pair of) individual(s), the order of entry, model coefficients, and tolerance at entry are presented for the original model; t-test and significance level are shown for each independent variable that was significant at entry for the model after correction for serial correlation. Since variables are not restricted from entry even though multicollinear, there are some sign reversals and diminishing of earlier effects of independent variables as results of multicollinearity.

[b] Extreme multicollinearity for X_{12L} and X_4. Multicollinearity may be responsible for sign reversal of X_{12L}. The model when log of time is substituted for time is (in order of entry):

	$X_{1\text{-}G} = 58.91$	$+10.22(\log X_{17})$	$+3.61\ X_{11}$	$-2.25\ X_4$	$+0.17\ X_9$
Tolerance:		—	.36	.76	.66
t=		10.28	3.02	4.17	3.79
p <		.001	.01	.001	.001

	$-5.29\ X_{15}$	$+1.41\ X_{16}$	$+0.10\ X_6$	$-0.59\ X_8$
Tolerance:	.46	.60	.77	.25
t =	3.82	3.02	2.53	1.78
p <	.001	.01	.02	.10

$R^2 = 90.41\%$, DW = 0.96 (after correction for serial correlation using factor of 0.4: $R^2 = 81.36\%$, DW = 1.84).

[c] Extreme multicollinearity only for X_{12}.

[d] Extreme multicollinearity for X_{17} and X_{121}. Multicollinearity may be responsible for the diminishing of the effect of X_{12} and for sign reversal of X_{12L}.

[e] Extreme multicollinearity only for X_8. Multicollinearity may be responsible for its sign reversal.

[f] Extreme multicollinearity for X_{17} and X_8. Multicollinearity may be responsible for sign reversals of X_{12} and X_8.

[g] Extreme multicollinearity for X_{17}, X_{12}, X_{13}, and X_4. Multicollinearity may be responsible for the diminishing of the effect of X_{11} and for the sign reversal of X_{12L}.

is encouraging that for the group there is a drop if only 10% using the Franke and Kaul (1978) Model 1—from 97.1% to 87.2%. In the comprehensive models of Table 6, there are modest drops in variance explanation from those in Franke and Kaul's (1978) Table 3 for operators 1A+1, 2A+2, and 5+5A, a large drop for operator 3, and about the same explanation for operator 4—traceable to consideration here of the change in seating position. This move placing operator 4 next to the informal group leader for 10 months of the experiment appears to have contributed 4.41 units per hour to operator 4's productivity. Considering as a part of managerial discipline the continuing leadership of operator 2 as "straw boss," there is a total managerial discipline effect on operator 4 of 14 units additional hourly production, equivalent to the effects for operators 1A+1 and 2A+2 (where replacement or "personnel selection," as well as disciplinary example, was at work).

This reappraisal supports the earlier explanations of the changes in worker productivity at Hawthorne (Franke and Kaul, 1978; Franke, 1979b). The main factors affirmed here are managerial discipline, the economic depression, and the provision of rest pauses. While the potential of the last variable for reducing fatigue and increasing productivity has been well established—at least since the concept of scientific management enjoyed currency and "fatigue laboratories" existed in the early decades of this century—the mechanisms of the variables labeled managerial discipline and economic depression are less obvious.

Managerial Discipline

Managerial discipline in the first relay experiment (Franke, 1979b: 862-4) included threats and punishment by management and the provision of close "supervision" by the informal—but clearly dominant and domineering—group leader (replacement operator 2). It is not clear from the available literature just why managerial discipline was imposed on January 25, 1928, when both previously and subsequently other behavioral and productivity problems with various operators

---

titudes, health, weather, holidays, etc., further variance explanation could be expected. Indeed, it is apparent from looking closely at the weekly data that the proximity of certain holidays (especially Christmas) did have adverse effects on productivity, and Whitehead (1938) has reported some adverse effects from lack of sleep. Detailed consideration of these short-term factors would be especially important to statistical analysis of the day-by-day productivity data.

did not prompt major intervention by management.

The surviving records of the experiment might, however, provide an answer. Comments of operators 1 and 2, later relabeled 1A and 2A, as recorded for January 23, 1928, indicate that they were restricting their output—a common practice in many industrial facilities and, for that matter, elsewhere at Hawthorne (cf. Homans, 1950). In this experiment, though, restriction of output was viewed as a serious offense by management and led to summary removal of the offenders. This lesson obviously was learned by the other operators, and it is reasonable to assume that it accounts for much of the higher productivity levels attained during period 8 and later.[14] To substantiate this interpretation of the disciplinary intervention, the reader is referred to the documents that record the dialogue of the original operators 1 and 2 and the specific actions and opinions of management on January 23, 24, and 25, 1928.[15]

As discussed earlier (Franke, 1979b:862–3; Franke and Kaul, 1978), managerial discipline was not simply a matter of replacing two obstinate workers by two better ones. The intervention also provided a disciplinary example and closer supervision internal to the group. The day-by-day descriptions of the experiment in the original documents underpin this interpretation, which is given additional support by the regression coefficients for remaining operators 3, 4, and 5+5A in Tables 4, 5, and 6, above.[16]

Economic Depression

That the economic depression seems to have had early effects on productivity is puzzling, since workers might not have been expected to be particularly aware of the importance of the crash on October 24, 1929. However, two pieces of information help explain their sensitivity to this stimulus. First, as described by Whitehead (1938:I:137), the operators had been made aware in June 1929 that lessened volumes of work (for reasons that had nothing to do with the depression) might affect their schedules and earnings. Second, the Hawthorne records show that some of the operators in the test room as well as workers elsewhere in the plant were themselves participants in the stock market,[17] so that they could have been aware of the crash as an economically disturbing event. Thus, it is by no means implausible that the beginning of the economic depression had some influence upon the participants in the experiment. Nevertheless, the present analysis shows this influence on productivity to have been weaker than was indicated earlier (1978) by Franke and Kaul. In addition, as shown in the alternate comprehensive model for operators 5+5A in Table 6, the decrease in volume of work six months later may have been as important as was the beginning of the depression for operator 5A.

CONCLUSION

There was a 45% increase of worker productivity in the first relay experiment at Hawthorne. Most of the week-by-week differences in productivity rate during the experiment have been explained (for the group as well as for the individuals in it) by specific factors that are documented as having occurred during the experiment—managerial discipline, depression, and rest periods. Contrary to the conclusions reached by Schlaifer, one need not rely on "unmeasured variables for which time is a proxy" to explain productivity differences. In fact, the inclusion of time as a variable does not add to explanation once the few pertinent experimental variables are considered.

Since, in this case, digital computers and statistical methods systematically applied to voluminous experimental data do seem capable of contradicting long-held beliefs by providing a reliable description of what happened at Hawthorne, it is surprising that a statistical exercise such as Robert Schlaifer's could assert nothing more than "the importance of the effect of the passage of time." Rigorous testing with data for weeks as well as for periods, and for individuals as well as for the group, shows clearly that specific experimental variables did lead to the achieved increases in productivity.

Richard Herbert Franke
Worcester Polytechnic Institute

---

[14] Cf. Figure 1 for the group and Table 1 and Appendix 1 for individuals.

[15] Cf. *Daily History Record,* microfilm reel 1, listed as box 2, folder 7.

[16] In Table 6, the regression coefficients under managerial discipline average 7.0 for the continuing three operators, as against 14.6 where replacement occurred in period 8. The disciplinary example and internal supervision effect of 7.0 is just under half the full effect for operators 1A+1 and 2A+2, suggesting that the magnitude of the disciplinary aspects as such nearly equals the effect of replacement where it occurred. Since the disciplinary aspects of managerial discipline could affect all of the operators, they then can be held to account for most of the impact of the variable at the group level.

[17] Cf. *Daily History Record* for June 4, August 14, and August 27, 1928 (Microfilm reel 1, listed as box 2, folder 7).

Table 6. Comprehensive Productivity Models Without Multicollinearity—Group and Individuals (X_1)[a]

Intercept	Managerial Discipline (X_{11})	Economic Depression (X_{12}) or Resulting Cutback (X_{12L})	Scheduled Rest Stops (X_8) or Rest Time (X_9)	Temporary Replacement of Oper. 5 by Oper. 5A (X_{14})	Change in Order of Seating (X_{16})	Defective Raw Materials (X_{13})	Variance Explanation	Durbin-Watson Coef.
Comprehensive Model for Group:[b]								
50.96	$+11.26X_{11}^{(1)}$	$+5.27X_{12}^{(2)}$	$+1.22X_{8}^{(3)}$	$-1.33X_{14}^{(4)}$	—	—	86.34%	0.73
	$t = 14.98$	$t = 10.08$	$t = 4.27$	$t = 2.35$			(70.32%)	(1.81)
	$(p < .001)$	$(p < .001)$	$(p < .001)$	$(p < .02)$			Factor =	0.5
Comprehensive Model for Operators 1A + 1:								
49.52	$+13.91X_{11}^{(1)}$	$+6.73X_{12}^{(2)}$	$+1.29X_{8}^{(3)}$	—	$-2.15X_{16}^{(4)}$	—	86.03%	1.10
	$t = 15.88$	$t = 10.04$	$t = 3.78$		$t = 2.85$		(71.80%)	(1.99)
	$(p < .001)$	$(p < .001)$	$(p < .001)$		$(p < .01)$		Factor =	0.4
Comprehensive Model for Operators 2A + 2 (= Model 1):								
49.20	$+15.32X_{11}^{(1)}$	$+6.15X_{12}^{(2)}$	$+0.21X_{9}^{(3)}$	—	—	—	86.82	0.80
	$t = 13.78$	$t = 7.92$	$t = 4.39$				(69.75%)	(1.90)
	$(p < .001)$	$(p < .001)$	$(p < .001)$				Factor =	0.5
Comprehensive Model for Operator 3 (= Model 1):								
52.38	$+6.68X_{11}^{(1)}$	$+1.85X_{12}^{(3)}$	$+0.19X_{9}^{(2)}$	—	—	—	71.29%	0.95
	$t = 6.97$	$t = 3.04$	$t = 5.19$				(42.92%)	(1.91)
	$(p < .001)$	$(p < .01)$	$(p < .001)$				Factor =	0.5
Comprehensive Model for Operator 4:								
51.58	$+9.59X_{11}^{(1)}$	$+2.81X_{12}^{(2)}$	$+0.22X_{9}^{(3)}$	—	$+4.41X_{16}^{(4)}$	$+3.27X_{13}^{(5)}$	75.82%	0.97
	$t = 8.65$	$t = 3.32$	$t = 4.70$		$t = 4.63$	$t = 3.47$	(58.24%)	(1.92)
	$(p < .001)$	$(p < .01)$	$(p < .001)$		$(p < .001)$	$(p < .001)$	Factor =	0.4
Comprehensive Model for Operators 5 + 5A:[c]								
50.49	$+4.69X_{11}^{(3)}$	$+7.32X_{12}^{(1)}$	$+0.11X_{9}^{(4)}$	$-7.85X_{14}^{(2)}$	—	—	77.29%	0.68
	$t = 4.92$	$t = 12.22$	$t = 2.90$	$t = 11.69$			(60.02%)	(1.84)
	$(p < .001)$	$(p < .001)$	$(p < .01)$	$(p < .001)$			Factor =	0.6

[a] Independent variables included in the regression attempts are variables 4 to 6, 8, 9, and 11 to 17 (described earlier in Table 2.) The "passage of time" is included, but not the composite variables (18 to 21) which are based on two or more of the simple variables and, thus, are highly multicollinear and indistinct in meaning (cf. Figure 2 and Table 3, shown earlier). The equations presented in Table 6 are the results of (1) avoidance of multicollinear variables by entering variables only if their tolerances (variance fraction not explained by independent variables already in the stepwise regression) are 0.75 or higher and (2) avoidance or removal of remaining variables which, after correction for serial correlation, have regression coefficients which are not significant at $p < .10$ (2-tailed).

[b] This equation results from stepwise multiple regression without inclusion of the passage of time. Its variance explanation (86.34%) is greater than that with the passage of time included (79.98%), in which were entered variables 17, 9, 16, 15, and 13 (DW = 0.48). In the third step of the equation in Table 6, it is X_8 (rest stops) rather than X_9 (rest time) that enters, since the tolerance for X_9 is 0.74980—just below the 0.75 limit set. Because number of rest stops explains less productivity variance than does rest time, the total explanation of this model is slightly less than that of Model 1 in Table 4. (However, for all individuals, the variance explanations in Table 6 exceed those in Table 4. The Table 6 explanations are also greater than those by the equations in Table 5, if these equations are carried further to correct for multicollinearity as has been done in Table 6.) Further comprehensive model testing inserting the log of the passage of time in place of variable 17 yields variance explanation of 85.37%, higher than when using variable 17 but lower than the explanation using models not including a measure of time.

[c] This equation is superior in variance explanation to that run when variable 12L is used (which then enters in preference to variable 12). This independently arrived at but slightly inferior model is:

Comprehensive Model for Operators 5 + 5A:								
50.34	$+4.91X_{11}^{(2)}$	$+6.96X_{12L}^{(1)}$	$+0.11X_{9}^{(4)}$	$-4.07X_{14}^{(3)}$	—	—	75.25%	0.60
	$t = 4.03$	$t = 8.84$	$t = 2.57$	$t = 4.85$			(46.69%)	(2.00)
	$(p < .001)$	$(p < .001)$	$(p < .02)$	$(p < .001)$			Factor =	0.6

Appendix 1. Weekly Measures of Productivity in the First Relay Experiment

Work Dates	Period	Week in Period	Passage of Time (Weeks)	Productivity, Relays per Hour per Oper. for Operators:						Days per Week
				1A+1	2A+2	3	4	5+5A	Group	
4/26–30/27	1	01	1	51.1	51.3	50.4	48.2	48.3	49.9	4.5
5/2–7/27	1	02	2	50.5	48.1	50.0	49.9	48.7	49.4	5.5
5/9–14/27	1,2	01	3	47.6	45.6	50.2	52.9	48.0	48.9	5.5
5/16–21/27	2	02	4	50.2	54.5	52.6	54.0	50.8	52.4	5.5
5/24–28/27	2	03	5	48.1	47.0	50.3	51.1	47.8	48.8	4.5
5/30/27–6/4/27	2	04	6	46.7	45.7	43.7	48.1	47.7	46.4	5.5
6/6–11/27	2	05	7	47.2	48.9	51.0	49.9	50.1	49.4	5.5
6/13–18/27	3	01	8	47.4	48.9	51.6	50.0	50.4	49.7	5.5
6/20–25/27	3	02	9	46.4	48.1	52.9	51.0	51.8	50.0	5.5
6/27/27–7/2/27	3	03	10	44.4	46.6	52.5	—	50.1	48.4	5.5
7/5–9/27	3	04	11	48.8	49.5	54.4	51.2	48.7	50.5	4.5
7/11–16/27	3	05	12	47.5	51.2	54.5	53.8	52.1	51.8	5.5
7/18–23/27	3	06	13	50.3	52.4	54.4	53.9	50.4	52.3	5.5
7/25–30/27	3	07	14	52.1	53.3	54.3	52.9	50.9	52.7	5.5
8/1–6/27	3	08	15	50.7	53.4	54.8	52.7	50.4	52.4	5.5
8/8–13/27	4	01	16	52.9	—	53.1	52.6	51.0	52.4	5.5
8/15–20/27	4	02	17	52.1	48.9	55.2	53.3	50.4	52.0	5.5
8/22–27/27	4	03	18	49.9	50.6	—	54.5	50.2	51.3	5.5
8/29/27–9/3/27	4	04	19	50.6	51.6	51.4	52.2	51.6	51.5	5.5
9/6–10/27	4	05	20	51.8	51.7	54.9	55.0	—	53.4	4.5
9/12–17/27	5	01	21	54.2	56.4	56.0	54.4	54.2	55.0	5.5
9/19–24/27	5	02	22	52.8	53.0	58.7	56.9	53.2	54.9	5.5
9/26/27–10/1/27	5	03	23	55.8	56.8	56.1	55.8	51.9	55.3	5.5
10/3–8/27	5	04	24	53.7	55.6	57.0	57.1	52.6	55.2	5.5
10/10–15/27	6	01	25	55.2	55.9	54.1	54.0	51.8	54.2	5.5
10/17–22/27	6	02	26	53.9	54.6	54.4	56.2	54.2	54.7	5.5
10/24–29/27	6	03	27	53.2	51.4	55.9	55.3	53.7	53.9	5.5
10/31/27–11/5/27	6	04	28	54.0	52.7	58.3	57.3	54.4	55.3	5.5
11/7–12/27	7	01	29	55.2	54.9	57.4	55.4	54.3	55.4	5.5
11/14–19/27	7	02	30	54.9	56.0	57.8	56.4	55.6	56.1	5.5
11/21–23,25,26/27	7	03	31	55.2	55.4	57.9	56.3	54.5	55.8	4.5
11/28/27–12/3/27	7	04	32	56.4	56.6	57.8	56.6	54.6	56.4	5.5
12/5–10/27	7	05	33	54.7	55.2	55.7	54.9	53.4	54.8	5.5
12/12–17/27	7	06	34	53.3	52.9	58.6	58.6	50.4	54.8	5.5
12/19–24/27	7	07	35	49.4	49.7	57.7	58.6	53.2	53.7	5.5
12/27–30/27	7	08	36	53.2	52.5	58.5	61.1	53.7	55.8	4.0
1/3–7/28	7	09	37	53.2	52.0	59.7	59.1	55.8	56.0	4.5
1/9–14/28	7	10	38	55.0	51.9	61.8	60.4	55.4	56.9	5.5
1/16–21/28	7	11	39	54.5	56.3	62.6	62.5	55.0	58.2	5.5
1/23–28/28	8	01	40	60.7	59.9	62.1	62.0	56.0	60.1	5.5
1/30/28–2/4/28	8	02	41	63.0	64.7	61.6	63.5	56.8	61.9	5.5
2/6–11/28	8	03	42	62.4	64.6	60.4	62.5	55.9	61.2	5.5
2/13–18/28	8	04	43	62.8	66.7	62.8	63.6	57.3	62.6	5.5
2/20–25/28	8	05	44	62.0	64.5	63.8	63.7	58.8	62.6	5.5
2/27/28–3/3/28	8	06	45	64.0	66.2	62.2	64.0	56.7	62.6	5.5
3/5–10/28	8	07	46	64.8	65.1	62.6	62.6	56.2	62.3	5.5
3/12–17/28	9	01	47	64.8	68.5	63.7	64.9	59.1	64.2	5.5
3/19–24/28	9	02	48	64.5	67.0	63.8	64.5	61.0	64.2	5.5
3/26–31/28	9	03	49	65.3	68.1	62.8	63.3	57.6	63.4	5.5
4/2–7/28	9	04	50	67.3	68.5	61.9	61.6	60.2	63.9	5.5
4/9–14/28	10	01	51	65.3	66.9	61.6	60.7	56.6	62.2	5.5
4/16–21/28	10	02	52	67.7	69.0	61.4	61.1	58.0	63.4	5.5
4/23–28/28	10	03	53	62.2	62.9	58.8	60.2	56.5	60.1	5.5
4/30/28–5/5/28	10	04	54	63.5	63.8	60.5	62.0	55.7	61.1	5.5
5/7–12/28	10	05	55	60.1	62.9	64.0	61.7	54.9	60.7	5.5
5/14–19/28	10	06	56	61.3	63.3	61.9	62.7	55.7	61.0	5.5
5/21–26/28	10	07	57	62.0	63.6	65.1	64.6	51.6	61.4	5.5
5/28,29,31/28–6/2/28	10	08	58	64.6	64.9	61.8	64.2	53.2	61.7	4.5
6/4–9/28	10	09	59	65.8	67.1	63.6	65.9	55.4	63.6	5.5
6/11–16/28	10	10	60	63.9	64.4	62.3	64.2	56.7	62.3	5.5
6/15–23/28	10	11	61	64.9	65.7	62.9	63.8	55.7	62.6	5.5

Appendix 1. (Continued)

Work Dates	Period	Week in Period	Passage of Time (Weeks)	Productivity, Relays per Hour per Oper. for Operators:						Days per Week
				1A+1	2A+2	3	4	5+5A	Group	
6/25–30/28	10	12	62	65.5	64.5	61.2	62.1	53.3	61.3	5.5
7/2,3,5,6/28	11	01	63	65.2	66.7	62.7	61.7	52.4	61.7	4.0
7/9–13/28	11	02	64	64.7	65.6	65.4	63.2	53.2	62.4	5.0
7/16–20/28	11	03	65	66.5	67.3	65.0	63.4	53.9	63.2	5.0
7/23–27/28	11	04	66	66.4	67.5	62.7	62.6	56.0	63.0	5.0
Vacation	11	05	67	—	—	—	—	—	—	—
Vacation	11	06	68	—	—	—	—	—	—	—
8/13–17/28	11	07	69	63.7	63.5	65.1	63.0	52.4	61.5	5.0
8/20–24/28	11	08	70	68.0	68.5	64.4	63.1	58.9	64.6	5.0
8/27–31/28	11	09	71	64.7	66.1	62.3	63.2	58.4	62.9	5.0
9/4–8/28	12	01	72	61.1	62.6	62.1	62.6	56.5	61.0	4.5
9/10–15/28	12	02	73	61.7	62.9	60.7	62.3	55.7	60.7	5.5
9/17–22/28	12	03	74	62.6	63.9	60.1	61.9	55.2	60.7	5.5
9/24–29/28	12	04	75	61.9	63.4	59.4	60.5	55.7	60.2	5.5
10/1–6/28	12	05	76	63.0	64.4	61.8	61.5	57.3	61.6	5.5
10/8–13/28	12	06	77	62.9	64.0	60.4	62.4	56.2	61.2	5.5
10/15–20/28	12	07	78	63.3	64.5	57.7	61.2	57.9	60.9	5.5
10/22–27/28	12	08	79	61.8	63.3	58.0	61.3	55.2	59.9	5.5
10/29/28–11/3/28	12	09	80	61.0	62.8	59.6	60.4	57.3	60.2	5.5
11/5–10/28	12	10	81	62.9	63.8	60.1	60.6	55.2	60.5	5.5
11/12–17/28	12	11	82	63.9	65.2	58.3	60.6	56.4	60.9	5.5
11/19–24/28	12	12	83	63.2	65.1	57.7	59.9	54.6	60.1	5.5
11/26–28,29/28–12/1/28	13	01	84	65.6	67.0	61.7	61.3	60.1	63.2	4.5
12/3–8/28	13	02	85	67.6	69.0	64.7	64.4	59.4	65.0	5.5
12/10–15/28	13	03	86	61.6	67.3	61.5	63.8	60.8	63.0	5.5
12/17–22/28	13	04	87	67.1	68.7	60.5	65.5	61.5	64.7	5.5
12/24,26–28/28	13	05	88	65.9	67.9	58.8	62.2	61.6	63.3	4.0
12/31/28,1/2–5/29	13	06	89	67.0	69.8	63.3	66.4	58.4	65.0	4.5
1/7–12/29	13	07	90	66.5	68.8	65.1	67.3	57.1	65.0	5.5
1/14–19/29	13	08	91	65.0	67.7	64.2	67.7	58.6	64.6	5.5
1/21–26/29	13	09	92	66.6	70.3	65.6	67.9	57.6	65.6	5.5
1/28/29–2/2/29	13	10	93	66.9	68.8	65.5	68.8	58.2	65.6	5.5
2/4–9/29	13	11	94	69.6	73.3	65.2	68.4	59.1	67.1	5.5
2/11–16/29	13	12	95	66.3	71.1	61.8	67.5	55.2	64.4	5.5
2/18–23/29	13	13	96	67.4	74.1	65.8	70.5	59.6	67.5	5.5
2/25/29–3/2/29	13	14	97	65.2	69.5	65.1	69.9	56.8	65.3	5.5
3/4–9/29	13	15	98	65.2	69.8	64.4	69.5	59.0	65.6	5.5
3/11–16/29	13	16	99	65.6	71.8	62.6	68.4	58.4	65.4	5.5
3/18–23/29	13	17	100	70.1	74.1	64.6	69.7	60.2	67.7	5.5
3/25–30/29	13	18	101	70.3	73.9	64.1	69.8	61.1	67.8	5.5
4/1–6/29	13	19	102	65.5	70.9	62.7	69.5	57.1	65.1	5.5
4/8–13/29	13	20	103	70.0	73.5	68.5	72.9	60.1	69.0	5.5
4/15–20/29	13	21	104	67.2	73.1	66.6	72.2	57.3	67.3	5.5
4/22–27/29	13	22	105	67.1	73.5	65.3	71.7	59.3	67.4	5.5
4/29/29–5/4/29	13	23	106	69.1	75.6	69.1	74.1	61.8	69.9	5.5
5/6–11/29	13	24	107	69.5	75.0	69.1	73.4	65.5	70.5	5.5
5/13–18/29	13	25	108	70.3	74.8	67.6	73.4	63.4	70.0	5.5
5/20–25/29	13	26	109	69.5	74.2	66.5	72.0	62.6	68.9	5.5
5/27/29–6/1/29	13	27	110	64.4	71.0	62.1	67.3	60.6	65.1	5.5
6/3–8/29	13	28	111	70.9	78.6	61.8	71.2	62.0	68.9	5.5
6/10–15/29	13	29	112	68.9	75.8	63.6	71.6	59.3	68.4	5.5
6/17–21/29	13	30	113	68.8	76.8	63.3	71.4	60.3	68.1	5.0
6/24–29/29	13	31	114	71.2	78.4	66.2	73.8	60.9	70.1	5.5
7/1–3,5/29	14	01	115	66.4	71.9	61.2	69.7	61.2	67.5	4.0
7/8–12/29	14	02	116	66.7	71.3	59.5	68.1	56.5	64.4	5.0
Vacation	14	03	117	—	—	—	—	—	—	—
Vacation	14	04	118	—	—	—	—	—	—	—
7/29/29–8/2/29	14	05	119	63.5	69.5	61.3	70.2	55.0	63.4	5.0
8/5–9/29	14	06	120	68.5	74.3	63.5	71.9	43.5	64.5	5.0
8/12–16/29	14	07	121	65.6	71.9	66.7	70.5	47.1	64.5	5.0
8/19–23/29	14	08	122	66.9	72.4	64.4	71.4	46.7	64.5	5.0

Appendix 1. (Continued)

				Productivity, Relays per Hour per Oper. for Operators:						
Work Dates	Period	Week in Period	Passage of Time (Weeks)	1A+1	2A+2	3	4	5+5A	Group	Days per Week
8/26–30/29	14	09	123	65.5	71.0	64.3	69.8	49.1	64.0	5.0
9/3–7/29	15	01	124	68.3	72.9	65.6	70.7	46.5	64.9	4.5
9/9–14/29	15	02	125	66.7	73.8	66.9	72.3	50.1	66.0	5.5
9/16–21/29	15	03	126	68.1	75.6	64.6	71.1	51.5	66.2	5.5
9/23–28/29	15	04	127	69.8	75.1	66.3	74.4	43.4	66.3	5.5
9/30/29–10/5/29	15	05	128	67.9	75.4	66.2	75.0	49.2	66.8	5.5
10/7–12/29	15	06	129	66.5	73.8	65.5	71.5	48.9	65.3	5.5
10/14–19/29	15	07	130	69.3	75.0	63.8	74.2	54.7	67.4	5.5
10/21–26/29	15	08	131	66.8	73.5	64.2	71.4	51.6	65.5	5.5
10/28/29–11/2/29	15	09	132	68.8	74.6	68.8	73.5	53.0	67.7	5.5
11/4–9/29	15	10	133	68.3	74.2	65.0	73.2	53.1	66.8	5.5
11/11–16/29	15	11	134	69.8	76.9	67.4	75.6	54.1	68.8	5.5
11/18–23/29	15	12	135	73.0	77.2	67.0	76.1	52.3	69.1	5.5
11/25–27,29,30/29	15	13	136	72.0	74.6	62.5	73.5	53.4	67.4	4.5
12/2–7/29	15	14	137	70.9	75.5	66.6	74.8	54.9	68.5	5.5
12/9–14/29	15	15	138	69.0	74.1	63.9	72.2	51.2	66.1	5.5
12/16–21/29	15	16	139	70.6	75.1	60.0	71.6	51.0	66.1	5.5
12/23,24,26,27/29	15	17	140	64.9	69.4	59.0	68.6	53.0	63.0	4.0
12/30,31/29,1/2–4/30	15	18	141	73.4	76.5	64.4	71.1	57.8	68.6	4.5
1/6–11/30	15	19	142	73.1	77.2	67.3	74.7	57.3	69.9	5.5
1/13–18/30	15	20	143	69.9	73.0	63.4	68.9	55.1	66.1	5.5
1/20–25/30	15	21	144	69.5	72.1	64.0	68.8	54.3	65.7	5.5
1/27/30–2/1/30	15	22	145	68.1	72.7	63.6	68.0	53.9	65.3	5.5
2/3–8/30	15	23	146	72.1	78.0	65.4	74.7	56.6	69.4	5.5
2/10–15/30	15	24	147	70.7	76.7	65.1	70.4	56.9	67.9	5.5
2/17–22/30	15	25	148	74.6	78.8	65.9	71.9	56.6	69.9	5.5
2/24/30–3/1/30	15	26	149	70.2	76.3	64.4	66.9	57.6	66.9	5.5
3/3–8/30	15	27	150	70.4	73.3	64.1	69.9	56.7	66.9	5.5
3/10–15/30	15	28	151	70.5	75.3	65.3	71.2	58.5	68.2	5.5
3/17–22/30	15	29	152	71.9	76.5	65.5	72.3	59.7	69.2	5.5
3/24–29/30	15	30	153	70.7	73.0	64.6	70.1	58.1	67.8	5.5
3/31/30–4/5/30	15	31	154	78.4	80.7	69.2	70.1	62.0	72.3	5.5
4/7–12/30	16	01	155	79.6	82.4	69.6	74.7	62.1	73.7	5.5
4/14–18/30	16	02	156	75.6	79.5	70.1	78.3	59.8	72.7	5.0
4/21–26/30	16	03	157	69.4	74.8	67.6	71.5	59.7	68.6	5.5
4/28/30–5/3/30	16	04	158	70.3	74.4	66.2	69.6	60.7	68.2	5.5
5/5–9/30	17	01	159	67.8	71.7	62.0	68.4	59.6	65.9	5.0
5/12–16/30	17	02	160	64.9	71.0	65.1	68.5	53.4	64.6	5.0
5/19–23/30	17	03	161	67.7	71.3	63.4	68.3	56.2	65.8	5.0
5/26–29/30	17	04	162	67.4	71.3	62.4	65.8	59.9	65.3	4.0
6/2–6/30	17	05	163	69.8	78.2	63.7	75.0	60.7	69.5	5.0
6/9–13/30	17	06	164	70.5	79.3	66.4	74.5	58.2	69.8	5.0
6/16–20/30	17	07	165	72.8	79.1	70.5	75.0	61.1	71.7	5.0
6/23–27/30	17	08	166	70.2	76.9	67.2	76.2	63.3	70.7	5.0
6/30/30–7/3/30	17	09	167	68.2	78.2	62.9	75.6	61.4	69.2	4.0
7/7–11/30	17	10	168	64.8	71.6	63.6	68.8	61.2	66.0	5.0
7/14–18/30	17	11	169	—	—	—	—	—	—	—
7/21–25/30	17	12	170	—	—	—	—	—	—	—
7/28/30–8/1/30	17	13	171	67.6	72.1	63.5	70.0	60.0	66.7	5.0
8/4–8/30	17	14	172	70.6	80.1	64.8	78.5	63.1	71.4	5.0
8/11–15/30	17	15	173	73.8	80.7	69.0	79.1	63.2	73.1	5.0
8/18–22/30	17	16	174	72.2	76.5	68.5	74.6	61.9	70.4	5.0
8/25–29/30	17	17	175	69.8	75.2	66.3	71.6	63.4	69.2	5.0
9/2–5/30	17	18	176	74.8	78.6	66.6	77.4	65.4	72.6	4.0
9/8–12/30	17	19	177	71.4	75.4	69.2	74.2	63.6	70.7	5.0
9/15–19/30	17	20	178	74.5	79.6	69.7	77.0	65.6	73.3	5.0
9/22–26/30	17	21	179	70.7	74.9	68.2	74.8	65.4	70.6	5.0
9/29/30–10/3/30	17	22	180	71.7	75.8	67.8	75.2	65.1	71.1	5.0
10/6–10/30	17	23	181	77.6	80.8	68.5	80.5	65.1	74.5	5.0
10/13–17/30	17	24	182	74.0	81.0	69.0	80.6	65.0	73.9	5.0
10/20–24/30	17	25	183	70.6	78.8	68.8	77.6	66.3	72.4	5.0

Appendix 1. (Continued)

Work Dates	Period	Week in Period	Passage of Time (Weeks)	Productivity, Relays per Hour per Oper. for Operators: 1A+1	2A+2	3	4	5+5A	Group	Days per Week
10/27–31/30	18	01	184	71.5	77.8	67.1	76.8	67.0	72.1	4.5
11/3–7/30	18	02	185	73.2	80.7	66.4	79.9	66.1	73.3	4.5
11/10–14/30	18	03	186	72.3	80.1	66.7	78.2	67.7	73.0	4.5
11/17–21/30	18	04	187	71.8	81.6	68.5	80.7	68.5	74.1	4.5
11/24–26/30	18	05	188	69.3	73.5	67.4	72.5	66.6	69.8	3.0
12/1–5/30	18	06	189	71.4	73.3	66.9	69.3	67.4	69.6	4.5
12/8–12/30	18	07	190	72.8	74.5	65.3	68.7	66.1	69.5	4.5
12/15–19/30	18	08	191	70.5	76.7	65.3	72.7	64.7	70.0	4.5
12/22–24/30	18	09	192	61.0	76.0	59.4	65.4	65.7	65.5	3.0
12/29–31/30	18	10	193	65.4	75.2	61.7	70.6	64.1	67.3	3.0
1/5–9/31	18	11	194	69.4	77.7	66.8	74.6	64.0	70.3	4.5
1/12–16/31	18	12	195	66.7	78.1	65.9	72.4	64.3	69.5	4.5
1/19–23/31	18	13	196	72.1	77.2	66.5	69.7	64.1	69.9	4.5
1/26–30/31	18	14	197	72.2	78.6	66.2	75.4	63.9	71.3	4.5
2/2–6/31	18	15	198	66.9	78.4	65.3	71.3	64.2	69.1	4.5
2/9–13/31	19	01	199	76.9	74.3	70.0	75.9	67.6	72.9	4.5
2/16–20/31	19	02	200	77.5	74.9	69.0	69.6	68.1	72.3	4.5
2/23–27/31	19	03	201	72.0	72.5	67.6	71.0	69.4	70.5	4.5
3/2–6/31	19	04	202	79.1	81.0	65.9	71.6	65.3	72.3	4.5
3/9–13/31	19	05	203	74.8	77.0	63.1	71.4	64.4	70.2	4.5
3/16–20/31	19	06	204	70.2	75.8	63.1	71.4	67.7	69.6	4.5
3/23–27/31	19	07	205	74.9	78.7	67.6	69.0	64.7	70.9	4.5
3/30/31–4/3/31	19	08	206	73.4	78.4	64.3	68.0	63.2	69.4	4.5
4/6–10/31	19	09	207	74.2	76.3	62.8	68.5	64.7	69.1	4.5
4/13–17/31	19	10	208	74.2	74.9	65.1	75.2	66.7	71.2	4.5
4/20–24/31	19	11	209	73.0	72.6	63.9	66.1	65.6	68.2	4.5
4/27/31–5/1/31	19	12	210	75.4	74.7	65.1	75.1	66.6	71.3	4.5
5/4–8/31	19	13	211	71.3	74.8	63.3	67.3	66.0	68.6	4.5
5/11–15/31	19	14	212	71.3	75.1	61.2	66.5	66.7	68.1	4.5
5/18–22/31	19	15	213	72.6	77.8	63.9	68.2	66.5	69.8	4.5
5/25–29/31	20	01	214	70.1	76.4	65.4	66.7	66.0	68.9	5.0
6/1–5/31	20	02	215	72.9	76.5	66.8	69.1	65.7	70.0	5.0
6/8–12/31	20	03	216	72.4	76.9	62.4	68.6	63.2	68.7	5.0
6/15–19/31	20	04	217	72.1	77.2	62.5	67.5	63.1	68.5	5.0
6/22–26/31	20	05	218	68.1	74.1	64.3	67.0	63.0	67.3	5.0
6/29/31–7/3/31	20	06	219	71.6	74.3	64.0	66.0	64.5	68.1	5.0
7/6–10/31	20	07	220	69.8	77.4	60.6	66.4	64.8	67.8	5.0
7/11–17/31	20	08	221	—	—	—	—	—	—	—
7/20–24/31	20	09	222	—	—	—	—	—	—	—
7/27–31/31	20	10	223	80.8	81.4	65.8	69.5	65.0	72.5	5.0
8/3–7/31	20	11	224	80.0	80.2	64.3	67.2	65.1	71.3	5.0
8/10–14/31	20	12	225	76.8	78.1	65.5	64.0	65.1	69.9	5.0
8/17–21/31	20	13	226	78.0	79.5	66.6	69.9	65.0	71.5	5.0
8/24–28/31	20	14	227	73.0	74.9	64.8	63.8	64.9	68.2	5.0
8/31/31–9/4/31	20	15	228	74.3	74.9	66.4	82.4	68.7	73.5	5.0
9/8–11/31	20	16	229	72.4	74.9	65.0	69.0	67.5	69.8	4.0
9/14–18/31	20	17	230	74.5	76.7	62.5	66.7	66.7	69.4	5.0
9/21–25/31	20	18	231	71.1	72.3	66.6	67.2	68.9	69.3	5.0
9/28/31–10/2/31	20	19	232	71.8	75.6	66.7	73.5	65.7	70.6	5.0
10/5–9/31	20	20	233	71.6	76.1	64.7	66.8	64.9	68.8	5.0
10/12–16/31	20	21	234	72.0	73.3	62.4	65.9	63.6	67.4	5.0
10/19–23/31	20	22	235	70.4	73.4	62.1	63.1	63.3	66.5	5.0
10/26–30/31	20	23	236	75.8	77.0	67.3	72.5	65.2	71.5	5.0
11/2–6/31	20	24	237	73.2	73.9	63.4	67.7	65.5	68.7	5.0
11/9–13/31	20	25	238	75.2	76.0	64.3	67.1	65.4	69.6	5.0
11/17–20/31	21	01	239	70.0	71.7	67.4	69.1	67.5	69.1	4.0
11/23–25,27/31	21	02	240	68.8	71.1	65.4	66.0	68.0	67.8	4.0
12/1–4/31	21	03	241	72.0	74.8	68.2	71.6	67.6	70.7	4.0
12/7–11/31	22	01	242	75.1	77.7	64.6	74.5	63.5	71.2	5.0
12/14–18/31	22	02	243	72.8	75.4	63.8	65.4	64.2	68.2	5.0
12/21–24/31	22	03	244	71.1	74.3	64.9	65.5	63.0	67.8	4.0

Appendix 1. (Continued)

Work Dates	Period	Week in Period	Passage of Time (Weeks)	Productivity, Relays per Hour per Oper. for Operators:						Days per Week
				1A+1	2A+2	3	4	5+5A	Group	
12/28–31/31	22	04	245	73.0	76.0	68.8	67.9	67.4	70.6	4.0
1/4–8/32	22	05	246	74.4	74.6	71.6	74.3	65.5	72.1	5.0
1/11–15/32	22	06	247	76.8	78.9	72.1	77.8	67.3	74.6	5.0
1/18–22/32	22	07	248	78.7	79.9	72.9	79.5	68.2	75.8	5.0
1/26–29/32	22	08	249	73.0	75.9	71.4	76.2	67.9	72.8	4.0
2/1–5/32	22	09	250	78.1	70.7	67.0	74.7	65.8	71.3	5.0
2/9–12/32	23	01	251	79.3	—	65.6	74.4	69.8	72.2	4.0
2/16–19/32	23	02	252	76.4	—	66.4	72.0	65.4	70.0	4.0
2/23–26/32	23	03	253	75.8	78.0	68.8	73.0	66.2	72.3	4.0
3/1–4/32	24	01	254	73.6	74.2	65.2	71.4	63.0	69.5	4.0
3/8–11/32	24	02	255	68.4	74.7	65.9	76.4	63.9	69.9	4.0
3/15–18/32	24	03	256	69.9	74.6	65.6	66.7	64.2	68.2	4.0
3/21–24/32	24	04	257	69.8	69.3	64.2	69.0	63.2	67.1	4.0
3/29–31/32,4/1/32	24	05	258	76.2	77.5	58.5	67.5	63.9	68.7	4.0
4/5–8/32	24	06	259	78.4	79.0	62.8	68.8	59.2	69.5	4.0
4/13–15/32	24	07	260	71.5	73.7	62.7	68.2	61.6	67.6	3.0
4/19–22/32	24	08	261	74.6	74.5	65.4	65.8	62.5	68.6	4.0
4/27–29/32	24	09	262	71.4	71.9	67.6	67.8	63.1	68.4	3.0
Days off	24	10	263	—	—	—	—	—	—	—
Days off	24	11	264	—	—	—	—	—	—	—
5/17–20/32	24	12	265	67.4	68.8	65.1	68.3	59.7	65.8	4.0
5/24–27/32	24	13	266	69.2	69.1	68.7	68.5	61.9	67.5	4.0
Days off	24	14	267	—	—	—	—	—	—	—
Days off	24	15	268	—	—	—	—	—	—	—
Days off	24	16	269	—	—	—	—	—	—	—
6/14–17/32	24	17	270	66.9	68.5	67.4	70.8	58.2	66.4	4.0

Appendix 2. Guide to Hawthorne Records

This is a brief guide to the documentation of the Hawthorne studies. Complete microfilm records of the studies are preserved on microfilm at the Library of the University of Wisconsin–Milwaukee. The original Hawthorne documents were obtained from the Hawthorne plant of the Western Electric Corporation during the spring of 1977 by Richard H. Franke, James D. Kaul, Ruth Zubrensky, and George Carian. The documents were microfilmed as historical records by Stanley I. Mallach and Steven Smith of the Library, with particular attention to recording all penciled markings, paper-clipped notes, and the like, preserving in many instances evidence which seems never before to have been noted. Particularly important were the good offices of William C. Roselle, Director of the UWM Library, and of E. L. Byrom and Florence Hybl in the Western Electric Public Relations Department at Hawthorne. Copies of the microfilm are in the Gordon Library at Worcester Polytechnic Institute, the Baker Library at Harvard, and in the Hawthorne Plant.

The records are presented on seven reels of microfilm representing the seventeen original boxes of experimental data, reports, correspondence, meeting minutes, articles, interviews, and conversations. This appendix orders the records by topic, showing to the left the microfilm reel number and the original box number and folder number of each document, as specified on the microfilm. For a more detailed guide, see Mallach and Smith (1977).

LOCATION			TOPIC
Reel	Box	Folder	*Records*
			(1) ILLUMINATION
1	1	9	Effects of illumination on industrial efficiency—1920s.
			(2) FIRST RELAY
1	1	3	Progress Report 1, 12/3/27: rest and hours.
1	1	4	Progress Report 2, 2/25/28: rest and hours.
1	1	5	Progress Report 3, 8/15/28: rest and working conditions.
1	1	6	Progress Report 4, 5/11/29: rest and working conditions.
1	1	7	Progress Report 5, 1931, never completed: miscellaneous information.
1	1	8	Pennock's notes on the test group conference in 1927—goals outline.
1	2	1	Test-period schedule; piece rates; conversion factors—1927 to 1932.
1	2	2	Daily temperature and humidity record—1927 to 1930.
1	2	3	Vascular skin reaction readings: fatigue measurement.
1	2	4	Mayo report on blood pressure readings for operators.
1	2	5	Menus and meal records for operators—1927 to 1928

Appendix 2. (Continued)

LOCATION			TOPIC
Reel	Box	Folder	*Records*
1	2	6	Daily record of operators' living habits—1927
1	2	7	Daily history record of test room, vol. 1—1927 to 1933.
2	3	1	Stock list and bench layout—1931.
2	3	2	Operator comments—1927 to 1930.
2	3	3	Operator comments continued—1928 to 1930.
2	3	4	Summary of weekly activity in test room—8/8/31 to 12/6/31.
2	3	5	Personal histories of operators—1927 to 1930.
2	3	6	Relationship between hours of sleep and speed tests—1930.
2	3	7	Relationship between voluntary rest and output quality—1930.
2	3	8	Relationship between quantity and quality of output—1931.
2	3	9	Study of output quality—1930 to 1931.
2	4	1	Study of operator reactions to shorter work week—1930.
2	4	3	Temperature and humidity studies—1927 to 1931.
2	4	4	Attitude questionnaires from operators—1929 to 1931.
2	4	5	Operator 1 interviews—5/31 to 5/32.
2	4	6	Operator 2 interviews—5/31 to 6/32.
2	4	7	Operator 3 interviews—6/31 to 6/32.
3	4	8	Operator 4 interviews—6/31 to 5/32.
3	4	9	Operator 5 interviews—6/31 to 2/32.
3	5	1	Interviews with operators—1933.
3	5	2	Study of operator removed for health and attitude reasons—1928.
3	5	3	Test-room observations, vol. I–1/32 to 3/32.
3	5	4	Test-room observations, vol. II–4/32 to 6/32.
3	5	5	Comparison of 5 day vs. 4½ day week, study of rest periods—1930.
			(3) SECOND RELAY (also see FIRST RELAY)
2	3	11	Output data—1928 to 1929.
2	3	12	Reasons why workers restrict output—1931.
			(4) MICA SPLITTING
3	6	1	Daily output variations—1928.
3	6	2	Attendance irregularities comparison between regular department and test room—1927 to 1930.
3	6	3	Production comparison between test-room and nontest-room workers—1928 to 1930.
3	6	4	Operators' comments—10/28 to 11/30.
3	6	5	Report on time required for inexperienced operators to become efficient—1928.
3	6	6	Comparison between a.m. and p.m. hourly output—1928.
3	6	7	Daily routine of mica splitting and armature straightening operators—1930 to 1931.
3	6	8	Case study of an operator's removal from experiment—1929 to 1930.
3	6	9	Study of factors influencing an operator's performance—1929 to 1930.
3	6	10	Operators' case histories—3/31.
3	6	11	Operator interviews—1930.
			(5) INTERVIEWING
4	8	4	Report on development and objectives of program for year ending 12/31/30.
4	9	4	Reactions of supervisors and employees to program—1929.
4	9	5	Methods and practices used in interviews program—1929.
4	9	6	Minutes of special training courses for interviews—1929.
4	9	7	Mathematical index for interviews—1931.
4	11	1	Projects of Industrial Research Division dealing with interviewing program—1929.
4	11	4	7 papers on aspects of Hawthorne studies, interviews included—1936.
5	12	1	Study of employee attitudes toward the bogey system—1931.
5	13	1,5	Report on interviews of supervisors in operating branch—1931.
6	14	1	Operating branch employee interviews—1929.
6	14	2,3	Analysis of operating branch employees comments.
6	14	4	Numerical analyses of 10,300 employee interviews—1929.
6	14	5	Analysis of comments on supervision of work—1929.
6	14	6,7	Analysis of 1929 interviews—1930.
6	14	8	Summary of comments and analysis of 1930 interviews—1930.
6	14	9	Minutes of supervisors meeting on complaints found—1930 to 1931.
			(6) BANK WIRING
3	7	1,2	Procedures for establishing bank wiring study using male workers—1931.
3	7	3	Report on bank wiring room—11/9/31 to 3/18/32.
3	7	4	Record of observations, vol. I—11/31 to 1/32.
3	7	5	Record of observations, vol. II—1/32 to 2/32.

Appendix 2. (Continued)

LOCATION			TOPIC
Reel	Box	Folder	*Records*
3	7	6	Record of observations, vol. III—3/32 to 5/32.
3	7	7	Interviews with bank wiring operators.
			(7) COUNSELING
5	11	6	Impact of Hawthorne studies and personnel counseling—1955.
7	15	2	"Counseling in a group situation," by Phyllis Dodds—1940.
7	15	3	"The experience in personnel counseling," by W. J. Dickson—1936.
7	15	4	"Report on counseling activity," 1/36 to 3/37.
7	15	5	"Counseling theory and practice"—1949.
7	16	1,2	Procedure outline for setting up an experiment in personnel work—1936 (with correspondence).
7	16	4	Memoranda on selection and training of counselors—1941 to 1948.
7	16	5	Analysis of a counselor's work—1/1/43 to 3/31/43.
7	16	6	Case study of counseling impact on female work group—1945.
7	16	7	Attributes for rating and selection of counselors—1948.
7	16	8	Report on Hawthorne counseling activity—1949.
7	16	9	Counseling conferences materials—1/24/61.
7	16	10	Outline and discussion of effectiveness of personnel counselors.
7	16	11	Orientation program for Hawthorne counselors.
7	16	12	Illustration of aspects of counselors' work.
7	17	1	Supervisor's observations of effects of counseling—1944.
			(8) EMPLOYEE RELATIONS
4	8	3	Studies aimed at improving employee relations—1930.
4	11	3	1940 Conference papers on employee relations from Harvard-Hawthorne group.
7	16	3	Case study, impact of impulsive supervisory work on employee relations—1939.
7	17	9	Plan for improving employee relations—1924.
7	17	10	Employee relations research study.
			(9) LAYOFFS
4	8	1	Interpretation of manufacturing employee interviews—1930.
4	8	2	Special interviews with laid off employees—4/30 to 5/30.
7	15	8	Report on employee reactions to layoffs—1938.
			(10) WAGES
7	15	6	Points of view of Hawthorne departments concerning wages—1936.
7	15	7	Report on wage incentives, time standards, and labor grades departments—1936.
7	15	11	Report on inspectors after wages were reduced—1939.
7	15	12	Correspondence and conference report on labor-management conflict on piece rates—1940.
			(11) INDUSTRIAL RESEARCH DIVISION
4	11	1	Interviewing projects of Industrial Research Division—1929.
4	11	2	Plan for Industrial Research Division, 1929 program—1928.
5	13	3	Interviewing with employees in rotational training—1930.
7	17	6	Account of Industrial Research Division work—1931.
7	17	7	Observations on Industrial Research Division by Pennock—1931.
			(12) SUPERVISORY PERSONNEL
4	9	1	Interviews with group chiefs—1929.
4	9	2	Interviews with subsection chiefs—1929.
4	9	3	Interviews with section chiefs—1929.
5	13	1	Training program for supervisors—1928.
5	13	4	Development work with supervisors—1931 to 1932.
5	13	6	Materials for research group on supervisory training methods—1931 to 1932.
6	14	9	Meetings minutes of supervisors discussing complaints—1930 to 1931.
7	15	10	Study of the mobility of supervisors—1932 to 1938.
			(13) ARTICLES, SPEECHES, AND CORRESPONDENCE
1	1	2	Reflections of Elton Mayo, interviewing report, supervisors' conference—1931.
4	8	5	Speeches and articles on aspects of industrial relations—1928 to 1930.
4	11	3	Harvard-Hawthorne research papers delivered—1940.
5	11	5	"The human factor in industry," by R. D. Gross—1931.
5	11	6	Hawthorne studies impact—1955.
5	12	3	Correspondence and materials for *Management and Workers*—1939–1950.
5	12	4	Miscellaneous papers and speeches—1928 to 1940.
7	7	2	Correspondence of Western Electric executives—3/20 to 3/32.
7	17	5	Miscellaneous correspondence and materials—10/28 to 3/31.
7	17	8	Articles and outlines for research studies—1929.

REFERENCES

Albee, G. W.
1977 "The Protestant ethic, sex, and psychotherapy." American Psychologist 32:150–61.

American Productivity Center
1980 Total Factor Productivity Index: Houston, Texas: American Productivity Center.

Baloff, Nicholas
1966 "Startups in machine-intensive production systems." Journal of Industrial Engineering 17:25–32.

Denison, Edward F.
1979 "Explanations of declining productivity growth." Survey of Current Business 59(8):1–24.

Dhrymes, Phoebus J.
1978 Introductory Econometrics. New York: Springer.

Dooley, David, and Ralph Catalano
1980 "Economic change as a cause of behavioral disorder." Psychological Bulletin 87:450–68.

Durbin, J., and G. S. Watson
1950 "Testing for serial correlation in least squares regression, I." Biometrika 37:409–28.
1951 "Testing for serial correlation in least squares regression, II." Biometrika 38:159–78.

Elliott, J. Walter
1973 Economic Analysis for Management Decisions. Homewood, Ill.: Richard D. Irwin.

Farrar, Donald E., and Robert R. Glauber
1967 "Multicollinearity in regression analysis: the problem revisited." Review of Economics and Statistics 49:92–107.

Franke, Richard Herbert
1979a "Investment for economic growth?" Paper presented, American Association for the Advancement of Science, Houston.
1979b "The Hawthorne experiments: re-view." American Sociological Review 44:861–7.

Franke, Richard Herbert, and James D. Kaul
1978 "The Hawthorne experiments: first statistical interpretation." American Sociological Review 43:623–43. Also presented as slightly more extensive report at the annual meeting of the American Association for the Advancement of Science [AAAS], Washington, D.C., February 16, 1978.

Hanushek, Eric A., and John E. Jackson
1977 Statistical Methods for Social Scientists. New York: Academic Press.

Homans, George Caspar
1950 The Human Group. New York: Harcourt Brace.

Johnston, John
1972 Econometric Methods (2nd Ed.). New York: McGraw-Hill.

Kim, Jae-On, and Frank J. Kohout
1975 "Multiple regression analysis: subprogram regression." Pp. 320–67 in Norman H. Nie, C. Hadlai Hull, Jean G. Jenkins, Karin Steinbrenner, and Dale H. Bent (eds.), Statistical Package for the Social Sciences (2nd Ed.). New York: McGraw-Hill.

Kumar, T. Krishna
1975 "Multicollinearity in regression analysis." Review of Economics and Statistics 57:365–6.

Mallach, Stanley, and Steven Smith
1977 Records of the Industrial Relations Experiment Carried Out by the Western Electric Company at the Hawthorne Works, Hawthorne, Illinois. Index, University of Wisconsin–Milwaukee Library, Milwaukee.

Nystrom, Paul C., and Richard H. Franke
1977 "The productivity of U.S. manufacturing." Transactions of the American Association of Cost Engineers 21:269–72.

O'Hagan, John, and Brendan McCabe
1975 "Tests for the severity of multicollinearity in regression analysis: a comment. Review of Economics and Statistics 57:368–70.

Pindyck, Robert S., and Daniel L. Rubinfeld
1976 Econometric Models and Economic Forecasts. New York: McGraw-Hill.

Pitcher, Brian L.
1981 "The Hawthorne experiments: statistical evidence for a learning hypothesis." Social Forces (forthcoming).

Schlaifer, Robert.
1980 "The relay assembly test room: an alternative statistical interpretation." American Sociological Review 45:995–1005.

Spaulding, Andrew G.
1980 Hawthorne: The Twenty-fourth Hour. Unpublished undergraduate dissertation. Department of Management, Worcester Polytechnic Institute.

Theil, H., and A. L. Nagar
1961 "Testing the independence of regression disturbances." American Statistical Association Journal 56:793–806.

Wallis, Kenneth F.
1972 Introductory Econometrics. Chicago: Aldine.

Wardwell, Walter I.
1979 "Critique of a recent professional put-down of the Hawthorne researches." American Sociological Review 44:858–61.

Whitehead, T. N.
1938 The Industrial Worker. 2 Vols. Cambridge: Harvard University Press.

Wiljanen, Laurel (Laurel Wiljanen Neece)
1979 Lighting the Way: First Quantitative Analysis of the Hawthorne Illumination Experiments of 1924 to 1927. Unpublished undergraduate dissertation. Department of Management, Worcester Polytechnic Institute.

WELSH POLITICAL MOBILIZATION: A COMMENT AND A NOTE*

(COMMENT ON RAGIN, *ASR*, AUGUST, 1979)

In a recent study, Ragin (1979) tested various hypotheses related to ethnic political mobilization in Wales. Using electoral results, he analyzed three different theories that could possibly explain such mobilization, with part of the analysis based on the inhabitants' identification with the traditional Welsh culture of the area as opposed to the English cultural alternative. The basis of his analysis rested on using voter support for two parties that have been active in Wales in various elections and a number of socioeconomic characteristics of the shires (counties) in the area. The key dependent variables used were support for the Liberal Party for the general elections from 1935 to February, 1974 (ten elections), and for the Plaid Cymru, the Welsh nationalist party, for general elections from 1950 to 1974 (eight elections). The nationalist party has had an obvious ethnic appeal for at least some of its supporters, and Ragin argued that the Liberals made efforts to present themselves as a party that favored Welsh interests. The Liberal Party's longstanding commitment to decentralization and even to federalism are factors that justify this view to at least some extent. Ragin's effort to test the relevance of the three (at times) competing theories for underlying ethnic mobilization has a distinct value, and there is no reason to disagree with the theoretical worth of his study. There are also no compelling reasons to question the statistical methods that he chose to use since they appear to be appropriate ones. There are, however, some serious questions in regard to the areal basis of his data and the resulting aggregation of information, and some questions related to missing observations and how they were handled. His data base is sufficiently weak in some ways so as to preclude uncritical acceptance of his results. The goal of this comment is twofold. First, the problems of data management in Ragin's study will be discussed, and the comments made may provide an additional perspective from which to view his results. Second, some further information in regard to the impact of Welsh culture on voting will be presented and, hopefully, it will prove useful for future studies of ethnic mobilization in Wales. Finally, a few observations will be offered on the new data that are presented.

In order to test the various hypotheses in question, Ragin (1979) used the 13 Welsh shires as his areal base. These proved to be the smallest areal units for which both voting results and socioeconomic statistics could be gathered. Voting results are available at the constituency level, but the socioeconomic data in the United Kingdom are reported for only shires and boroughs (cities or component parts of larger urban areas). Unfortunately, the 36 Welsh election constituencies do not adapt well to this type of format. While no constituency includes parts of 2 or more shires, the shires contain a highly variable number of electoral districts. One constituency consists of 2 relatively sparsely populated shires, while 5 other shires each constitute an electoral district. Four shires have 2 constituencies, 1 basically rural and the other primarily urban. The last 2 shires contain the bulk of both the population and of the constituencies. Monmouth has 6 while Glamorgan[1] alone has 16 of the total of 36. Thus, 1 shire accounts for almost half of the variation among the electoral districts but serves as only one observation. In regard to any analysis of ethnic mobilization, there may be as many phenomena occurring within Glamorgan as there are within Wales as a whole. While weighting measures in the regression analyses alleviates some of the difficulty in this regard, these multiconstituency shires present another problem for analysis in terms of the socioeconomic variables that Ragin used. Aggregating at the shire level for Glamorgan combines large urban areas such as Cardiff, smaller urban areas, the mining valleys of the south, and rural areas into one data observation. It is more than reasonable to expect differing characteristics for the different types of constituencies. Furthermore, the urban nature of some constituencies was enhanced by the practice, prevalent in Wales before 1950, of forming electoral constituencies out of nonadjacent urban communities in a given shire and leaving the remainder of the shire, basically rural, to form another electoral district. In sum, the use of the shires as the basic areal unit, however necessary from a data-base point of view, obscures a great deal of important variation, particularly given the hypotheses that were compared and tested.

There is another basic problem that arises in regard to the data (Ragin, 1979) used. As far as

---

* Direct all correspondence to: James M. Lutz; Department of Political Science; Iowa State University; Ames, IA 50011.

I would like to acknowledge the help of members of the U.S. Bureau of the Census, especially Dr. Diana DeAre, in the collection of the data on Welsh speakers through the use of the library holdings in Washington, D.C. I also appreciate the extremely helpful comments of an anonymous referee.

[1] Shire and constituency names used are the old designations, not the new Welsh (Celtic) equivalents.

can be discerned, no account was taken of the fact that it has been many years since the Liberal Party contested all the constituencies in a general election. While a party's decision *not* to field a candidate might reflect a perceived lack of support in a given area, results for the Plaid since 1970 would indicate that there is at least a minimum level of support for the two parties in all constituencies as the party has always gotten some votes. Thus, if the lack of a candidate from one or both of these parties in some constituencies was deemed to indicate zero support, ethnic mobilization was understated, perhaps seriously in some cases. Using a zero value in the various statistical analyses, or having some values reflecting the inclusion of zeros for the aggregated shire totals, undoubtedly induced a margin of error into the statistical results. At the very least, it would be useful to know if this situation were considered and what steps, if any, were made to correct for it. The importance of dealing with this factor can be demonstrated by the 6 general elections between 1950 and 1966. In these elections the Liberals contested only 71 seats out of a possible 216 contests, only 32.9%. Thus, for over two-thirds of the seats in question, a Liberal candidate was not present. For these 6 elections the Plaid contested only 79 seats—36.6% of those possible—although the nationalists did contest all the seats from the 1970 general election, on.

In addition to the above consideration, tactical voting decisions on the part of Liberal supporters, for example, to vote for a nationalist candidate in the absence of their own candidate would clearly affect the level of support for the Plaid. The reverse of this situation could also have occurred prior to 1970 with nationalist sympathizers or potential supporters voting for a Liberal candidate if one were present and a member of their preferred party were not running. This possibility is important because the analyses (Ragin, 1979) using Liberal vote percentages and Plaid percentages were run separately, and the results of the regression analyses tended to support different hypotheses. Indeed, it might have proven enlightening to have joined the vote levels of the two parties, with corrections for the missing observations, for a third regression analysis—to see what relationships then existed. Again, using the 6 elections from 1950 to 1960 as an example, Liberal candidates and nationalist candidates overlapped in 32 cases, slightly higher than expected given the number of candidates of the two parties, but, in 86 cases, there was a candidate of only one of the parties (and, in 91 cases, a candidate of neither). Thus, there was ample opportunity for tactical voting to occur. It does remain likely, however, that such a third regression analysis might be ambiguous, by itself, as the Liberals and the nationalists might have been tapping different areas of support—particularly considering that the Liberal party has other bases of support, given its position as a party competing throughout Great Britain.

A few comments as to how the aggregation problem and the lack of candidates may have affected Ragin's (1979) findings can be ventured, although the lack of socioeconomic data at the constituency level will make them somewhat speculative. Ragin found agricultural employment associated with support for the Liberals. The 6 general elections used before can demonstrate at least one reason for this finding. Of the 10 large constituencies in the north and west which have an agricultural employment sector of some magnitude, the Liberals put up candidates quite frequently. They contested all 6 elections in 4 constituencies. One of them, Montgomery, had been a continuously held Liberal seat from 1900 until the general election of 1979. Of the 60 contests in these constituencies, Liberal candidates were present in 41 of them, 68.3% compared to their 32.7% average for Wales as a whole. By contrast, in these same elections they fielded only 11 candidates in the 96 contests in Glamorgan and only 3 in the 36 contests in Monmouth, the more urbanized and industrialized shires in south Wales. The lack of candidates in these areas of Wales might go a long way toward explaining Ragin's finding of a negative relationship between Liberal support and the peripheral industries of mining and heavy industrial manufacturing. To the extent that the party's choices as to which constituencies held the most support for them were correct ones, Ragin's findings may be in the appropriate directions, but they would undoubtedly be weaker.

The Welsh nationalists' selectivity in presenting candidates for the 1950 to 1966 period was somewhat different. Their candidates overlapped with the Liberals in the agricultural north and west, but they were more prevalent in Glamorgan, particularly in the mining areas in the northern part of the shire. The association of nationalist support with tertiary industry might reflect the aggregation of socioeconomic data at the shire level. In the 1970 and February 1974 general elections, the nationalists' greatest strength in Glamorgan was in the mining areas (Merthyr, Caerphilly, Aberdare, and Rhondda), but not in the urban areas and the semiurban areas in the south. It is thus possible that the nationalist support was more related to peripheral mining industries than tertiary industry.

In sum, the above data problems induce

Table 1

Shire and Constituency*	Speak Welsh (%)	Speak Only Welsh (%)	Write Welsh (%)
Anglesey	65.7	4.5	55.2
Brecon and Radnor[1]	18.0	0.9	11.8
Caernarvon—Caernarvon	83.9	6.5	75.7
—Conway	44.5	2.6	34.4
Cardigan	67.6	3.7	58.6
Carmarthen—Carmarthen	70.6	3.3	55.3
—Llanelli	63.6	2.6	45.0
Denbigh—Denbigh	36.7	2.4	27.9
—Wrexham	17.7	1.0	12.0
Flint—East	8.9	0.4	5.5
—West	21.3	1.3	14.4
Glamorgan—Abernarvon	11.4	0.8	6.5
—Barry[2]	5.9	0.4	4.1
—Barry[3]	5.5	0.3	3.7
—Caerphilly[2]	6.8	0.5	4.1
—Caerphilly[3]	6.8	0.5	4.2
—Gower	46.0	2.0	29.8
—Neath	18.5	1.3	10.5
—Ogmore	11.1	0.9	6.5
—Pontypridd	6.4	0.4	4.0
—Aberdare	15.4	1.2	8.4
—Cardiff North[2]	5.8	0.4	4.1
—Cardiff North[3]	6.4	0.5	4.6
—Cardiff Northwest[4]	6.6	0.4	4.8
—Cardiff Southeast[2]	4.2	0.3	2.7
—Cardiff Southeast[3]	3.2	0.3	2.0
—Cardiff West[2]	4.0	0.3	2.7
—Cardiff West[3]	4.2	0.4	2.8
—Merthyr Tydfil	11.3	0.9	5.5
—Rhondda East[5]	13.8	0.8	7.3
—Rhondda West[5]	14.2	0.8	7.5
—Rhondda[4]	14.0	0.8	7.4
—Swansea East	16.0	0.7	9.0
—Swansea West	10.1	0.5	6.0
Merioneth	73.5	7.8	65.3
Monmouth—Arbetillery	0.9	0.1	0.4
—Bedwelty	2.8	0.2	1.4
—Ebbw Vale	3.5	0.3	1.8
—Monmouth	2.4	0.2	1.4
—Newport	1.7	0.1	0.9
—Pontypool	1.5	0.1	0.8
Montgomery	28.1	1.3	22.0
Pembroke	20.7	1.0	14.8

* Shire and constituency names are old style, not new Welsh equivalents.

[1] Two shires in joint constituency.

[2] 1970 constituency boundaries.

[3] 1974 and 1979 constituency boundaries.

[4] New constituency created in 1974.

[5] 1970 constituencies.

some doubt in regard to Ragin's (1979) overall results. Perhaps these problems explain some of his relatively unexpected findings and support for different hypotheses depending upon which party's support was analyzed as their selective presentation of candidates varied so much in Glamorgan—and one shire contributing most to the regression results, given the weighting process used. It is also equally possible that the consideration of these factors might have changed some of the results. Thus, notwithstanding an excellent effort to test some important theories on ethnic political mobilization in the Welsh context, the results need to be viewed with caution.

The second object of this comment is to provide some additional information at the constituency level for one of the key indicators associated with the Welsh cultural tradition—the proportion of the population that speaks Welsh as, at least, a second language. The proportion of Welsh speakers in each constituency in the 1970s can be determined through the use of a special census publication of the government of the United Kingdom (Office of Population Censuses and Surveys, 1973) and from information on the components of the various constituencies (Craig, 1972). Similar information for the 1960s is already available in Philip (1975). The presentation of this information may prove useful for future tests or retests of the hypotheses on ethnic political mobilization. The breakdown of Welsh speakers in the constituencies for the 1970s is presented in Table 1. As is immediately obvious, there are very clear differences between some of the constituencies within the same shire. The more urban constituencies contain individuals who are less likely to know Welsh, while the nearby rural areas have a much higher proportion of Welsh speakers and writers. The wide variations among the constituencies in Glamorgan are particularly noteworthy. While none of the constituencies in Glamorgan have the highest levels in Wales, some are among the lowest.

The information presented in the table can be used, initially, to test the relationship of the identification with the Welsh cultural tradition as indicated by Welsh speaking ability and support for the Plaid. Ability to write Welsh, the inability to speak English, and the ability to speak Welsh are all highly correlated (r = .90 or better), so there is no significant difference between the three measures for a preliminary analysis. Welsh speaking proved to be highly correlated with support for the Plaid in the four most recent general elections—r^2 equals .52 for 1970; .40 for February, 1974; .55 for October, 1974; and .60 for 1979. Three constituencies (Caernarvon, Carmarthen, and Merioneth) account for a major portion of the high level of association. There are large numbers of Welsh speakers, and the Plaid has achieved its highest level of support in these three constituencies, winning two of them in February, 1974, all three in October, 1974, and two in 1979. These have been the only seats the party has won in general elections. The correlation of lower levels of Welsh speaking and support for the nationalists is weaker with these three constituencies removed. The variance explained drops to .19 for 1970; .31 for February, 1974; .43 for October, 1974; and .32 for 1979. Thus, the relationship is weaker but still present, particularly for October, 1974. In the final analysis, identification with Welsh culture (as indicated by language ability) is an important determinant of support for the Plaid, but insufficient alone. These findings would support the need for studies like Ragin's (1979) to discover other attributes associated with support for the Welsh nationalists and for ethnic mobilization, in general, and they also indicate the advisability of using better levels of spatial input when possible and avoiding the problems presented by missing observations.

REFERENCES

Craig, F. W. S. (ed. and compiler)
1972 Boundaries of Parliamentary Constituencies, 1885–1972. Chichester: Political Reference Publications.

Office of Population Censuses and Surveys
1973 Census 1971: Report on the Welsh Language in Wales. Cardiff: Her Majesty's Stationary Office.

Philip, Alan Butt
1975 The Welsh Question: Nationalism in Welsh Politics, 1945–1970. Cardiff: University of Wales.

Ragin, Charles C.
1979 "Ethnic political mobilization: the Welsh case." American Sociological Review, 44:619–35.

AGGREGATION GAIN AND LOSS IN ELECTORAL RESEARCH*

(A RESPONSE TO LUTZ'S COMMENT)

While Lutz's comment on my article is almost entirely speculative, it does raise some interesting issues. This response to Lutz's comment gives me an opportunity to demonstrate techniques that remedy the problems Lutz mentions. Before presenting these analyses, I first discuss Lutz's statements concerning my article and point out some of the problems and weaknesses in his speculations.

Lutz first argues that the observational units (counties) I use in my analysis, however necessary from a data availability standpoint, are not adequate because they are larger than the fundamental British electoral unit (the constituency) and contain vastly different numbers of constituencies (up to 16). He suggests that my aggregation of constituencies to counties may have obscured important sources of variation since the different constituencies contained within a multiconstituency county are unlikely to be homogeneous. He notes, for example, that the county of Glamorgan, with 16 constituencies, "may" contain as much variation in socioeconomic conditions relevant to ethnic mobilization as exists in Wales as a whole (Lutz, 1980:2).[1]

---

* Direct all correspondence to: Charles C. Ragin; Department of Sociology; Indiana University; Bloomington, IN 47405.

[1] Lutz comments that aggregating at the county level for Glamorgan combines urban areas, semi-urban areas, rural areas and "the mining valleys of

While it is true that aggregation generally compresses or reduces variation, this result is in no way automatic.[2] The fact of variance reduction must be established in each separate case. In Wales, for example, there are whole counties that are almost entirely rural and agricultural, and, despite Lutz's protests to the contrary, the fact remains that Glamorgan is predominantly urban and industrial. Indeed, students of Welsh politics and society typically distinguish between the rural north of Wales, comprising a majority of the counties of Wales, and the urban, industrial south, comprising the counties of Glamorgan and Mommouth and parts of other counties. In short, there is a great deal of variation among the counties of Wales in terms of socioeconomic composition, perhaps as great as the variation among the constituencies. The argument that a particular aggregation has compressed variation must be established empirically, not by ad hoc speculation.

Lutz's argument concerning my use of county data also ignores the fact that my analysis was both cross-sectional and longitudinal. Even if important sources of variation were obscured by the aggregation of constituencies to counties, the fact remains that I examined longitudinal variations within counties by pooling cross-sectional and time-series variations. In the analysis of Liberal Party support, for example, each county is observed at 10 points in time (1935 to 1974); in the analysis of Plaid Cymru support each county is observed at 8 points in time (1950 to 1974). Dramatic variations in the socioeconomic conditions relevant to ethnic political mobilization occurred in these counties over this period.

The second point raised by Lutz concerns my handling of "missing" data (Lutz, 1980:1). Because minor parties are often strapped for funds, they frequently are not able to contest every constituency election. When there is no candidate in a constituency election, the party, of course, receives no votes, and in the statistical analysis the party is coded as having received 0% of the votes cast. Lutz argues that this procedure probably underestimates the support for a party in such constituencies for, if the party had fielded a candidate, it surely would have received more than 0% of the vote. Thus, Lutz speculates that this procedure introduced a margin of error in my analysis.

There are several problems with Lutz's argument. First, it should be noted that I did not use constituency results in my analysis; I used county results. One could speculate that the aggregation of constituencies to counties resulted in aggregation *gain* (see below) and removed possible biases introduced by using the 0% coding for failure to compete. Second, I should point out that Lutz's presentation of information—on the extent to which the Liberal Party and the Plaid Cymru failed to compete in Welsh constituencies—is extremely selective. He reports data on the 1950 to 1966 period (Lutz, 1980:4). During this period, support for the Plaid Cymru was slight and scattered. It was after the election of 1966 that this party experienced a period of real growth. In fact, in the elections of 1970 and 1974 the Plaid Cymru contested every constituency election in Wales. Also during the 1950 to 1966 period, the Liberal Party was almost eliminated from British politics. The Liberal Party contested few elections because its chances of winning any of them were so slim. Before 1950, of course, the Liberal Party contested almost all constituency elections in Wales, a fact Lutz fails to mention, and after the rebirth of Welsh nationalism in the late 1960s, the Liberal Party contested many more elections in Wales than it had in the 1950 to 1966 period. In 1974, for example, the Liberals contested 31 out of 36 constituency elections in Wales. Thus, Lutz's focus on the 1950 to 1966 period seriously distorts the available data on patterns of candidature, especially considering the fact that the post-1966 (Liberal and Plaid Cymru) and the pre-1950 (Liberal) periods were responsible for the largest portion of the variation in the measures of party support I used in my analysis.

Another problem with Lutz's argument is that he ignores features of the British electoral system that favor the 0% coding. Electoral laws in Britain force minor parties to compete only in constituencies where there is likely to be at least a minimal level of support for their candidates. Every candidate is required to put up a substantial monetary deposit in order to be listed on the ballot in a constituency election. If she/he fails to win at least 12.5% of the poll, then the deposit is forfeited. In effect, the law compels minor parties unable to finance deposits for their candidates in every constituency to assess the socioeconomic and political conditions of each constituency and to determine where the party's money might best be risked. The fact that minor parties, such as the Plaid Cymru and, since 1931, the Liberal Party, most often forfeit a majority of their deposits suggests that these parties put up deposits (and candidates) in every constituency where they

---

the south" (1980:3). It should be noted, to avoid confusion, that the mining areas are located in the northern part of Glamorgan.

[2] This discussion of variance reduction sets aside, for the present, the relationship between weighting and reduction.

have even an outside chance of reaching the 12.5% minimum. In the election of 1966, for example, the Plaid Cymru contested 20 constituency elections in Wales and lost 18 of their deposits. Their lowest poll was 1.9% (in East Flintshire). These data suggest that (1) a party's decision not to field a candidate in a particular constituency must reflect a perceived lack of support in the constituency, and (2) the 0% coding that the party receives in such cases may be a rough, though certainly conservative, estimate of the poll it would have received if it had actually contested the election.[3]

The third issue raised by Lutz concerns the matter of tactical voting. In some constituency elections, only one of the two minor parties (Liberal and Plaid Cymru) put up candidates. Thus, supporters of the Liberal Party may have voted for the Plaid Cymru candidate in the absence of a Liberal Party candidate, and *vice versa*. This is an important issue, especially considered in the context of the second issue raised by Lutz. In my original article I argued that, even though both minor parties claim to represent the interests of Wales as a whole, the bases of support for the two parties differ. Lutz suggests, however, that the combined effect of tactical voting decisions and uneven availability of candidates may have been to create artificial differences between these two parties in the analysis of the various bases of their support.

While this is an interesting speculation, there are several problems with it. First, it should be noted that the Liberal Party in Wales, especially since 1964, generally has contested a subset of the seats contested by the Plaid Cymru and that in most elections there is a weak, *positive* relationship between the presence of a Liberal candidate and the Plaid Cymru percentage of the vote. Thus, the Plaid Cymru tends to be more aggressive than the Liberal Party in putting up candidates, but it has not benefited from the failure of the Liberal Party to put up candidates in some Welsh constituencies. Another problem with Lutz's argument is that his presentation of data is, again, very selective. Lutz (1980:4–6) focuses on the 1950 to 1966 period, when in fact the real resurgence of Welsh nationalism and the support for these two parties came *after* 1966. In my analysis of the support for these parties the greatest contribution to the total variation in their support came from the 1970 and 1974 elections. In these two elections overlapping candidatures were by far the rule, not the exception.

One possibility not considered by Lutz is that aggregation *gain* may have resulted from my procedures. Suppose county A is composed of two constituencies, A1 and A2, and that these two constituencies differ in socioeconomic composition. In A1 Liberals think that they have a good chance of avoiding a forfeiture of their deposit and put up a candidate, while in A2 they perceive a probable lack of support and do not put up a candidate. Suppose further that the reverse is true for the Plaid Cymru, and they put up a candidate in A2 but not in A1. If tactical voting forces some Plaid Cymru supporters in A1 to vote for the Liberal Party and some Liberal Party supporters in A2 to vote for the Plaid Cymru, then Plaid Cymru support will be underestimated in A1 (0%) and overestimated in A2, while Liberal Party support will be underestimated in A2 (0%) and overestimated in A1. To aggregate these two constituencies together and compute county-wide percentages, however, may yield accurate measures of Plaid Cymru and Liberal support (at the county level) because the aggregate measures would combine overestimates and underestimates. Uncorrected for patterns of candidature, the constituency-level measures would produce distorted results. True, this line of reasoning is completely speculative, but it is no less speculative than Lutz's comment.

Speculative discussion, however, is no substitute for empirical research. Most of the issues Lutz raises can be addressed only with appropriate constituency-level data. Lutz laments their absence. Some constituency-level data have been made available, however, by Great Britain's General Register Office. While these data are only roughly comparable to the data I used in my study, they allow a reanalysis of the social bases of the support for the Liberal Party and the Plaid Cymru at the constituency level.

Before I discuss the nature of my reanalysis, I should point out the shortcomings of these data. (1) They are recent data, and they are available for only one point in time. Thus, in my reanalysis I examine only the elections of 1964, 1966, 1970 and 1974. (2) The data are not detailed enough to allow the construction of measures comparable to those used in my article. A key variable in that article was an occupational composition measure indicating the proportion of the labor force employed in advanced sector jobs in the tertiary and in modern industries (that is, nonperiphery industries). It is impossible to duplicate this measure with the constituency data supplied by the General Register Office. As a substitute for the measure of advanced sector employment, I constructed an estimate based on the relationship between tertiary employment and manufacturing employment.

---

[3] Another way to look at this issue is to consider any measure of party support that incorporates the 0% coding (for failure to contest) a compound measure. With the 0% coding two pieces of information are combined in the measure of party support: (1) whether or not the minor party perceived any possibility of support (0 versus any other number), and (2) the actual percentage of the vote received by the party if it perceived any possibility of support. Both pieces of information are relevant to party support; the compound measure may be even superior.

The reanalysis I present below answers all of Lutz's speculations. The fact that I use constituency-level data in the reanalysis obviously answers the first speculation, that my findings may have been shaped by the necessity of using county-level data. Lutz's second speculation, that the 0% coding of failures to contest elections may have biased my findings, is answered in two different ways. In one analysis I use two dichotomously coded variables (No Liberals and No Plaid Cymru) to control statistically for the absence of candidates. When the Liberal Party failed to compete in a particular constituency election, for example, No Liberal is coded 1. Otherwise, No Liberal is coded 0. A parallel procedure is used in the coding on No Plaid Cymru. These variables are used in the analysis to control for the effects of differential patterns of candidature. In another analysis I answer Lutz's second speculation by performing a separate statistical examination of constituencies with overlapping candidatures. In this analysis the number of observations is reduced from 144 (36 constituencies × four elections) to 69. Lutz's third speculation concerns the problem of tactical voting. The two solutions to the problem of the 0% coding of failures to contest elections answer this speculation. The question of tactical voting, however, also touches a larger issue only briefly discussed by Lutz. In his discussion of the tactical voting problem, Lutz suggests that the social bases of the support for the Liberal Party and the Plaid Cymru may be identical, or at least very similar. Given the fact that both parties claim to represent the interests of Wales as a whole, this fourth, implicit speculation warrants close examination.

Unfortunately, it is not possible to address this fourth speculation convincingly simply by adding the support for the Plaid Cymru to the support for the Liberal Party and running a third statistical analysis. This method does not really *test* the degree to which the social bases of the support for the two parties differ. As an alternative I propose using canonical correlation analysis. With canonical correlation analysis it is possible to examine the dimensionality of a set of interval scale dependent variables as they relate to a set of independent variables (see Cooley and Lohnes, 1962). Canonical correlation analysis proceeds in a stepwise fashion, extracting orthogonal canonical variates one at a time. An important feature of canonical correlation analysis is that it derives a linear combination from each of two sets of variables in such a way that the correlation between the two linear combinations is maximized, and canonical weights for independent and dependent variables can be computed. This feature of canonical correlation analysis makes it possible for the researcher to assess the interrelationships among a set of dependent variables *relative to* a given set of predictor variables and their interrelationships. Another important feature of canonical correlation analysis is that it allows the researcher to perform tests of statistical significance on the orthogonally extracted canonical variates. In an analysis of two dependent variables, for example, two canonical variates are extracted. If the second variate does not account for a statistically significant portion of the variance in the dependent variables, it is rejected.

In order to examine the hypothesis that the social bases of the support for the Plaid Cymru and the Liberal Party are identical, or at least very similar, I use measures of the support for these two parties as multiple dependent variables in canonical correlation analyses. With two dependent variables several patterns of results are possible. Only one pattern supports the argument that the social bases of the support for these two parties are similar. This argument is supported when just the first canonical variate is statistically significant *and* both measures of party support correlate strongly (and in the same direction) with the first variate. This is not the pattern I would predict on the basis of my earlier work, however. In my article I show that even though the parties compete for the support of similar sets of voters (see, e.g., Ragin, 1979:626, Table 3), there are several differences in the bases of the support for these two parties. The Liberal Party tends to do better in agricultural areas, while the Plaid Cymru tends to do better in areas where there is a greater persistence of Welsh traditionalism—for example, noncomformity or Welsh-speaking, in areas where there is higher percentage of the labor force employed in advanced sector occupations, and in areas that combine these two features. On the basis of the similarities and differences discussed in my article, I predict two significant canonical variates, one showing similarities in the support for these two parties and one showing differences.

I report the results of my test of these competing speculations in Table 1. The dependent variables in the canonical analyses are the percentage of voters in each constituency supporting the Plaid Cymru and the percentage supporting the Liberal Party. The independent variables are the percentage of the labor force employed in agriculture, the estimate of the percentage of the labor force employed in the advanced sector (mentioned earlier), the percentage of the population of each constituency able to speak Welsh (from Lutz, 1980), the multiplicative interaction of the advanced sector employment measure and the measure of Welsh-speaking (following the results reported in Ragin, 1979:631, Table 5), the dichotomous variables (described above) that act as controls for patterns of candidature, and dichotomous variables that control for the differences in the levels of support for these two parties across elections. (Election 66 is coded 1 if the party support measures are from the 1966 election and 0 otherwise, etc. The 1964 election is the omitted category.) The results of three separate canonical correlation analyses are reported. The first reports the results that are obtained when no controls for patterns of candidature are included. This analysis shows the consequences of accepting the 0% coding as a valid estimate of a party's support in constituencies where it fails to put up candidates. The second analysis includes the controls for patterns of candidature. The third reports the results of an analysis of constituencies with overlapping candidatures (see above).

In general, the results reported in Table 1 support those I presented in my original article (Ragin, 1979) and they provide additional support for my contention that there are important differences in the social bases of support for the Plaid Cymru and for the Liberal Party. The coefficients for the dependent variables in each analysis show that there are both similarities and differences in the support for these two parties. In each analysis the first canonical variate shows the similarities in their support while the second variate shows the differences. Also, in each analysis, both the first and the second canonical correlations are very strong and highly significant. The analysis that gives the least support to the argument that there are strong similarities in the support for these two parties is the second analysis. This analysis uses information on all 144 constituency elections and includes controls for patterns of candidature. The results of this analysis suggest that controlling for patterns of candidature may increase the statistically discernible differences between the two parties. This finding directly contradicts Lutz's speculation. The results of the third analysis, which includes only constituencies with overlapping candidatures, however, shows that the alternative solution to the problem of differential patterns of candidature presents somewhat different results. The similarities between the parties reported in the third analysis, however, may be the product of the selection mechanism that was used

Table 1. Canonical Correlation Analysis of the Effects of Welsh-Speaking, Agricultural Employment and Advanced Sector Employment on Liberal Party and Plaid Cymru Support, Constituency Data: 1966–1974

	CONDITIONS					
	I. No Controls for Patterns of Candidature (N = 144)		II. Controls for Patterns of Candidature (N = 144)		III. Constituencies with Overlapping Candidature only (N = 69)	
Variables	Canon. Var. 1	Canon. Var. 2	Canon. Var. 1	Canon. Var. 2	Canon. Var. 1	Canon. Var. 2
%Agricultural	.490	−.964	.420	−.380	.542	−.702
%Advanced Sector*	.003	−.373	−.061	−.147	−.102	−.148
%Welsh-Speakers	−.001	.436	−.307	.429	−.083	.839
Interaction AS×W	.460	.770	.560	.553	.753	.138
Election 66	−.052	.021	−.028	.006	−.077	.130
Election 70	.206	.481	−.011	.462	−.039	.761
Election 74	.459	.166	.154	.489	.214	.565
No Liberal			−.412	.703		
No Plaid Cymru			−.512	−.239		
%Liberal	.750	−.670	.888	−.473	.745	−.671
%Plaid Cymru	.586	.818	.375	.933	.727	.691
Canonical Corr.	.889	.602	.931	.745	.905	.724
Significance	.000	.000	.000	.000	.000	.000

* %Advanced Sector employment has been estimated; see text.

to define the subset of constituencies on which the analysis is based.

The size and direction of the canonical coefficients attached to the independent variables are fairly consistent across the three analyses and with my (Ragin, 1979) article. In each analysis: (1) %Agricultural has a strong positive effect on the similarities and a strong negative effect on the differences between Plaid Cymru support and Liberal support (%Plaid Cymru—Liberal), (2) %Advanced Sector has a weak, inconsistent effect on both similarities and differences, (3) %Welsh-Speakers has a strong positive effect on differences, and (4) the multiplicative interaction between Welsh-speaking and advanced sector employment (Interaction AS×W) has a strong positive effect on both similarities and differences. This last finding suggests that the joint effect of advanced sector employment and Welsh-speaking is to increase the support for both parties and to increase the lead of the Plaid Cymru over the Liberal Party. This seemingly contradictory pattern must be understood in the context of the fact that both of these parties are minor parties and that in some areas of Wales neither party does very well. In general, the findings support the interpretations of Welsh ethnic political mobilization I presented in my article.

Even though my reanalysis supports my original analysis and the decisions and assumptions on which it was based, in general the superiority of constituency data must be admitted. Unfortunately, adequate constituency data are not available for *historical* research. In my (Ragin, 1979), article my objective was to characterize ethnic political mobilization in Wales over the 1935 to 1974 period. Historical *county* level data were the only data available for this task; the crude constituency data that do exist are available for only one relatively recent point in time. I hope the results of my reanalysis encourage those willing to work with available historical materials to persist in their efforts to apply quantitative techniques to historical data.

REFERENCES

Cooley, W. W. and P. R. Lohnes
1962 Multivariate Procedures for the Behavioral Sciences. New York: Wiley.

Lutz, James M.
1980 "Welsh political mobilization: a comment and note." American Sociological Review 45:1028–36.

Ragin, Charles C.
1979 "Ethnic political mobilization: the Welsh case." American Sociological Review 44:619–35.

ITEMS

■ With this issue I end my stint as editor of the *American Sociological Review*. Looking back over the three years, I realize that the objectives I had when I first began remained the same throughout my term. I wanted to publish papers that would have lasting value in many different areas of sociology. Theoretical and historical pieces were sought after as much as were papers using the most up-to-date quantitative techniques. The basic question remained: Is it important, does it matter?

I thought that we should make room for pieces that needed more than the standard thirty manuscript pages—hence, the twenty-page expansion; and I also wanted to encourage shorter, less developed or polished pieces—hence, the section on research notes.

The biggest reward I received as editor was that I learned a lot about the kinds of research sociologists are engaged in, the directions sociology seemed to be moving in, and what the major issues are. I discovered there is quite a bit of disagreement over what constitutes an important or competent piece of work and that those disagreements weren't limited to any particular subfields. I also learned that a great many busy people were willing to take the time to write detailed, thoughtful reviews from which authors were able to benefit even if their papers were not published in the *American Sociological Review*.

I want to acknowledge with appreciation and thanks the help that I received from several colleagues and staff members. Most of all I want to thank Linna McDade and Clark McPhail who stayed on as Managing Editor and Deputy Editor for the full three years. Linna did a superb job of managing the office, keeping the manuscripts moving, and responding to irate and upset authors with sympathy and understanding. Clark McPhail played an important role in assigning manuscripts to readers and in advising me on their final disposition. Joe Spaeth shared these tasks for the past year and I am very grateful for his help and sound advice. Rafe Stolzenberg served as a deputy editor during my first two years, and his time and efforts are appreciated.

We were fortunate in having the services of two first-rate copy editors—Mary Mander, for the first two years, and Elizabeth Neumann, in the third year, performed wonderfully under all kinds of time pressures. They managed to satisfy authors, editors, and printers.

Finally, I want to express my appreciation to my advisory editors, to all of you who reviewed manuscripts, and to Henry Quellmalz who somehow always managed to get an issue out faster than we thought possible.

■ PETER H. ROSSI (The Presidential Address) is Professor, Department of Sociology, and Director, Social and Demographic Research Institute, University of Massachussetts at Amherst. Among his fields of research are state and local political elites and the politics of land-use regulation: His *Handbook of Survey Research* (with Wright and Anderson) will be published in 1981 by Academic Press.

■ IVAN D. CHASE (Social Process and Hierarchy Formation in Small Groups) is Assistant Professor, Department of Sociology, State University of New York at Stony Brook. His current research combines empirical techniques and theoretical approaches from the social sciences and animal behavior to increase understanding of social inequality in both humans and animals; studies hierarchy formation further, using larger groups; and (using microeconomic methods) examines biological theories of "cooperative" behavior in animals.

■ RANDALL COLLINS (Weber's Last Theory of Capitalism: A Systematization) is Professor, Department of Sociology and Center for Advanced Studies, University of Virginia. He is comparatively analyzing social determinants of philosophy and mathematics in various historical societies. His books, *Sociology Since Mid-Century*, with Norbert Wiley (Academic Press, and *Non-Obvious Sociology: An Introduction to Sociological Thinking* (Oxford University Press) are forthcoming.

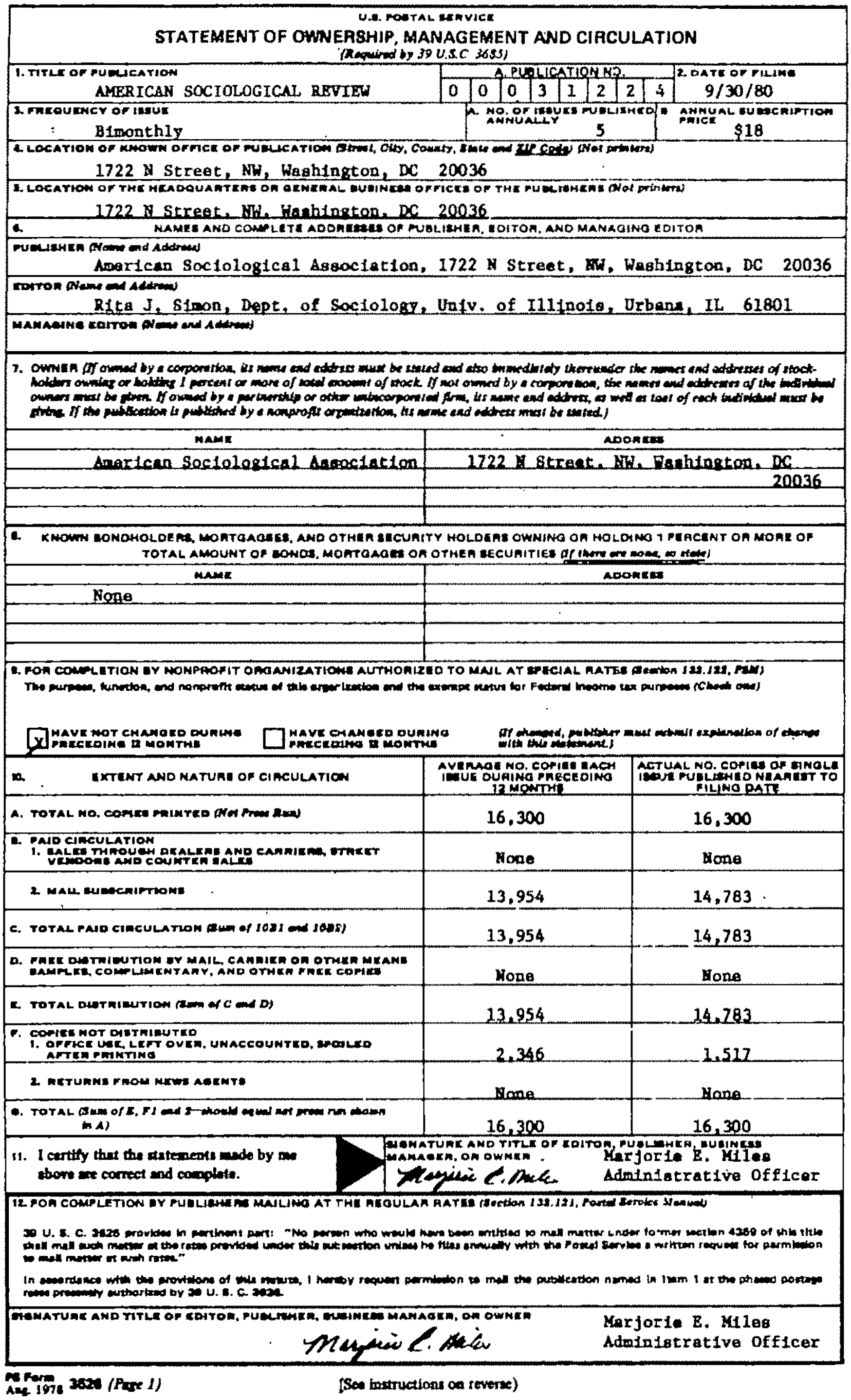
■ JOHN H. RALPH (Immigration and the Expansion of Schooling 1890–1970) is Assistant Professor, Department of Educational Studies, University of Delaware, where he is conducting a longitudinal study of school determinants in the United States with special attention to interest in political developments and school change at the turn of the century. RICHARD RUBINSON is Associate Professor, Department of Social Relations, Johns Hopkins University. He is studying processes of political transformation in the capitalist world economy in the nineteenth century, and is also conducting research in historical studies of educational expansion.

■ L. RICHARD DELLA FAVE (The Meek Shall Not Inherit the Earth) is Associate Professor, Department of Sociology and Anthropology, North Carolina State University. He is developing research to test empirically propositions set forth in the present article, as part of a larger project through which stratification is legitimated. Other research deals with the determinants of ethnic boundary maintenance. Also, he and Harold R. Kerbo have just completed a study of the concentration of stockvoting rights in major U.S. corporations.

■ HARVEY M. CHOLDIN (Suburban Status Instability) is Professor, Department of Sociology, University of Illinois at Urbana-Champaign. Current research centers on sociability and social change in suburbs and city neighborhoods. Forthcoming from Academic Press (and edited with Michael Micklin) is *Sociological Human Ecology*. CLAUDINE HANSON is a Ph.D. candidate, Department of Sociology, UIUC. She is researching determinants of urban subcommunity status as well as judgments of equity among poor, middle-class and rich Americans. ROBERT BOHRER is Professor, Department of Mathematics, University of Illinois at Urbana-Champaign. A mathematical statistician, he is working on simultaneous inference; statistical computing; and applications of time-series methods in psychophysiology.

■ MARILYN LESTER (Generating Newsworthiness) is Associate Professor, University of Nevada at Las Vegas. Other current interests include research and theoretical development in the field of properties of intimate experiences. ROBERT SCHLAIFER (Comment on Franke and Kaul) is William Ziegler Professor of Business Administration, Graduate School of Business Administration, Harvard University. He is currently researching Computerized Data Analysis.

■ JAMES M. LUTZ (Comment on Ragin) is Assistant Professor, Department of Political Science, Iowa State University. From Praeger (1978) came his book (written with Robert T. Green), *The United States and World Trade: Changing Patterns and Dimensions*. He is researching international terrorism and delving into the aspects of political/economic interactions (international level), including third-world dependency.



U.S. POSTAL SERVICE

STATEMENT OF OWNERSHIP, MANAGEMENT AND CIRCULATION

(Required by 39 U.S.C. 3685)

1. TITLE OF PUBLICATION: AMERICAN SOCIOLOGICAL REVIEW

A. PUBLICATION NO.: 0 0 0 3 1 2 2 4

2. DATE OF FILING: 9/30/80

3. FREQUENCY OF ISSUE: Bimonthly

A. NO. OF ISSUES PUBLISHED ANNUALLY: 5

B. ANNUAL SUBSCRIPTION PRICE: $18

4. LOCATION OF KNOWN OFFICE OF PUBLICATION *(Street, City, County, State and ZIP Code) (Not printers)*: 1722 N Street, NW, Washington, DC 20036

5. LOCATION OF THE HEADQUARTERS OR GENERAL BUSINESS OFFICES OF THE PUBLISHERS *(Not printers)*: 1722 N Street, NW, Washington, DC 20036

6. NAMES AND COMPLETE ADDRESSES OF PUBLISHER, EDITOR, AND MANAGING EDITOR

PUBLISHER *(Name and Address)*: American Sociological Association, 1722 N Street, NW, Washington, DC 20036

EDITOR *(Name and Address)*: Rita J. Simon, Dept. of Sociology, Univ. of Illinois, Urbana, IL 61801

MANAGING EDITOR *(Name and Address)*:

7. OWNER *(If owned by a corporation, its name and address must be stated and also immediately thereunder the names and addresses of stockholders owning or holding 1 percent or more of total amount of stock. If not owned by a corporation, the names and addresses of the individual owners must be given. If owned by a partnership or other unincorporated firm, its name and address, as well as that of each individual must be giving. If the publication is published by a nonprofit organization, its name and address must be stated.)*

NAME	ADDRESS
American Sociological Association	1722 N Street, NW, Washington, DC 20036

8. KNOWN BONDHOLDERS, MORTGAGEES, AND OTHER SECURITY HOLDERS OWNING OR HOLDING 1 PERCENT OR MORE OF TOTAL AMOUNT OF BONDS, MORTGAGES OR OTHER SECURITIES *(If there are none, so state)*

NAME	ADDRESS
None	

9. FOR COMPLETION BY NONPROFIT ORGANIZATIONS AUTHORIZED TO MAIL AT SPECIAL RATES *(Section 132.122, PSM)*
The purpose, function, and nonprofit status of this organization and the exempt status for Federal income tax purposes *(Check one)*

☒ HAVE NOT CHANGED DURING PRECEDING 12 MONTHS ☐ HAVE CHANGED DURING PRECEDING 12 MONTHS *(If changed, publisher must submit explanation of change with this statement.)*

10. EXTENT AND NATURE OF CIRCULATION	AVERAGE NO. COPIES EACH ISSUE DURING PRECEDING 12 MONTHS	ACTUAL NO. COPIES OF SINGLE ISSUE PUBLISHED NEAREST TO FILING DATE
A. TOTAL NO. COPIES PRINTED *(Net Press Run)*	16,300	16,300
B. PAID CIRCULATION 1. SALES THROUGH DEALERS AND CARRIERS, STREET VENDORS AND COUNTER SALES	None	None
2. MAIL SUBSCRIPTIONS	13,954	14,783
C. TOTAL PAID CIRCULATION *(Sum of 10B1 and 10B2)*	13,954	14,783
D. FREE DISTRIBUTION BY MAIL, CARRIER OR OTHER MEANS SAMPLES, COMPLIMENTARY, AND OTHER FREE COPIES	None	None
E. TOTAL DISTRIBUTION *(Sum of C and D)*	13,954	14,783
F. COPIES NOT DISTRIBUTED 1. OFFICE USE, LEFT OVER, UNACCOUNTED, SPOILED AFTER PRINTING	2,346	1,517
2. RETURNS FROM NEWS AGENTS	None	None
G. TOTAL *(Sum of E, F1 and 2—should equal net press run shown in A)*	16,300	16,300

11. I certify that the statements made by me above are correct and complete.

SIGNATURE AND TITLE OF EDITOR, PUBLISHER, BUSINESS MANAGER, OR OWNER
Marjorie E. Miles
Marjorie E. Miles
Administrative Officer

12. FOR COMPLETION BY PUBLISHERS MAILING AT THE REGULAR RATES *(Section 132.121, Postal Service Manual)*

39 U. S. C. 3626 provides in pertinent part: "No person who would have been entitled to mail matter under former section 4359 of this title shall mail such matter at the rates provided under this subsection unless he files annually with the Postal Service a written request for permission to mail matter at such rates."

In accordance with the provisions of this statute, I hereby request permission to mail the publication named in Item 1 at the phased postage rates presently authorized by 39 U. S. C. 3626.

SIGNATURE AND TITLE OF EDITOR, PUBLISHER, BUSINESS MANAGER, OR OWNER
Marjorie E. Miles
Marjorie E. Miles
Administrative Officer

PS Form 3526 *(Page 1)*
Aug. 1978

[See instructions on reverse)

1980

EDITORIAL CONSULTANTS

Abramson, Harold J.
Acock, Alan C.
Akers, Ronald L.
Albrecht, Gary
Albrecht, Stan
Althauser, Robert
Alwin, Duane
Amachree, Igolima T. D.
Antonio, Robert
Babchuk, Nicholas
Barchas, Patricia
Baron, James
Barton, Allen
Bean, Frank D.
Beck, E. M.
Bellah, Robert N.
Bengston, Vern
Bensman, Joseph
Benson, Kenneth
Berg, Ivar
Berk, Richard A.
Bibb, Robert
Biderman, Albert
Bidwell, Charles
Bielby, William
Bittner, Egon
Black, Donald
Blalock, Hubert M., Jr.
Blau, Peter M.
Blau, Zena Smith
Bohrnstedt, George
Bonacich, Edna
Bonacich, Phillip
Bord, Richard
Boulding, Elise
Bredo, Eric
Breton, Raymond
Breiger, Ronald L.
Browning, Charles
Brymer, Richard
Bucher, Rue
Buckley, Walter
Bunker, Stephen G.
Burke, Peter J.
Burstein, Paul
Burt, Ronald S.
Cannon, Lynn
Cardascia, John
Carey, James W.
Chandler, David
Chase, Allan
Chilton, Roland
Choldin, Harvey
Clark, John P.
Clark, Terry N.
Clausen, John A.
Clemente, Frank
Clignet, Remi
Clogg, Clifford
Clough, Patricia
Coates, Robert
Cohen, Larry
Cole, Jonathan
Cole, Stephen
Collins, Randall
Collver, Andrew
Corsaro, William
Coser, Lewis
Costner, Herbert L.
Crain, Robert
Cramer, James C.
Crawford, Robert
Crosbie, Paul V.
Cutright, Phillips
D'Amico, Ronald J.
Darrow, William
Daymont, Thomas N.
Demerath, N. J.
Direnzo, Gordon
DiTomaso, Nancy
Dohrenwend, Barbara
Douglas, Jack D.
Dreeben, Robert
Dutton, Diana
Eckberg, Douglas L.
Eckland, Bruce K.
Emerson, Richard
Empey, Lamar T.
Entwistle, Doris R.
Fararo, Thomas
Farber, Bernard
Farley, Reynolds
Faulkner, Robert R.
Faunce, William A.
Feagin, Joe R.
Featherman, David L.
Feinberg, William
Felson, Marc
Ferree, Myra Marx
Fienberg, Steven
Fine, Gary A.
Firebaugh, Glenn
Fischer, Claude
Fliegel, Frederick
Flood, Ann
Form, William
Frisbie, Parker
Gagnon, John
Galaskiewicz, Joseph
Gale, Richard
Galliher, John
Gamson, William
Gans, Herbert
Geis, Gilbert
Gelles, Richard
Geschwender, James
Gitlin, Todd
Gibbs, Jack P.
Glaser, Daniel
Goffman, Erving
Gordon, Robert A.
Gortmaker, Steven L.
Gottfredson, Michael
Gottlieb, David
Gouldner, Alvin W.
Gove, Walter R.
Graña, Cesar
Grandjean, Burke D.
Granovetter, Mark
Gregory, Paul R.
Griffin, Larry J.
Grimshaw, Allen
Guest, Avery
Guten, Sharon M.
Guterbock, Thomas M.
Habenstein, Robert
Hadden, Jeffrey K.
Hage, Jerald T.
Hall, Peter M.
Hall, Richard H.
Haller, Archibald O.
Hallinan, Maureen T.
Hamblin, Robert L.
Hamilton, Richard
Hannan, Michael T.
Hargens, Lowell L.
Harris, Anthony
Haug, Marie
Hauser, Robert M.
Hawkins, Richard
Hawley, Amos H.
Heirich, Max A.
Heise, David
Hepburn, John
Hewitt, John
Heyl, John
Higgins, Paul
Hinkle, Gisela
Hinkle, Roscoe
Hirsch, Paul
Hirschi, Travis W.
Hodson, Randy D.
Hogan, Dennis P.
Hollander, Paul
Hood, Thomas C.
Hraba, Joseph
Huber, Joan
Imrey, Peter B.
Jackman, Mary R.
Jackman, Robert W.
Jackson, Elton
Jasso, Guillermina
Jencks, Christopher
Jenkins, Craig
Johnson, Benton
Johnson, Michael P.
Johnson, Norris
Johnstone, John
Jones, Robert A.
Kalleberg, Arne
Kamens, David H.
Kanter, Rosabeth
Karsh, Bernard
Kasarda, John D.
Kelly, William
Kelman, Herbert
Key, William
Kim, Jae-On
Kimberly, James C.
Kimberly, John
King, David
Klaff, Vivian
Kleck, Gary
Kluegel, James
Knoke, David

Kohout, Frank
Kraus, Richard
Kriesberg, Louis
Land, Kenneth
Laslett, Barbara
Lawson, Ron
Leff, Nathaniel
Leinhardt, Samuel
Lenski, Gerhard
Lever, Janet
Levin, Donald
Lewis, J. David
Lewis, Michael
Lieberson, Stanley
Liebert, Roland
Light, Donald
Light, Ivan
Lin, Nan
Lincoln, James R.
Liska, Allen
Lizotte, Alan
Lofland, John
Lofland, Lyn H.
Loftin, Colin K.
Logan, John
Lundman, Richard
McCarthy, John
McCartney, James L.
McMillen, David
McNamee, Steven
McPhail, Clark
Maines, David R.
Mare, Robert D.
Marini, Margaret
Marx, Gary T.
Mason, Karen O.
Mayer, Lawrence
Mayhew, Bruce
Mazur, Allan
Melbin, Murray
Meltzer, Bernard
Mercer, Jane
Meyer, John
Michelson, William
Miller, Dan E.
Mirowsky, John
Mitroff, Ian
Moch, Michael
Modigliani, Andre
Molotch, Harvey L.
Montero, Darrel M.
Moore, Gwen
Moore, Joan W.
Morrison, Denton
Moskos, Charles C., Jr.
Mueller, Charles W.
Mullins, Nicholas C.
Nahemow, Nina
Nathanson, Constance
Nielsen, François
Oberschall, Anthony
Olneck, Michael R.
Oppenheimer, Valerie
Ornstein, Michael
Orum, Anthony
Ouchi, William G.
Pampel, Fred, Jr.
Panelas, Thomas
Parish, William
Pavalko, Ronald M.
Payne, David
Perinbanayagam, R. S.
Perrow, Charles B.
Pfeffer, Jeffrey
Phillips, David
Pinard, Maurice
Pineo, Peter C.
Ploch, Donald R.
Pollner, Melvin
Pondy, Louis
Pope, Whitney
Porter, Jack N.
Porter, Judith R.
Portes, Alexander
Porton, Gary
Poston, Dudley L.
Pratt, Lois
Preston, Samuel
Pullum, Thomas
Quadagno, Jill
Quarantelli, Enrico L.
Quinn, Robert
Rainwater, Lee
Ralph, Stanley
Ratcliff, Richard
Rau, William
Reed, John S.
Reskin, Barbara
Rexroat, Cynthia
Reynolds, Paul D.
Robinson, Jerry W., Jr.
Robinson, Robert
Rodgers, Willard
Rogers, Everett M.
Roistacher, Richard C.
Roncek, Dennis
Roof, W. Clark
Rose, Ed
Rose, Vicki
Rosenbaum, James
Rosenberg, Morris
Rosenfeld, Rachel
Ross, Catherine E.
Roth, Guenther
Rubinson, Richard
Rushing, William A.
Ryder, Norman B.
Salloway, Jeff
Sampson, Frank
Scarr, Sandra
Schoen, Robert
Schoenberg, Ronald
Schoenherr, Richard A.
Schooler, Carmi
Schwartz, Barry
Schwartz, Mildred A.
Schwartz, Pepper
Schwirian, Kent P.
Seeman, Melvin
Segal, David
Seiler, Lauren
Sewell, William H.
Shephard, Jon
Shimkin, Demitri
Short, James F.
Shover, Neal
Simon, William
Singer, Eleanor
Skocpol, Theda
Sly, David
Smelser, Neil J.
Smith, Joel
Smith-Lovin, Lynn
Smucker, Joseph
Snow, David
Snyder, David
Sørenson, Aage
Spaeth, Joe
Spates, James
Spenner, Kenneth
Spilerman, Seymour
Spitze, Glenna
Stafford, Mark
Stewart, Robert L.
Stewman, Shelby
Stokes, Randall G.
Strodtbeck, Fred
Stryker, Sheldon
Sudman, Seymour
Suttles, Gerard
Swafford, Michael
Sykes, Richard
Szymanski, Albert
Tausky, Curt
Taylor, D. Garth
Temme, Lloyd V.
Tenhouten, Warren D.
Terry, Robert
Tilly, Charles
Tilly, Louise
Treiman, Donald
Tiryakian, Edward
Tittle, Charles R.
Treas, Judith
Tucker, Charles
Tuma, Nancy Brandon
Turner, Jay
Turner, Ralph
Tyree, Andrea
Udy, Stanley H., Jr.
Useem, Michael
Van Arsdol, Maurice D., Jr.
Van den Berghe, Pierre
Vanneman, Reeve
Verbrugge, Lois M.
Vigderhous, Gideon
Villemez, Wayne J.
Wahrman, Ralph
Waite, Linda J.
Webster, Murray
Weigart, Andrew
Weiland, Marion
Weinberg, Martin
Weinstein, Eugene A.
Weiting, Stephen G.
Wenger, Dennis
Wheaton, Blair
Whetten, David
Wilensky, Harold L.
Wiley, David
Wilkening, Eugene

Williams, Robin M., Jr.
Wilson, John
Wilson, Thomas P.
Winship, Christopher
Wisan, Gail
Wright, Erik O.
Wright, James
Zald, Mayer N.
Zeitlin, Maurice
Zerubavel, Eviatar
Zimmerman, Don H.
Zucker, Lynne G.

INDEX

AMERICAN SOCIOLOGICAL REVIEW

Published Bimonthly by the American Sociological Association

CONTENTS OF VOLUME 45, NUMBERS 1–6

ARTICLES

Abrahamson, Mark Sudden Wealth, Gratification and Attainment: Durkheim's Anomie of Affluence Reconsidered 49
Ageton, Suzanne S. See Delbert S. Elliott 95
Albonetti, Celesta See John Hagan 802
Andrews, D. A. Some Experimental Investigations of the Principles of Differential Association through Deliberate Manipulations of the Structure of Service Systems 448
Baron, James, N., and William T. Bielby Bringing the Firms Back In: Stratification, Segmentation, and the Organization of Work 737
Berg, Bruce See Barry Glassner 647
Berk, Richard A., Kenneth J. Lenihan, and Peter H. Rossi Crime and Poverty: Some Experimental Evidence from Ex-Offenders 766
Bielby, William T. See James N. Baron 737
Blumer, Herbert Comment: Mead and Blumer: The Convergent Methodological Perspectives of Social Behaviorism and Symbolic Interactionism 409
Bohrer, Robert See Harvey M. Choldin 972
Bollen, Kenneth A. Issues in the Comparative Measurement of Political Democracy 370
Bordua, David J. See Alan J. Lizotte 229
Bridges, William P. Industry Marginality and Female Employment: A New Appraisal 58
Bumpass, Larry See Ronald R. Rindfuss 431
Burt, Ronald S., Kenneth P. Christman, and Harold C. Kilburn, Jr. Testing a Structural Theory of Corporate Cooptation: Interorganizational Directorate Ties As a Strategy for Avoiding Market Constraints on Profits 821
Chase, Ivan D. Social Process and Hierarchy Formation in Small Groups: A Comparative Perspective 905
Choldin, Harvey M., Claudine Hanson, and Robert Bohrer Suburban Status Instability 972
Christman, Kenneth P. See Ronald S. Burt 821
Cleary, Paul D. See Ronald C. Kessler 463
Collins, Randall Weber's Last Theory of Capitalism: A Systematization 925
Converse, Philip E. Rejoinder to Judd and Milburn 644
Cramer, James C. Fertility and Female Employment: Problems of Causal Direction 167
Della Fave, L. Richard The Meek Shall Not Inherit the Earth: Self-Evaluation and the Legitimacy of Stratification 955
Ekland-Olson, Sheldon See David A. Snow 787
Elliott, Delbert S., and Suzanne S. Ageton Reconciling Race and Class Differences in Self-Reported and Official Estimates of Delinquency 95
Evans, Peter B., and Michael Timberlake Dependence, Inequality, and the Growth of the Tertiary: A Comparative Analysis of Less Developed Countries 531
Glassner, Barry, and Bruce Berg How Jews Avoid Alcohol Problems 647
Hagan, John, Ilene H. Nagel (Bernstein), and Celesta Albonetti The differential Sentencing of White-Collar Offenders in Ten Federal District Courts 802
Hanson, Claudine See Harvey M. Choldin 972
Hogan, Dennis P. The Transition to Adulthood As a Career Contingency 261
Hong, Doo-Seung See Hagen Koo 610
Isaac, Larry, Elizabeth Mutran, and Sheldon Stryker Political Protest Orientations among Black and White Adults 191
Jasso, Guillermina A New Theory of Distributive Justice 3
Jencks, Christopher Heredity, Environment, and Public Policy Reconsidered 723
Judd, Charles M., and Michael A. Milburn Comment: Converse: The Structure of Attitude Systems in the General Public: Comparisons of a Structural Equation Model 627

Kessler, Ronald C., and Paul D. Cleary Social Class and Psychological Distress ... 463
Kilburn, Harold C., Jr. See Ronald S. Burt ... 821
Konda, Suresh L., and Shelby Stewman An Opportunity Labor Demand Model and Markovian Labor Supply Models: Comparative Tests in an Organization ... 276
Koo, Hagen, and Doo-Seung Hong Class and Income Inequality in Korea ... 610
LaFree, Gary D. The Effect of Sexual Stratification by Race on Official Reactions to Rape ... 842
Laslett, Barbara Beyond Methodology: The Place of Theory in Quantitative Historical Research ... 214
Lenihan, Kenneth J. See Richard A. Berk ... 766
Lester, Marilyn Generating Newsworthiness: The Interpretive Construction of Public Events ... 984
Lincoln, James R., and Gerald Zeitz Organizational Properties from Aggregate Data: Separating Individual and Structural Effects ... 391
Lizotte, Alan J., and David J. Bordua Firearms Ownership for Sport and Protection: Two Divergent Models ... 229
McPhail, Clark, and Cynthia Rexroat Rejoinder: *Ex Cathedra* Blumer or *Ex Libris* Mead? ... 420
Mercy, James A. See Lala Carr Steelman ... 571
Meyer, John W. See John Boli-Bennett ... 525
Milburn, Michael A. See Charles M. Judd ... 627
Miller, Jon Access to Interorganizational Networks As a Professional Resource ... 479
Mutran, Elizabeth See Larry Isaac ... 191
Nagel, Ilene H. (Bernstein) See John Hagan ... 802
Nielsen, François The Flemish Movement in Belgium After World War II: A Dynamic Analysis ... 76
Ralph, John H., and Richard Rubinson Immigration and the Expansion of Schooling in the United States, 1890–1970 ... 943
Ratcliff, Richard E. Banks and Corporate Lending: An Analysis of the Impact of the Internal Structure of the Capitalist Class on the Lending Behavior of Banks .. 553
Rau, William C. The Tacit Conventions of the Modernity School: An Analysis of Key Assumptions ... 244
Rexroat, Cynthia See Clark McPhail ... 420
Rindfuss, Ronald R., Larry Bumpass, and Craig St. John Education and Fertility: Implications for the Roles Women Occupy ... 431
Rosenfeld, Rachel A. Race and Sex Differences in Career Dynamics ... 583
Rossi, Peter H. See Richard A. Berk ... 766
Rossi, Peter H. The Presidential Address: The Challenge and Opportunities of Applied Social Research ... 889
Rubinson, Richard See John H. Ralph ... 943
St. John, Craig See Ronald R. Rindfuss ... 431
Snow, David A., Louis A. Zurcher, Jr., and Sheldon Ekland-Olson Social Networks and Social Movements: A Microstructural Approach to Differential Recruitment 787
Steelman, Lala Carr, and James A. Mercy Unconfounding the Confluence Model: A Test of Sibship Size and Birth-Order Effects on Intelligence ... 571
Stewman, Shelby See Suresh L. Konda ... 276
Stryker, Sheldon See Larry Isaac ... 191
Swafford, Michael Three Parametric Techniques for Contingency Table Analysis: A Nontechnical Commentary ... 664
Timberlake, Michael See Peter B. Evans ... 531
Tolbert, Charles M., II See E. M. Beck ... 712
Useem, Bert Solidarity Model, Breakdown Model, and the Boston Anti-Busing Movement ... 357
Weinberg, Martin S., and Colin J. Williams Sexual Embourgeoisment? Social Class and Sexual Activity: 1938–1970 ... 33
Williams, Colin J. See Martin S. Weinberg ... 33
Zeitz, Gerald See James R. Lincoln ... 391
Zurcher, Louis A., Jr. See David A. Snow ... 787

RESEARCH NOTES

Allison, Paul D. Analyzing Collapsed Contingency Tables without Actually Collapsing 123
Bucuvalas, Michael J. See Carol H. Weiss 302
McGranahan, David A. The Spatial Structure of Income Distribution in Rural Regions 313
Shelley, Louise The Geography of Soviet Criminality 111
Smith, Douglas A., and Christy A. Visher Sex and Involvement in Deviance/Crime: A Quantitative Review of the Empirical Literature 691
Visher, Christy A. See Douglas A. Smith 691
Weede, Erich Beyond Misspecification in Sociological Analyses of Income Inequality 497
Weiss, Carol H., and Michael J. Bucuvalas Truth Tests and Utility Tests: Decision-Makers' Frames of Reference for Social Science Research 302

COMMENTS AND REPLIES

Albrecht, Sandra L. See Norval D. Glenn 340
Attewell, Paul, and Robert Fitzgerald Comparing Stratification Theories (A Comment on Robinson and Kelley, ASR February 1979) 325
Beck, E. M., Patrick Horan, and Charles M. Tolbert, II Social Stratification in Industrial Society: Further Evidence for a Structural Alternative (Reply to Hauser) 712
Biles, David See John Braithwaite 334
Boli-Bennett, John, and John W. Meyer Constitutions As Ideology (Reply to Ratner-Burstein) 525
Bolin, Robert, and Susan Bolton Bolin Sociobiology and Paradigms in Evolutionary Theory (Comment on Quadagno, ASR February 1979) 154
Bolin, Susan Bolton See Robert Bolin 154
Booth, Alan, David R. Johnson, and John N. Edwards Reply to Gove and Hughes 870
Booth, Alan, David R. Johnson, and John N. Edwards In Pursuit of Pathology: The Effects of Human Crowding (Comment on Gove, Hughes, and Galle, ASR February 1979) 873
Cook, Karen S., Mary R. Gillmore, and Toshio Yamagishi Does Microeconomic Theory Need a Defense? (A Reply to Martin and Sell) 351
Edwards, John N. See Alan Booth 870, 873
Firebaugh, Glenn The Case of the *Missing-Values* Card, and Other Mysteries: Another Look at the Effect of Government Spending on Income Inequality (Comment on Stack, ASR December 1978) 325
Fitzgerald, Robert See Paul Attewell 325
Franke, Richard Herbert Worker Productivity at Hawthorne (Reply to Schlaifer) 1006
Gillmore, Mary R. See Karen S. Cook 351
Glenn, Norval D., and Sandra L. Albrecht Is the Status Structure in the United States Really More Fluid for Women Than for Men? (Comment on Glenn, Ross, and Tully, ASR October 1974) 340
Gottfredson, Michael R., and Michael J. Hindelang Trite but True (Response to Braithwaite and Biles) 338
Gove, Walter R., and Michael Hughes Sex Differences in Physical Health and How Medical Sociologists View Illness (Reply to Mechanic and Verbrugge) 514
Gove, Walter R., and Michael Hughes The Effects of Crowding Found in the Toronto Study: Some Methodological and Empirical Questions (A Comment on Booth and Edwards, ASR 1976) 864
Gove, Walter R., and Michael Hughes In Pursuit of Preconceptions: A Reply to the Claim of Booth and His Colleagues That Household Crowding Is Not an Important Variable (Reply to Booth, Johnson, and Edwards) 878
Greeley, Andrew M. Comment on Newport Article, ASR August 1979 719
Hauser, Robert M. On "Stratification in a Dual Economy" (Comment on Beck et al., ASR October 1978) 702
Hewitt, Christopher Reply to Jackman 347
Hindelang, Michael J. See Michael R. Gottfredson 338
Horan, Patrick See E. M. Beck 712
Hughes, Michael See Walter R. Gove 514, 864, 878
Jackman, Robert W. Keynesian Government Intervention and Income Inequality (Comment on Stack, ASR December 1978) 131
Jackman, Robert W. The Impact of Outliers on Income Inequality (Comment on

Hewitt, ASR June 1977; Stack, ASR February 1979; and Hewitt, ASR February 1979) ... 344
Johnson, David R. See Alan Booth ... 870, 873
Kelley, Jonathan See Robert V. Robinson ... 328
Lutz, James M. Welsh Political Mobilization: A Comment and a Note (Comment on Ragin, ASR August 1979) ... 1028
Martin, Michael W., and Jane Sell In Defense of Microeconomic Theory (Comment on Cook and Emerson, ASR October 1978) ... 349
Mechanic, David Comment on Gove and Hughes, ASR February 1979 ... 513
Morgan, Charles J. Darwin, Sociobiology and Sociology (Comment on Quadagno, ASR February 1979) ... 502
Newport, Frank Reply to "Comment on Newport Article, ASR August 1979" ... 720
Quadagno, Jill S. Reply to Bolin and Bolin ... 159
Quadagno, Jill S. Reply to Morgan ... 505
Ragin, Charles C. Aggregation Gain and Loss in Electoral Research (A Response to Lutz's Comment) ... 1031
Ratner, Ronnie Steinberg, and Paul Burstein Ideology, Specificity, and the Coding of Legal Documents: What Do Measures of Constitutional Content Measure? (Comment on Boli-Bennett and Meyer, ASR December 1978) ... 522
Scarr, Sandra, and Richard A. Weinberg Calling All Camps! The War Is Over! (Reply to Taylor) ... 859
Schlaifer, Robert The Relay Assembly Test Room: An Alternative Statistical Interpretation (Comment on Franke and Kaul, ASR October 1978) ... 995
Sell, Jane See Michael W. Martin ... 349
Stack, Steven Direct Government Involvement in the Economy: Theoretical and Empirical Extensions (Reply to Jackman and Firebaugh) ... 146
Taylor, Marcia Whicker The Noninfluence of "Family Background" on Intellectual Attainment (A Critique of Scarr and Weinberg, ASR October 1978) ... 855
Verbrugge, Lois M. Possible Causes of the Apparent Sex Differences in Physical Health (Comment on Walter R. Gove and Michael Hughes, ASR February 1979) 507
Weinberg, Richard A. See Sandra Scarr ... 859
Yamagishi, Toshio See Karen S. Cook ... 351

CHARLES C THOMAS • PUBLISHER

New! **POLICE HUMAN RELATIONS** *edited by* **George Henderson,** *Univ. of Oklahoma, Norman. (17 Contributors) '81, 368 pp., 3 il., 11 tables, cloth-$22.75, paper-$16.50*

New! **BECOMING A WOMAN: The Socialization of Gender** *by* **Bernice Lott,** *Univ. of Rhode Island, Kingston. '80, 464 pp., 6 il., cloth-$24.75, paper-$18.50*

New! **CHILD WELFARE FORECASTING: Context and Technique** *edited by* **John L. Craft, Stephen W. Epley** *and* **William M. Theisen,** *all of Univ. of Iowa, Iowa City. (10 Contributors) '80, 216 pp., 27 il., 49 tables, $21.75*

New! **WOMEN IN LAW ENFORCEMENT (2nd Ed.)** *by* **Peter Horne,** *St. Louis Community College at Meramec, St. Louis, Missouri. Foreword by* **Lewis J. Sherman.** *'80, 288 pp., 7 il., cloth-$15.50, paper-$10.50*

New! **CRIME AND THE ENVIRONMENT** *by* **K. D. Harries,** *Oklahoma State Univ., Stillwater. '80, 164 pp., 13 il., 21 tables, $14.00*

SOCIETY'S VICTIM—THE POLICEMAN: An Analysis of Job Stress in Policing (3rd Ptg.) *by* **William H. Kroes,** *Institute for Stress Management, Los Angeles. '80, 144 pp., 14 il., 4 tables, $12.50*

LAW, PSYCHOLOGY, AND THE COURTS: Rethinking Treatment of the Young and the Disturbed *by* **Ellsworth A. Fersch Jr.,** *Harvard Univ., Cambridge, Massachusetts. '79, 184 pp., 2 tables, $16.25*

OLD AND GROWING: Conversations, Letters, Observations, and Reflections on Growing Old *by* **Florence E. Vickery,** *College of Marin, Kentfield, California. Foreword by* **Jack Ossofsky.** *'78, 176 pp., 9 il., $13.00*

THE ECONOMICS OF CRIME AND LAW ENFORCEMENT *edited by* **Lee R. McPheters** *and* **William B. Stronge,** *both of Florida Atlantic Univ., Boca Raton. (29 Contributors) '76, 520 pp., 49 il., 55 tables, $25.00*

WHO SPEAKS FOR THE CHILD? *by* **Norman E. Silberberg,** *Sister Kenny Institute, Minneapolis, Minnesota, and* **Margaret C. Silberberg,** *St. Paul, Minnesota. '74, 226 pp., 4 il., cloth-$15.25, paper-$11.50*

THE OLDER WOMAN

New!

Lavender Rose or Gray Panther

Edited by **Marie Marschall Fuller,** *Texas Woman's Univ., and* **Cora Ann Martin,** *North Texas State Univ., both of Denton. (41 Contributors)* The thirty-three articles in this anthology present a sociological view of the unique problems and needs of the older woman in today's society. Special emphasis throughout the book is on the recognition of individuality and on the evolving role of women.

Following a broad profile of older women in contemporary America, the book is divided into seven sections as follows:

Part One offers a statistical description of the older woman, including employment patterns, marital status, and numbers.
Part Two underscores the socialization process of women and their varying responses to changing times.
Part Three examines biological facts and fallacies related to sex, menopause, health, and mental health.
Part Four explores the social bonds that tie older women into meaningful interpersonal relationships. Marriage, widowhood, familial relationships, and friendships are discussed.
Part Five focuses on older women and traditional institutions of society—family, economy, politics, religion, and education.
Part Six discusses lifestyles that characterize the older woman.
Part Seven forecasts the position of the older woman in tomorrow's world.

'80, 368 pp., 19 tables, $16.75, paper

Orders with remittance sent, on approval, postpaid

301-327 East Lawrence Avenue • Springfield • Illinois • 62717

A Society in the Making

Hungarian Social and Societal Policy 1945-75

By Zsuzsa Ferge   Preface by S. M. Miller

"... a milestone in social policy analysis.... an enormously rich melding of social policy perspectives developed by writers like Marshall and Titmus with systematic Marxist analysis. Ferge's approach provides a rich paradigm for social policy analysts of the future."

$22.50 —*Lee Rainwater, Harvard University*

Neocolonial Identity and Counter-Consciousness

Essays on Cultural Decolonization

By Renato Constantino   Edited with an introduction by István Mészáros

"... the first fundamental assessment of cultural imperialism.... a major classic in the field.... (The author) has brilliantly explored the foreign-defined educational system and values that become the vehicles for cultural identity.... a major, brilliant effort..." —*Gabriel Kolko, York University*

$20.00

Socialist Population Politics

The Political Implications of Demographic Trends in the USSR and Eastern Europe

By John F. Besemeres

"... a bold and fascinating political analysis of the implications ensuing from the current and pending shifts in the population of the USSR, Poland and Yugoslavia.... Besemeres fills a gap between ideology, domestic and international politics, and demography with particularly interesting materials and speculation." —*Murray Feshbach, Fellow/The Wilson Center*

$25.00

Soviet Work Attitudes

The Issue of Participation in Management

Edited by Murray Yanowitch

"Important reading for anyone who wants to know what the Soviet Union is really like. It reports on major and reliable research by Soviet social scientists."

$15.00 —*Seymour Martin Lipset, Stanford University*

Chinese Approaches to Family Planning

Edited with an introduction by Leo A. Orleans

"A fascinating and important view of what Chinese leaders are telling their people to educate them about family planning.... a major contribution to increasing our understanding of China."

$18.00 —*Ronald Freedman, University of Michigan*

Scratches on Our Minds

American Views of China and India (Previously reprinted as *Images of Asia*)

By Harold R. Isaacs

"... a seminal book, and one which might profitably inspire a flood of similar inquiries into our ideas of other regions.... Mr. Isaacs' painstaking research should go far toward reducing misunderstandings arising from sheer ignorance and from uncontrolled emotion." —*The Annals of the American Academy of Political and Social Science*

Photographs   Paper $7.95

M.E. Sharpe, Inc.

901 North Broadway, White Plains, NY 10603

DORSEY

Quality texts for 1981....

CONTEMPORARY SOCIAL PROBLEMS
Robert R. Bell, *Temple University*

Focuses on how social problems emerge over time and examines their various stages.

HUMAN RESPONSES TO SOCIAL PROBLEMS
Louis Schneider, *late of the University of Texas at Austin,* **Cookie White Stephan,** *New Mexico State University,* **Louis A. Zurcher, Jr.,** and **Sheldon Ekland-Olson,** *both of the University of Texas at Austin*

Unprecedented new text focuses on how people define, analyze, and attempt to deal with the issues and events in society which they consider to be social problems.

THE EMERGENCE OF SOCIOLOGICAL THEORY
Jonathan A. Turner, *University of California - Riverside,* and
Leonard Beeghley, *University of Florida*

Summarizes and analyzes the intellectual origins, profile and theoretical implications of 19th and early 20th century social thinkers.

Examination copies for adoption consideration available on request.
Please indicate course title and text presently used.

THE DORSEY PRESS
Homewood, Illinois 60430

A NEW WAVE
of Sociology Texts for 1981

PROBLEMS OF AN INDUSTRIAL SOCIETY, 2/e
WILLIAM A. FAUNCE, Michigan State University
256 pages (tent.), $8.95 (tent.)

Within an historical and developmental framework, this new edition examines a wider range of problems found in industrial societies, including problems of the aged and resource depletion.

INTRODUCTION TO SOCIAL POLICY
ELIZABETH D. HUTTMAN, California State University, Hayward
420 pages (tent.), $14.95 (tent.)

This new text clearly presents social policy concepts and then applies them to particular social welfare areas, emphasizing the real-world function of social policy.

INTRODUCTION TO SOCIAL RESEARCH, 3/e
The late **SANFORD LABOVITZ**; and **ROBERT HAGEDORN**,
University of Victoria, Canada
176 pages (tent.), $8.95 (tent.)

This successful text provides a broad, integrated treatment of basic concepts and key research ingredients of social research.

THE URBAN WORLD, 2/e
J. JOHN PALEN, Virginia Commonwealth University
480 pages (tent.), $17.95 (tent.); Instructor's Manual

Substantially revised, the second edition features solid, readable coverage of recent trends in urban sociology and retains the cross-cultural perspective of the first edition.

MEN, WOMEN, AND CHANGE: A Sociology of Marriage and Family, 2/e
LETHA DAWSON SCANZONI and **JOHN SCANZONI**,
University of North Carolina, Greensboro
608 pages (tent.), $17.95 (tent.); Instructor's Manual

Solidly grounded in sociological theory, the new second edition provides a practical framework for understanding the dynamic process involved in premarital, marital, and alternative relationships.

SOCIOLOGY: The Human Science, 2/e
ELBERT W. STEWART
656 pages (tent.), $18.95 (tent.); Instructor's Manual; Test File; The Examiner System, a computerized test-generation system

A book for the 1980s, the second edition adopts social change as its integrating theme, addresses important new developments, and offers an excellent built-in Study Guide at the end of every chapter.

COLLEGE DIVISION • McGRAW-HILL BOOK COMPANY
1221 Avenue of the Americas • New York, N.Y. 10020

Prices subject to change.

THE NATION'S FAMILIES: 1960-1990

George Masnick, Harvard University
Mary Jo Bane, Harvard University

Foreword by **David T. Kresge,** Director of the Joint Center
for Urban Studies of MIT and Harvard University

The Nation's Families: 1960-1990 identifies current population, household, and lifestyle trends and interprets their implications for future policy planning and institutional accommodation. The authors set aside several misconceptions about recently observed changes in the American family and suggest changes in store over the next decade. Among the factors of family life measured and projected are principal householders' marital status, household structure, women's participation in the labor force, contributions to household income, personal relationships, and lifestyles. Special attention is given to trends and differentials in the behavior of birth cohorts as they age over the life course. The text is highlighted by over 50 graphs and tables.

HARDBOUND EDITION, 1980 ISBN 0-86569-050-2 $17.95
PAPERBOUND EDITION, 1980 ISBN 0-86569-051-0 $10.00

Examination copies of the hardbound edition are available at 50% of regular price. Payment will be refunded upon evidence of multi-copy purchase from Auburn House for class use.

The paperbound edition is available for purchase only at regular price plus $1.50 postage and handling. Multi-copy orders add $1 per book for postage and handling. Orders from individuals must be accompanied by payment. Massachusetts residents add 5% sales tax.

Master Charge and Visa
accepted for orders over $15.
When ordering by credit card,
please include card number
and its expiration date.

Address orders to
Auburn House
Publishing Company
131 Clarendon Street
Boston, Massachusetts 02116
Telephone 617-247-2650

The Annenberg School of Communications

The University of Pennsylvania
Philadelphia

A graduate program for new careers in communications scholarship, research and policymaking.

A select group of candidates for the Master of Arts and the Ph.D. in Communications learns the theories and methods of scholarship in interpersonal and mass communications; visual communication and cultural analysis; and media management and regulation.

The program is designed for the superior student in the humanities and social sciences. It provides the guidance of a distinguished faculty, academic resources of a major research university, and the financial aid needed to pursue full-time study. Recent graduates have assumed posts in scholarly and scientific institutions; government, business and industry; and media research and management.

For information write Admissions, The Annenberg School of Communications, 3620 Walnut Street, Philadelphia, PA. 19104 or call (215) 243-7041.

The Annenberg School of Communications at the University of Pennsylvania welcomes applicants without regard to race, color, sex, sexual or affectional preference, age, religion, national or ethnic origin or physical handicap.

Teenage Sexuality, Pregnancy, and Childbearing

Frank F. Furstenberg, Jr., Richard Lincoln, and Jane Menken, Editors

Examining the social consequences of teenage childbearing, the editors explain, in particular, the effects on adolescent parents themselves, their offspring, and their families. These essays search for the determinants of early parenthood, consider the means for preventing premature childbearing, and discuss ways of tempering its consequences.

This comprehensive and useful book reviews the new evidence on the economic, political, and social impact of teenage sexual activity on our society. Twenty-eight key reports provide a concise collection of reference material with an up-to-date bibliography at the end of each selection.

$22.95 cloth **416 pages**
$10.50 paper

University of Pennsylvania Press
3933 Walnut Street
Philadelphia, Pennsylvania 19104

INTERNATIONAL SYMPOSIUM ON SINGLE-PARENT FAMILIES

BEN-GURION UNIVERSITY OF THE NEGEV, BEER SHEVA, ISRAEL, JULY 5—9, 1981

Solly Dreman Ph.D., Chairperson

With the Participation of: Donald Bloch M.D., Frederick Duhl M.D., Bonny Duhl M.Ed., Jay Haley M.A. (tentative), Martin Kirschenbaum Ph.D., Elisabeth Kübler-Ross M.D., Salvador Minuchin M.D., Peggy Papp M.S.W., Cliff Sager M.D., Robert Siroka Ph.D., Carl Whitaker M.D. and others.

Proposed Topics: Death and terminal illness; Divorce and separation; Alternate life-styles; Social and legal implications and support systems; Cross-cultural differences; The extended family; Blended families and remarriage; The non-functioning parent.

For further information:
The Secretariat
International Symposium on Single-Parent Families
P.O.B. 3378
Tel Aviv, Israel

Strategic Styles

Coping in the Inner City

JANET K. MANCINI

Portraits of five black males amid the social and physical decay of the inner city. Shows how unique individuals develop in a society which throws many strikes against its members. $15.95

The Man with a Steel Guitar

A Portrait of Ambition, Desperation, and Crime

NORMAN GREENBERG

An absorbing life history of a talented country musician who was forced to turn to armed robbery as his quest for wealth and fame was continually thwarted. "An interesting document"—*Kirkus Reviews*. $10.95

Work Hazards and Industrial Conflict

CARL GERSUNY

Hardly a day passes without reports on litigation over work injuries or conflicts over standards for hazardous substances. This thoughtful book offers new ways of looking at a perennial problem. *A University of Rhode Island book*. $12.00

Competence and Coping During Adulthood

LYNNE A. BOND *and* JAMES C. ROSEN, *editors*

Features "research findings on questions concerning life satisfaction, life stress, family, work, physical health, and life planning"—*APA Monitor. A book from the Vermont Conference on the Primary Prevention of Psychopathology*. $20.00

University Press of New England

Hanover, New Hampshire and London, England

CHICAGO

Since 1891, Publishers of Scholarly Books and Journals

Village and Family in Contemporary Contemporary China

William L. Parish and Martin King Whyte

"This book tells us much that we always wanted to know about China but feared we would never find out. As such it is the most important sociological contribution to our understanding of the People's Republic of China yet to appear."—G. William Skinner, Stanford University

Paper 436 pages $8.95 Available

Homo Hierarchicus

The Caste System and Its Implications

Complete Revised English Edition

Louis Dumont

"This book is a major contribution to the general sociological theory of Indian caste."—*South Asia Review*

Paper $TBA November Also available in cloth.

A Nation of Behavers

Martin E. Marty

"As a means of making sense out of the potpourri of competing groups that compose religious America today, *A Nation of Behavers* is a first-rate tool."—Edward R. Fiske, *New York Times*

Paper 252 pages $5.95 Available

Mehinaku

The Drama of Daily Life in a Brazilian Indian Village

Thomas Gregor

"Gregor's book is wonderful, a must for anyone interested in Amazonia or in the working out of the dramaturgical metaphor in a society which Erving Goffman himself might have designed."
—*Explorer*

Paper 398 pages $7.95 Available

Madness in Society

Chapters in the Historical Sociology of Mental Illness

George Rosen

"The author of this fascinating volume is a medical historian who applies a thoroughly sociological perspective in examining the social and cultural definition and the response to mental disorder over a long span of Western history."—*American Journal of Sociology*

Paper 352 pages $7.50 Available

Doctoring Together

A Study of Professional Social Control

Eliot Freidson

"*Doctoring Together* is an intensive qualitative study of a large prepaid medical group and how it carried out its activities with particular focus on professional self-regulation.... It is a unique book dealing with a very important issue, and Freidson is thoughtful, provocative and extremely informative in his description of how this medical clinic functioned."—David Mechanic, Rutgers University

Paper 312 pages $7.95 October

Harold D. Lasswell

On Political Sociology

Edited and with an Introduction by Dwaine Marvick

"Lasswell's disciplinary abode is simply a base point from which one of the most comprehensive intellectual edifices in the human sciences has been constructed."—*Contemporary Sociology*

Paper 462 pages $7.00 Available

Poverty and Social Change

Kirsten Gronbjerg, David Street and Gerald D. Suttles

The authors provide a historical account of how the current welfare state developed in America, and show how it reinforces old interest groups or generates new ones, making genuine welfare reform difficult to achieve.

Paper 256 pages $9.00 Available

The University of Chicago Press

Chicago 60637

ANNOUNCING

The Franklin Watts College Division

(Formerly New Viewpoints)

Feb. 1981 **Juvenile Delinquency and Juvenile Justice**

JACK KLEMPNER, Avila College, Kansas City, and RODGER D. PARKER, Univ. of Missouri, Kansas City. The authors, former probation officers, analyze the causes of increasing juvenile delinquency and the failure of the present juvenile justice system. They offer practical proposals for reforming the system and coping with the underlying social problems.

$13.95 (tent.) Hardcover 531-05419-5

Jan. 1981 **Aging in a Changing Society** *2nd Ed.*

ZENA SMITH BLAU, Univ. of Houston. Enriched by a new study of the needs of 60,000 Texans over 55, this widely acclaimed book shows that society has "as yet found no ways to use the qualities, talents, and skills of its aging citizens," and presents many new ways this can be accomplished. The first edition was "highly recommended" by *Choice* "for both undergraduate and graduate levels. A must for those involved in gerontological studies and teaching."

$7.95 (tent.) Paper 531-05637-6

1980 **The Work of Social Work**

ALLAN R. MENDELSOHN — $12.50 Hardcover 531-05412-8
$6.95 Paper 531-05626-0

1980 **Stepfamilies:** *A Cooperative Responsibility*

FREDRICK CAPALDI and BARBARA McRAE

$10.95 Hardcover 531-06373-9/$6.95 Paper 531-06750-5

1979 **Criminal Justice:** *Issues & Ironies 2nd Ed.*

ABRAHAM S. BLUMBERG — $15.00 Hardcover 531-05407-1
$9.95 Paper 531-05618-X

1979 **Cities in Transition:** *Social Changes and Institutional Responses in Urban Development*

PETER R. GLUCK and RICHARD J. MEISTER

$12.95 Hardcover 531-05409-8/$6.95 Paper 531-05623-6

1979 **Post-Affluent America:** *The Social Economy of the Future*

GARY GAPPERT — $12.95 Hardcover 531-05403-9
$6.95 Paper 531-05612-0

1978 **Police and the Community:** *An Analytic Perspective*

ROBERT S. CLARK — $12.95 Hardcover 531-05405-5
$6.95 Paper 531-05615-5

We welcome manuscripts or project ideas for basic social science courses.
Please contact Will Davison, Executive Editor. Dept. SR

College Division, Franklin Watts
730 Fifth Avenue, New York, N.Y. 10019

A GROLIER COMPANY

Give your students
a sound introduction to . . .

THE SOCIAL WELFARE INSTITITUION:
An Introduction, Third Edition

Ronald C. Federico, *University of Cincinnati*
1980, Cloth, 416 pages, Instructor's Guide

This text continues to provide beginning students with a conceptual understanding of the scope and major components of the social welfare institution and the profession. The Third Edition examines the meaning of social welfare and the characteristics of social welfare as a social institution. An analytical framework aids student understanding of the approaches to social welfare services.

CRIMINOLOGY: Perspectives on Crime and Criminality

Peter Wickman, *State University of New York, Potsdam and*
Phillip Whitten with the assistance of Robert Levey
1980, Cloth, 640 pages, Instructor's Guide

Written for the beginning student in criminology, the text covers major perspectives, issues, and the diverse concerns and debates that characterize contemporary criminology. The balanced "humanistic" view of criminology that is critical as well as current is designed to stimulate the student's systematic study of crime, criminality and criminal behavior.

DELINQUENCY: A Sociological View

Gary F. Jensen, *University of Arizona*
Dean G. Rojek, *University of Georgia*
1980, Cloth, 416 pages, Test Item File

This text presents a systematic sociological analysis of juvenile delinquency with careful attention given to treatment and prevention. It examines the origins of adolescent misbehavior; appraises society's reaction to the problem; and discusses the success of the current system of juvenile justice in dealing with the problem.

Select a text
from D. C. Heath!

Call toll free: 800-225-1388

125 Spring Street
Lexington, MA 02173

D.C. HEATH
College Division

A SOCIAL INSTITUTION . . .

New for 1981

Coming in February!

THE SOCIOLOGY OF DEVIANCE: An Introduction by Jack D. Douglas & Frances Chaput Waksler. Cloth/c. 512 pages/$16.95 tentative/with Test Bank. *Incomparably up-to-date,* this innovative new text is unique for its use of the "new perspective" (e.g., symbolic interactionism, labeling theory) as both an analytical tool and a unifying theoretical framework. Its *thorough coverage* of the key topics in deviance incorporates the very latest approaches, and it introduces a *wholly new subject:* cognitive deviance (e.g., religious cults, supernaturalism).

Coming in January!

INTRODUCTION TO CRIMINOLOGY, 2nd Edition, by Hugh D. Barlow. Cloth/c. 576 pages/$16.95 tentative/with Instructor's Manual. In its new edition, this *descriptive* introduction to theory and method in criminology, crime and criminal offenders, and criminal justice continues to stress the importance of social and historical context. Thoroughly updated, the Second Edition features two separate chapters on punishment, plus *all-new* sections on child abuse, spouse rape, victimization, self-report studies, biogenic theories, and prosecution and sentencing.

Coming in January!

CITY SCENES: Problems and Prospects, 2nd Edition, edited by J. John Palen. Paper/c. 320 pages/$9.95 tentative. Such key issues as poverty, urban regeneration and displacement, race and ethnicity, and planning for the year 2000 are the focus of this widely-admired urban sociology reader. *Expanded and updated,* the Second Edition features 12 (out of 22) new articles and two entirely new chapters: "Neighborhood and Community" and "Patterns in Developing Countries."

Coming in February!

SOCIAL PROBLEMS: The Contemporary Debates, 3rd Edition, edited by John B. Williamson, Linda J. Evans & Anne Munley. Paper/c. 512 pages/$9.95 tentative/with Test Bank. This widely-used text/reader explores 17 social problems in a debate format that directly juxtaposes proponents of differing perspectives (e.g., conservative/liberal/radical). Among the 7 *new issues* debated in this Third Edition are family violence, the death penalty, nuclear power, and the prospects for affluence in America's future.

Little, Brown Texts and Readers

Recently Published

SOCIAL PROBLEMS by Burton Wright & John P. Weiss. Cloth/556 pages/$15.95/with Study Guide and Instructor's Manual. This new introductory text examines 15 contemporary problems within a *unique integrating framework:* how societal conflicts arise, how they're perpetuated, and how they might be resolved.

FAMILY IN TRANSITION: Rethinking Marriage, Sexuality, Child Rearing, and Family Organization, 3rd Edition, edited by Arlene Skolnick & Jerome H. Skolnick. Paper/579 pages/$9.95/ with Test Bank. In its new edition, the most widely-used Marriage and Family reader features *70% new material,* including articles on love, divorce, divorce law, and remarriage.

SOCIAL GERONTOLOGY: An Introduction to the Dynamics of Aging by David L. Decker. Cloth/304 pages/$14.95/with Test Bank. This new text analyzes recent demographic shifts to explore the significance of aging as a social phenomenon and explores a range of problems facing older Americans.

THE SOCIOLOGY OF HEALTH: Principles, Professions, and Issues by Fredric D. Wolinsky. Cloth/449 pages/$15.95. A comprehensive, readable survey of health-related behavior, this new text focuses on the major concepts, careers, and debates in medical sociology.

CONSCIENCE AND CONVENIENCE: The Asylum and Its Alternatives in Progressive America by David J. Rothman. Paper/464 pages/$8.95. This sequel to the prize-winning work, THE DISCOVERY OF THE ASYLUM, traces Progressive reform efforts in the realms of criminal justice, juvenile justice, and mental health.

PRISONS IN TURMOIL by John Irwin. Paper/277 pages/$6.95 A scholar and former inmate offers a fascinating, *inside account* of how prisoners cope with life behind bars, including an analysis of prison social structures.

HOUSE OF CARDS: The Legalization and Control of Casino Gambling by Jerome H. Skolnick. Paper/382 pages/$7.95. This study of Nevada's casino industry focuses on administrative and ethical problems in legalization and licensing.

COLLEGE DIVISION
LITTLE, BROWN AND COMPANY
34 Beacon Street, Boston, Massachusetts 02106

THE BATTERED CHILD

Third Edition, Revised and Expanded

Edited by **C. Henry Kempe, M.D.**

and **Ray E. Helfer, M.D.**

Popular awareness of child abuse, neglect, sexual molestation, and emotional deprivation has increased tremendously in the past decade, due in part to the remarkable impact of the first edition of this book.

Cloth 464 pages $25.00

GREAT EXPECTATIONS

Marriage and Divorce in Post-Victorian America

Elaine Tyler May

This study attributes the dramatic rise in divorce in the years up to 1929 to our changing society and its romantic expectations.

Cloth 208 pages $15.00

DEMOCRACY, AUTHORITY, AND ALIENATION IN WORK

Workers' Participation in an American Corporation

John F. Witte

An analysis of a project in workplace democracy based on the author's experiences as a participant-observer in a large Los Angeles-based corporation. Witte's research is based on the rich data that were available to him: lengthy interviews; analyses of turnover and productivity; and minutes, plans, and voting records.

Cloth 228 pages $20.00

ACTORS AND SYSTEMS

The Politics of Collective Action

Michel Crozier and **Erhard Friedberg**

Translated by ***Arthur Goldhammer***

"This may be one of the most important contributions that has been made for twenty years in the study of what power really is." —George Suffert, *Le Point*

Cloth 272 pages $25.00

BLACK REPRESENTATION AND URBAN POLICY

Albert K. Karnig and **Susan Welch**

"This work, the first of its kind, provides valuable information about a newly and rapidly developing process of black politization."—Charles V. Hamilton, Columbia University

Cloth 200 pages $20.00

HOMO HIERARCHICUS

The Caste System and Its Implications

Complete Revised English Edition

Louis Dumont

"This book is a major contribution to the general sociological theory of Indian caste."—*South Asia Review*

Paper TBA

Also available in cloth

The University of

THE CULTURE OF PUBLIC PROBLEMS

Drinking-Driving and the Symbolic Order

Joseph R. Gusfield

Using drinking and driving as a case-example of a public problem, Gusfield emphasizes the role of language and ritual in creating a sense of order and authority in human behavior.

Cloth 272 pages $20.00

METAPHORS WE LIVE BY

George Lakoff and **Mark Johnson**

"A well-written and entertaining book and a gold mine of examples and ideas to researchers outside of linguistics. . . . A door has been opened to a new aspect of human language."
—Jaime G. Carbonell, Carnegie-Mellon University

Cloth 242 pages $13.95

AMERICAN SOCIOLOGY AND PRAGMATISM

Mead, Chicago Sociology, and Symbolic Interaction

J. David Lewis and **Richard L. Smith**

Lewis and Smith challenge much current thinking in philosophy and sociology by reconstructing the intellectual histories of both American pragmatism and sociology as they developed from 1892 to 1935 at the University of Chicago.

Cloth 376 pages $25.00

GEORGE HERBERT MEAD

Self, Language, and the World

David L. Miller

"Unquestionably the most thorough presentation of Mead's philosophical thought in the literature. . . .[It] is comprehensive, very carefully developed, and remarkably simple and clear."
— Herbert Blumer:—*American Journal of Sociology*

Paper 324 pages $7.95

THE DECLINING SIGNIFICANCE OF RACE

Blacks and Changing American Institutions

Second Edition

William Julius Wilson

Here is a new paperback edition of the book that remains a center of controversy. In an Afterword to the paperback, Wilson reflects on the debate surrounding his findings on class and race in present-day America.

"Wilson has written a profound and provocative book that is destined to become a classic in the field."—*American Journal of Sociology*

Winner of the **Sydney M. Spivack Award** of the **American Sociological Association**

Paper 248 pages $5.95
Still available in cloth

Chicago Press

Human Nature and History

A Response to Sociobiology

KENNETH BOCK. In recent years a number of biologists—E.O. Wilson and Konrad Lorenz among them—have expanded their theories to include explanations of human social behavior. A response to this sociobiological analysis of human societies, *Human Nature and History* argues that an understanding of man's social and cultural differences is best sought in the pages of history, and not in biology or comparative ethology.

"An eloquent defense of historical processes as movers and shapers of mankind.... [Bock] has carefully marshalled his material and presents a well-developed thesis.... Withal, a worthy addition."—*Kirkus Reviews*

$18.95

Programmed to Learn

An Essay on the Evolution of Culture

RONALD H. PULLIAM AND CHRISTOPHER DUNFORD. "The sociobiologist asserts that man's behavior is determined by his genetic heritage; the psychologist maintains that man's behavior is determined by cultural heritage. Pulliam and Dunford propose a synthesis, based on the combination of genetic theory and cognitive consistency theory.... The educated layreader will find the book timely, accessible, and very readable."—*Library Journal*

$10.00

To order send check or money order to **Dept. JN** at the address below, including $1.30 per order for postage and handling.

Columbia
University Press

136 South Broadway Irvington, New York 10533

sociology

Journal of the British Sociological Association

Quarterly

Editor: Professor Philip Abrams

SOCIOLOGY, the journal of the British Sociological association, is widely recognised as the leading British journal in the field, the journal which covers all that is of importance in British sociology, publishing articles which report the results of significant research together with speculative and controversial contributions to major sociological debates.

As well as major articles, there are sections for DEBATES, extended REVIEW ARTICLES, BOOK REVIEWS and SHORTER NOTICES.

Contributors to past issues have included: Basil Bernstein; Stan Cohen; Norbert Elias; Tony Giddens; John Goldthorpe; David Lane; David Lockwood; Steven Lukes; John Rex; Margaret Stacey; Peter Worsley.

SOCIOLOGY is published by the BSA in February, May, August and November

sociology

ANNUAL SUBSCRIPTION RATES (£)

	UK	EUROPE	REST OF WORLD
BSA members	7.50	16.00*	18.00*
Non-member individuals	16.00	19.00	19.00
Institutions **	18.00	21.50	21.50

Special rate to members of the American Sociological Association $30.00

* represents overseas membership fee which includes sociology subscription

** subject to review and possible increase in 1981

BACK NUMBERS

Individual copies £6.50 each, including postage
Special offer on complete back run (Vols 1-14): UK £155 Overseas £160 (inc p&p)

All enquiries and orders to:
The Business Manager, Sociology, 10 Portugal Street, London WC2A 2HU

sociology

— a journal of exceptional academic quality —

To understand all levels of society, students need the highest level of scholarship.

21 OCT 1981

Coming this Spring—
CAVAN & FERDINAND

JUVENILE DELINQUENCY *Fourth Edition*

This completely revised edition offers a complete, factual account of delinquency that emphasizes the sociological aspects and also covers much of the latest psychological research. New features include: • material on the rapidly emerging problem of female delinquency • an examination of historical evidence regarding the foundation of juvenile justice upon *parens patriae* and its meaning for modern day juvenile justice • recent material regarding the community and delinquency.

New! KENKEL

SOCIETY IN ACTION **Introduction to Sociology, *Second Edition***

Emphasizes evolutionary theory and integrates this theme into the family, sex roles, stratification, and economic institutions. Also included are new chapters on the sociology of sex roles and human groups, new boxed inserts on contemporary issues, and more cross-cultural references. Study Guide and Test Bank. Instructor's Manual.

New! COLEMAN & CRESSEY

SOCIAL PROBLEMS

Combining functionalism with the conflict and social psychological approaches, this text covers more social problems than any other text available. Twelve "debates" argue the pros and cons of vital social issues. Instructor's Manual.

New! ACUÑA

OCCUPIED AMERICA **A History of Chicanos, *Second Editon***

Still the only available history of Chicanos written by a Chicano, this comprehensive, well-documented text now includes many new primary sources and offers a closer look at the economic causes of the Chicanos' subjugation.

New! PERRY & PERRY

CONTEMPORARY SOCIETY

An Introduction to Social Science, *Third Edition*

Extensively revised, this unique text/reader features: • a new chapter on sexuality and sex roles • a greatly expanded economics section • more integrated treatment of Marxism, capitalism, and socialism • and all-new readings, marginal notes, and chapter summaries.

Harper & Row

To request examination copies, write to Suite 3D, Harper & Row, 10 East 53d St., New York, N.Y., 10022. Please include your course title, enrollment, and present text.